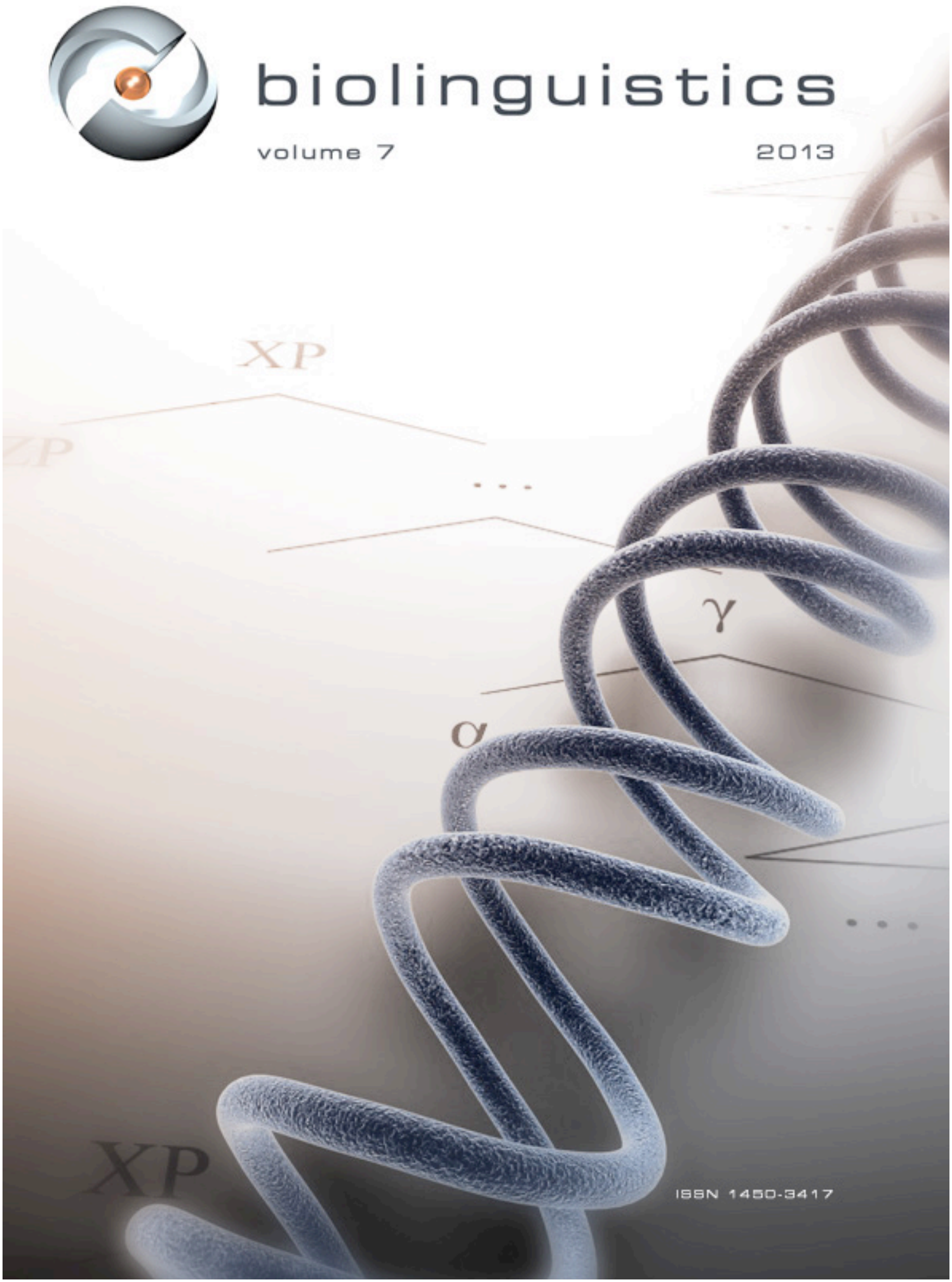

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Third Factors and the Performance Interface in Language Design

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This paper shows that systematic properties of performance systems can play an important role within the biolinguistic perspective on language by providing third-factor explanations for crucial design features of human language. In particular, it is demonstrated that the performance interface in language design contributes to the biolinguistic research program in three ways: (i) it can provide additional support for current views on UG, as shown in the context of complex center-embedding; (ii) it can revise current conceptions of UG by relegating widely assumed grammatical constraints to properties of the performance systems, as pointed out in the context of linear ordering; (iii) it can contribute to explaining heretofore unexplained data that are disallowed by the grammar, but can be explained by systematic properties of the performance systems.

Keywords: center-embedding; ellipsis; linguistic performance; third factor; word order

1. Introduction

Asking why natural languages are built ('designed') the way they are by considering how systematic properties of the performance systems, the acquisition, production and comprehension systems, connect to the representation of grammars is anything but new. One prominent attempt in the pre-minimalist era to approach this issue is Berwick & Weinberg's (1984) influential book *The Grammatical Basis of Linguistic Performance*. Since they try to provide an "explanation for the form of certain axioms of the grammatical system in terms of extragrammatical principles" (Berwick & Weinberg 1984: 143), they deal with an explanatory relationship between human sentence parsing and linguistic con-

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straints imposed by the grammar that this paper is concerned with, too. However, we want to depart from their approach in central respects, following Fodor (1985). Berwick & Weinberg's work is part of the long attempt to directly translate formal models of linguistic competence, using modern computer-science algorithms, into a performance model (cf. Miller & Chomsky 1963). Recently, this tradition has been continued by Di Sciullo (2000), Phillips (2004), Berwick (2011), and many others. Although both the theory of grammar and the idea of what counts as an explanation of language design have shifted significantly since Berwick & Weinberg's work, we want to point out in this section that Fodor's (1985) objections to Berwick & Weinberg's theory are in the spirit of current methodology in biolinguistics. To see why, let us briefly recall their approach.

Based on a set of parsing problems created by the dependencies between fillers and gaps, Berwick & Weinberg claim that Universal Grammar (UG) should include a locality constraint whose functional source is the parsing mechanism. In particular, they argue in favor of intermediate traces (i.e. Subjacency conditions) in terms of keeping the left-context of a structural configuration active at every derivational cycle. They claim that precisely such parsing problems gave rise to the evolution of this linguistic constraint, which is part of UG and hence must have evolved, according to them, by selection pressures. Note that their parser is intended to be a natural implementation of the rules and representations of the grammar. However, the grammar, at this time, was characterized by Government/Binding (GB)-theory. Due to its highly modular structure, GB-theory contains a rich UG, with several constraints and principles operating on different levels of representation. Thus, Berwick & Weinberg's move to assign a locality constraint to UG by demonstrating a corresponding constraint in the parser poses no problem for the general character of the assumed theory of grammar.

Fodor (1985) raised several objections to Berwick & Weinberg (1984). Her main points were: (i) Berwick & Weinberg assume a constraint in the parser to provide a functional explanation for the constraint in the grammar and hence do not take into account that the constraint might be motivated independently. (ii) Their constraint cannot account for all possible constructions. On evolutionary grounds, then, Fodor argues that their functional account of this aspect of language design is not convincing because "the fit that can be established between the linguistic phenomenon to be explained and the functional problem that is identified as its source […] is not close" (Fodor 1985: 20). Based on these criticisms, she asks what could count as a performance explanation of this aspect of language design and provides some useful distinctions that may help in approaching this issue.

First, she states that the weakest claim would be that a parser P can incorporate some constraint C, that is, P can obey C (Type 1). Second, according to Fodor, a stronger claim would be that P benefits from C, that is, P can not only incorporate C but it also operates more efficiently if it does incorporate C than if it does not. If other kinds of parsers could be shown not to benefit from C, then P could offer a stronger functional motivation for C than these other parsers do (Type 2 = Berwick & Weinberg). The third explanatory option is that P must incorporate C, that is, C is entailed by the defining properties of P (Type 3). Accord-

ing to Fodor, an adequate statement about language design only follows when there are reasons to believe that human sentence parsing has exactly these properties. That is, the statement must be of Type 3. In other words, "we would recognize explanatoriness to the extent that the relevant properties of P are independently motivated in some fashion" (Fodor 1985: 5). By postulating that an adequate ('deep') explanation should take into account independent motivations and by assuming that a constraint assigned to UG should not only serve as a solution that solves part of an evolutionary problem but instead serves as an 'optimal solution,' Fodor, in many respects, anticipates the biolinguistic perspective on language design.

According to Chomsky (2005: 6), three factors have to be explored when one aims at an explanation of language design:

(i) The genetic endowment (= UG)
(ii) Linguistic experience
(iii) Principles not specific to the language faculty

According to this distinction, factor (i) contains the components of the faculty of language that are both language- and species-specific; thereby it roughly corresponds to what Hauser *et al*. (2002) call the 'faculty of language — narrow sense' (FLN). Factor (ii) refers to the linguistic input, which is the source of variation within this parcellation of language design. Factor (iii) contains principles of biological and computational systems not specific to the faculty of language. According to Chomsky (2005: 6), these are "(a) principles of data analysis that might be used in language acquisition and other domains; (b) principles of structural architecture and developmental constraints […] including principles of efficient computation," and, as Chomsky (2007: 3, fn. 4) adds, "properties of the human brain that determine what cognitive systems can exist."

Given this factor distinction, Berwick & Weinberg's (1984) claim that UG includes the locality constraint is unwanted. Within the biolinguistic perspective on language design, a third-factor explanation offers a benchmark for what counts as a genuine explanation and thereby corresponds to Fodor's (1985: 30) "full-blooded Type 3 explanation." In other words, as pointed out also by Richards (2008: 134), biolinguistics is characterized by a trend away from factor (i), that is, UG must be small and simple, on evolutionary grounds. The faculty of language, according to Chomsky, arose too recently for there to have been enough time (in evolutionary terms) for the development of a rich UG containing several language-specific principles, constraints, etc. Accordingly, as Chomsky (2007) argues, the more we can ascribe to third factors and the less to UG, the more tractable the issue of language evolution becomes. Given this shift in perspective, it is reasonable to assume that UG only contains properties such as recursive Merge, binary branching structure, and the valued-unvalued feature distinction. All other universal properties might follow from the interaction between UG and principles of extralinguistic components that belong to factor (iii). These principles, by definition, do not depend on UG and are independently motivated.

Note that these principles, unlike the principles of UG in the GB-theory/Principles and Parameters (P&P)-theory, are presumably invariant be-

cause, according to Boeckx (2011: 210), "[t]here is simply no way for principles of efficient computation to be parametrized […], it strikes me as implausible to entertain the possibility that a principle like 'Shortest Move' could be active in some languages, but not in others. Put differently, […] there can be no parameters within the statements of the general principles that shape natural language syntax." Of course, that does not mean that third factors cannot contribute to explaining parameters. Consider, for instance, the head-complement parameter (cf. Holmberg 2010, Richards 2008): UG allows that X can merge with an XP, but UG does not fix their linear order, that is, X can either precede or follow the complement XP. When Merge is maximally unspecified — when it is symmetrical (cf. Chomsky's 2000a set-Merge) — it cannot specify any particular ordering. However, the physics of speech, that is, the nature of the articulatory and perceptual apparatus require one of the two logical orders, since pronouncing or perceiving the head and the complement simultaneously is impossible. Thus, the head-complement parameter, according to this approach, is a third-factor effect.

As the above reasoning concerning the head-complement parameter has shown, third-factor explanations sometimes refer to rather abstract design features. In this paper, we will show how concrete properties of the performance systems can contribute to explaining the design features of language. In particular, we will be concerned with non-trivial systematic processing phenomena and argue that they are part of an implicit knowledge of human language performance systems, and thus, they lend themselves to third-factor explanations of the design features of human language.

In section 2, we will show how performance data support current conceptions of UG. In particular, we will investigate recent claims that the grammar includes special constraints on center-embedding and ask whether their properties follow from independently established constraints on sentence processing. In section 3, we will discuss how properties of the performance systems can revise current approaches to UG. Specifically, we will discuss a third-factor explanation of a constraint on linear ordering that is widely assumed to be part of UG. After having shown that the assumption of independently motivated performance systems is methodologically beneficial, given the biolinguistic framework to reduce UG, in section 4 we will present empirical evidence that such systems are needed anyway in order to account for data in the context of acceptable ungrammaticality. Section 5 summarizes the main results and concludes.

2. The Performance Interface and Complex Center-Embedding

Although Miller & Chomsky (1963), as mentioned above, argued in favor of a general transparency between (theories of) grammar and (theories of) linguistic performance, they also provide arguments for keeping grammar and performance strictly separate. In other words, they claimed that some design features of human language, like recursive embedding, cannot be explained by any of the three types of explanations pointed out by Fodor (1985) because these design features do not show any relationship between a grammatical constraint and properties of the performance systems.

One prominent argument, initially developed by Chomsky & Miller (1963), in favor of drawing a sharp distinction between processes on the level of performance and formal mechanisms on the level of grammar rests on the property of recursive self-embedding and the observation that multiple center-embedding leads to structures that can no longer be produced or comprehended under normal on-line conditions, as illustrated by (1):

(1) The rat the cat the dog chased killed ate the malt.
(Chomsky & Miller 1963: 286)

The fact that such sentences are quite incomprehensible has no bearing on the possibility of generating them on the level of grammar because, as Chomsky (1963: 327) points out by means of an analogy, "the inability of a person to multiply 18,674 times 26,521 in his head is no indication that he has failed to grasp the rules of multiplication." The overall conclusion, then, is that such structures are excluded by performance factors that limit the realization of our grammatical competence. In particular, Miller & Chomsky (1963) showed that, given certain reasonable assumptions about language processing, this construction, unlike other relative clause configurations, creates a major strain on working memory. They therefore concluded that it is a performance violation, not a competence violation.

This conclusion was disputed by Reich (1969), who claimed that a sentence such as (2) is not just unacceptable — that is, beyond the processing capabilities of the human sentence processor — but downright ungrammatical, where the term 'ungrammatical' is understood in the classical way of meaning 'not within the set of sentences derivable by the mental competence grammar.'

(2) The rat that the cat that the dog worried killed ate the malt.
(Reich 1969: 831)

The dispute about sentences as in (1) and (2) points to a deeper problem. Even if we know that a certain sentence is ungrammatical, we cannot know *a priori* what to blame for the unacceptability: The performance mechanisms, which do not have the capacity required for processing the sentence, or the competence grammar, which does not generate the sentence? Chomsky & Miller (1963) opted for the first alternative and attributed the unacceptability of sentences with double center-embedding to limitations on working memory. Reich (1969) took the opposite way. He proposed a finite-state grammar capable of generating sentences with degree-1 center-embedding but not center-embeddings of degree 2 or higher (for related ideas in a connectionist setting, cf. Christiansen & Chater 1999).

Data from language processing — either data from psycholinguistic experiments or corpus data — are no different in this regard. They cannot show whether a sentence is unacceptable due to performance limitations or because it is outside the scope of the grammar. Such data can nevertheless be quite helpful in cases where the source of unacceptability is under dispute. In particular, performance data can provide evidence on whether the limited use made of certain syntactic structures can plausibly be attributed to performance factors, or

whether grammatical constraints are necessary for this purpose.

With regard to multiple center-embedding, Roeck *et al*. (1982) argued that corpus data provide clear evidence against Reich's (1969) claim that the competence grammar cannot generate more than one level of center-embedding. They presented several corpus examples of doubly center-embedded clauses and thus showed that such sentences are produced from time to time in actual language use. Recently, such empirical approaches to multiple center-embedding have regained attention in the context of Hauser *et al*.'s (2002) claim that recursive syntactic embedding is the only human- and language-specific component of the human language faculty. In what follows, we will show, based on our own empirical data, that such approaches do not provide evidence against recent biolinguistic claims that infinite recursive nesting is a central part of UG (cf. Sauerland & Trotzke 2011 for a recent collection of papers).

In a recent volume on recursion, Karlsson (2010: 55) claims that "[m]ultiple nesting cannot […] reasonably be considered a central design feature of language, as claimed by Hauser *et al*. (2002)." His claim is based on a corpus study of multiple center-embedded clauses, where he analyzed 132 doubly center-embedded clauses from seven European languages (cf. Karlsson 2007). Given these data, he proposed specific grammatical constraints on multiple center-embedding and claimed that they reveal that "more aspects of competence (i.e. grammar) are involved in multiple center-embedding than Chomsky and his followers have been assuming" (Karlsson 2007: 385). Thus, by formulating grammatical constraints, Karlsson objects to the view that any constraint on center-embedding must solely follow from the performance systems. Like Berwick & Weinberg (1984), he assumes that properties of the performance systems provide a functional explanation for the constraints in the grammar, since he claims that "the constraints are epiphenomenal consequences of more basic cognitive properties, especially short-term memory limitations" (Karlsson 2007: 385). Thus, according to Fodor's (1985) typology, he offers a 'type 2 explanation' and does not take into account that the constraints might be motivated independently and do not exist in the grammar ('type 3 explanation').

In this section, we will argue that the properties of these grammatical constraints follow from independently motivated constraints on sentence processing and that they are therefore superfluous. To show that we are dealing with systematic properties of the performance systems, we will present data from both production and comprehension, our hypothesis being that it is precisely the collusion of speakers and hearers that yields such systematic properties. In particular, we will discuss corpus data and results from associated acceptability experiments that have investigated doubly center-embedded relative clauses in German. The major question addressed by the corpus data is whether doubly center-embedded relative clauses have special properties that call for specific grammatical constraints on multiple center-embedding (e.g., Karlsson 2007), or whether their properties follow from independently established constraints on sentence processing (e.g., Gibson 2000).

In order to address these questions, Bader (2012) analyzed the deWaC corpus (cf. Baroni *et al*. 2009) for the occurrence of multiply center-embedded relative clauses (RCs) in German. This study goes beyond Karlsson (2007) not only

by looking at a larger number of examples but also by taking into account structural variants involving extraposition. This makes it possible for the first time to determine empirically whether multiply center-embedded RCs have unique properties requiring specific grammatical constraints.

Four sentence structures were investigated. Sentence (3) is an original corpus example with a doubly center-embedded RC (RC-low within RC-high, intraposed relative clauses).

(3) *German*
RC-low within RC-high, intraposed relative clauses
Internationale Studien belegen, dass Medizinstudenten, denen
International studies prove that medical-students to-whom
identische Krankenakten, die nur in Bezug auf Alter und Geschlecht
identical patient's files that only in relation to age and gender
variieren, vorgelegt werden, unterschiedlich entscheiden.
vary presented are unequally decide
'International studies show that medical students decide unequally if they are confronted with patient's files that only differ with respect to age and gender.'

A search of the deWaC corpus with its 1,278,177,539 tokens of text revealed 351 instances of doubly center-embedded RCs as in (3). In accordance with Karlsson (2007), sentences with more deeply embedded RCs were practically absent. Thus, doubly center-embedded RCs do occur, but they are rare.

However, doubly center-embedded RCs are not only special by involving two degrees of clausal center-embedding, they are also special on several other measures. For example, a doubly center-embedded RC disrupts the dependency between its head noun (*Medizinstudenten* 'students of medicine' in (3)) and the corresponding clause-final verb (*entscheiden* 'decide') much more severely than a simple RC not containing a second RC. Since the disruption of dependencies is a major source of sentence complexity — as captured by the notion of structural integration cost in the *Dependency Locality Theory* (DLT) of Gibson (2000) — the rareness of doubly center-embedded RCs cannot be attributed to the degree of center-embedding as such without further justification. In order to determine whether doubly center-embedded RCs have special properties due to their high degree of center-embedding, it is crucial to compare them to other RC structures that are matched as closely as possible but at the same time involve no center-embedding or only a single degree of center-embedding.

Such a comparison was made possible in Bader (2012) by analyzing three further types of complex RCs which differ from doubly center-embedded RCs only with regard to the position of the RCs. This was achieved by applying extraposition to RC-high, RC-low, or both. Schematic tree structures for the four sentence types that were thus investigated in the corpus study are given in Figure 1. Original corpus examples are shown in (3) above and (4)–(6) below.

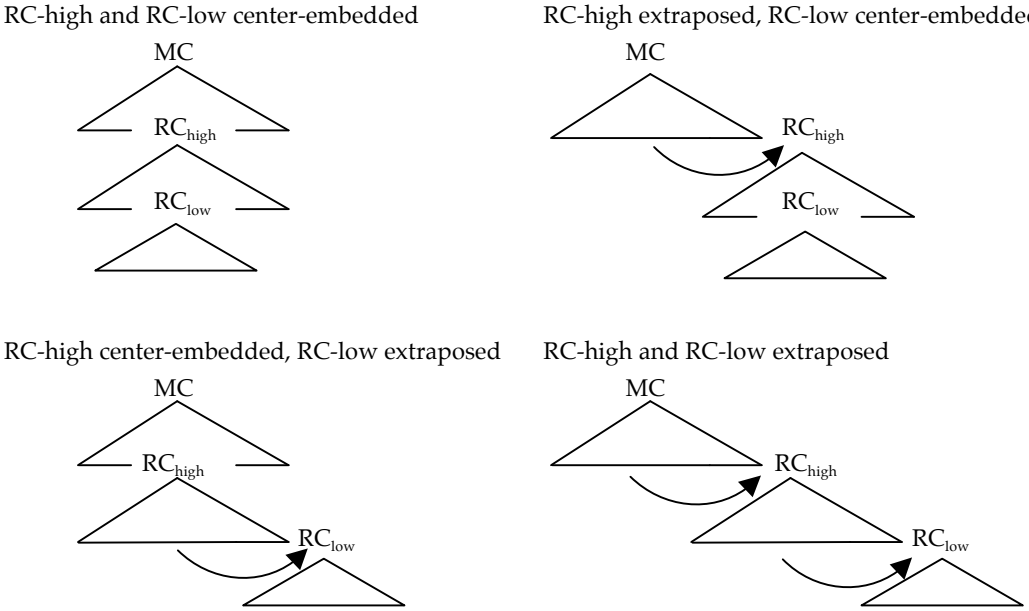

Figure 1: Schematic tree structures for the four sentence types investigated in the corpus study (MC = matrix clause; RC = relative clause)

(4) *German*

RC-low behind RC-high, intraposed relative clauses

Ihr werdet bemerkt haben, dass Völker, die in Ländern leben, in
You will realized have that peoples who in countries live in
denen ein besseres Verständnis von Leben und Tod herrscht, den
which a better understanding of life and death governs the
Weggang eines geliebten Menschen oftmals zelebrieren.
departure of-one loved person often celebrate.

'You will have realized that peoples who live in countries where there exists a better understanding of life and death often celebrate the passing away of a beloved person.'

(5) *German*

RC-low within RC-high, extraposed relative clauses

Hector Sanchez ist davon überzeugt, daß der Geist von Tom Donovan
Hector Sanchez is by-that convinced that the ghost of Tom Donovan
zurückgekehrt ist, der vor zehn Jahren während einer Explosion, die
returned is who before ten years during an explosion which
Annie, Dan und er versehentlich ausgelöst hatten, ums Leben kam.
Annie Dan and he accidentally caused had over life came

'Hector Sanchez is convinced that the ghost of Tom Donovan has returned, who was killed in an explosion that was accidentally caused by Annie, Dan and himself, has returned.'

(6) *German*

RC-low behind RC-high, extraposed relative clauses

Grundlegend ist hier die Annahme, dass es keine allgemeingültige
Essentially is here the assumption that it no universal
Definition gibt, die die Lieder beschreibt, die sich für den Einsatz
definition gives that the songs describes that itself for the use
im Unterricht eignen.
in class suit.

'Basically here is the assumption that there is no generally accepted definition describing the songs that may suit for the usage in class.'

In (4), RC-low has been extraposed behind RC-high, but the two relative clauses are still center-embedded within the matrix clause (RC-low behind RC-high, intraposed relative clauses). In (5), RC-low is again center-embedded within RC-high, but the relative clauses have been extraposed behind the matrix clause (RC-low within RC-high, extraposed relative clauses). In (6), RC-low has been extraposed behind RC-high and the relative clauses as a whole have been extraposed (RC-low behind RC-high, extraposed relative clauses).

The existence of doubly center-embedded RCs raises two major questions. First, why do doubly center-embedded RCs occur so rarely, or, put more generally, what factors affect the frequency with which they are produced? Second, why do doubly center-embedded RCs occur at all, that is, why are they not avoided completely by means of extraposition? If it is not the degree of center-embedding as such, but the processing cost induced by clausal embedding in general, then these two questions should find answers that are not specifically tailored to the case of double center-embedding. Instead, the answers should be general enough to also cover the RC structures in (4)–(6).

We begin with the first question: Why do doubly center-embedded RCs occur so rarely? If performance constraints are responsible for this, and not grammatical constraints on multiple center-embedding, then sentences with intraposed complex relative clauses should be rare in general because they introduce a lengthy dependency between the antecedent NP of RC-high and the clause-final verb (e.g., Gibson 2000). This should be true whether RC-low occurs within RC-high (degree of center-embedding = 2) or behind RC-high (degree of center-embedding = 1). In accordance with this prediction, the corpus study revealed that doubly center-embedded relative clauses as well as intraposed relative clauses with RC-low behind RC-high ((3) and (4)) are rare in comparison to similar sentences with the relative clauses extraposed ((5) and (6)).

As far as the particular constraints proposed in Karlsson (2007) were found to hold, it turned out that they reflect more general properties of complex RCs, properties that are not specific to doubly center-embedded RCs. As a case at hand, consider the NO-MULTIPLE-OBJECT-RELATIVIZATION constraint which is given in (7) (from Karlsson 2007: 383):

(7) **O–O constraint*

Direct objects must not be multiply relativized in C^2s.

Among the doubly center-embedded RCs analyzed in Bader (2012), there were only approximately 4% in which both the relative pronoun of the higher RC and the relative pronoun of the lower RC were objects. The *O–O constraint thus seems to hold, not as an absolute constraint but as a very strong preference.

However, a closer analysis revealed that the *O–O constraint is just a descriptive generalization that applies not only to doubly center-embedded RCs but to the other types of complex RCs as well. For sentences as in (4)–(6), the rate of O–O RCs was also about 4% or even less. Furthermore, the rareness of complex RCs in which both relative pronouns are objects could be shown to follow from the rareness of object relativization in general. This is shown in Table 1 for the case of doubly center-embedded RCs.

	p(rel-pro/high) *p(rel-pro/low)	Predicted proportion	Observed proportion	Predicted frequency	Observed frequency
Subject-Subject	0.835*0.785	0.66	0.66	155.4	157
Subject-Object	0.835*0.215	0.18	0.17	42.6	41
Object-Subject	0.165*0.785	0.13	0.12	30.6	29
Object-Object	0.165*0.215	0.04	0.04	8.4	10

Table 1

The row labeled "Subject-Subject" shows the relevant data for RCs in which both the relative pronoun of the higher RC and the relative pronoun of the lower RC are subjects. Overall, the probability (proportion) of subjects in higher RCs was 0.835 and the probability of subjects in lower RCs was 0.785. If these probabilities were independent of each other, then the joint probability of both relative pronouns being a subject is the product of the two individual probabilities. This joint probability is shown in the column labeled "predicted proportion." As a comparison with the observed proportions in the next columns shows, the predicted proportions and the observed proportions are quite close together, as also shown by the predicted and observed frequencies in the last two columns. For the remaining three combinations of subject and object relative pronoun, the same considerations apply.

What these considerations show is that there is no need to invoke a constraint like the *O–O constraint in order to account for the low proportion of object-object RCs. Instead, this low proportion follows from the low overall proportions of object RCs. Furthermore, since a low proportion of object-object RCs was not only observed for doubly center-embedded RCs, and calculations similar to those shown in Table 1 lead to the same results for the other RC types, we can conclude that we are dealing with a more general phenomenon here which is not related to the degree of center-embedding.

We turn now to the second question: Why are doubly center-embedded RCs not avoided completely by means of extraposition? The null hypothesis is that the decision of whether or not to extrapose a doubly center-embedded relative clause should be governed by the same factors that are also at work for relative clauses with a single degree of center-embedding. This hypothesis was also

confirmed. In accordance with prior findings for simple RCs (e.g., Hawkins 1994), the main determinant of extraposition is the amount of material to be crossed by extraposition. If extraposition is only across the clause-final verb, it is almost obligatory. If extraposition is across some non-verbal material in addition to the verb, it becomes highly unlikely. Importantly, this is true both when RC-low occurs within RC-high and when RC-low occurs behind RC-high, as confirmed by a logistic regression model.

Overall, the present corpus results argue that constraints on multiple center-embedding follow from processing limitations and that accordingly grammatical constraints on multiple center-embedding are not needed. In order to corroborate this claim, Bader (2012) presents an experiment that required participants to judge the grammaticality of all four major structures investigated in the current corpus study (see (3)–(6)).[1] The experiment used speeded grammaticality judgments, a method which has been used before both within psycholinguistics (e.g., Warner & Glass 1987) and within experimental syntax (Bader & Schmid 2009). In experiments using this procedure, participants have to quickly judge sentences as either grammatical or ungrammatical. Sentences are presented word-by-word on a computer screen with a presentation rate that leaves enough time for fully comprehending sentences but no time for deliberate reasoning. In the present context, this method is particularly appropriate because, as amply documented in Karlsson (2007: 379–380), the linguistic literature abounds with claims to the effect that sentences containing multiply center-embedded relative clauses are ungrammatical. As already pointed out by Karlsson, the finding of such sentences in authentic texts provides strong reasons to reject these claims. However, given their absence from spoken language and their rareness in written language, it cannot be excluded that such sentences are grammatically degraded, even if they are not outright ungrammatical.

The results of Bader's experiment can be summarized as follows: (i) Sentences with extraposed relative clauses ((5) and (6)) were judged as grammatical most of the time, with no significant differences depending on whether RC-low was center-embedded within RC-high or extraposed behind RC-high. (ii) In comparison to sentences with extraposed relative clauses ((5) and (6)), sentences with center-embedded relative clauses ((3) and (4)) received lower percentages of grammatical judgments, whether RC-low occurred within or behind RC-high. This effect was highly significant, but its magnitude was quite moderate, amounting to a difference of about 9%. Thus, even sentences with doubly center-embedded relative clauses were judged as grammatical in almost three quarters of all cases.

In this section, we have shown how the performance interface in language design provides support for Chomsky & Miller's (1963) claim that there are no specific grammatical constraints on multiple center-embedding. In particular, by presenting data from both language production and comprehension, we have demonstrated that there are systematic properties of the performance systems that constrain multiple center-embedding. Accordingly, following Chomsky's

[1] In addition to complete sentences, the experiment also included sentences in which the verb cluster of RC-high was missing. This issue of missing-VPs will be discussed in section 4.1.

(2005) three-factor parcellation, constraints on multiple center-embedding can be accounted for by a third-factor explanation and do not require us to complicate our theory of grammar (UG). Thus, in contrast to Karlsson's (2007, 2010) 'type 2 explanation,' we provided a 'type 3 explanation,' which refers to the explanatory power of the independently motivated systematicity of the performance systems.[2]

After having shown that performance studies can serve to support common views of UG, let us now look at one linguistic constraint that is widely assumed to be part of UG, but that can possibly be relegated to third-factor principles by taking into account the performance interface.

3. The Performance Interface and Linear Ordering: FOFC

In this section, we will look at a linguistic constraint that is widely assumed to be part of UG. Recently, however, some scholars have attempted to relegate this constraint to third-factor principles by referring to the performance systems, that is, to efficient processing.

Based on the fact that the word order V–O–Aux does not exist either synchronically or diachronically in Germanic, while all other orders are attested, Holmberg (2000: 124) formulated the following generalization that predicts that head-final phrases can occur embedded in head-initial phrases, but head-initial phrases cannot occur embedded in head-final phrases:

(8) *The Final-Over-Final Constraint (FOFC)*

If α is a head-initial phrase and β is a phrase immediately dominating α, then β must be head-initial. If α is a head-final phrase, and β is a phrase immediately dominating α, then β can be head-initial or head-final.

Thus, the FOFC states that head-finality must be lower in the structure than head-initiality. The generalization can be formally stated as follows, where αP is the complement of β and ΥP is the complement of α:

(9) a. $[_{\beta P}\ \beta\ [_{\alpha P}\ \alpha\ \Upsilon P]\ ]$ harmonic order
b. $[_{\beta P}\ [_{\alpha P}\ \Upsilon P\ \alpha]\ \beta\ ]$ harmonic order
c. $[_{\beta P}\ \beta\ [_{\alpha P}\ \Upsilon P\ \alpha]\ ]$ disharmonic order
d. $*[_{\beta P}\ [_{\alpha P}\ \alpha\ \Upsilon P\ ]\ \beta\ ]$ disharmonic order & violating FOFC

We will abstract away from exceptions that are discussed in the literature and that have yielded refinements of FOFC (cf. Biberauer *et al*. 2007 *et seq.*). Because

[2] In addition to constraints on sentence processing pointed out in this section, other factors, such as alignment constraints between syntax and prosody (cf. Fodor & Nickels 2011), might also play a crucial role in explaining the limitations of multiple center-embedding. However, it is beyond the scope of the present paper to decide if such constraints could be motivated on performance theoretic grounds ('type 3 explanation'), as suggested by an anonymous reviewer, or if these alignment constraints are an integral part of the grammar that entails advantages for parsing the structures ('type 2 explanation').

this generalization is widely assumed to hold (though see Hawkins to appear), we assume that there is something universal to it. Moreover, it has been claimed that there are cases where FOFC holds without exception. The most prominent case is the fact about VO-languages that they do not permit sentence-final complementizers (cf., e.g., Hawkins 1990). Referring to our formal statements above, this fact can be formulated as follows:

(10)	a.	V–O & Comp-TP	harmonic order	(= 9a)
	b.	O–V & TP-Comp	harmonic order	(= 9b)
	c.	O–V & Comp-TP	disharmonic order	(= 9c)
	d.	*V–O & TP-Comp	disharmonic order & violating FOFC	(= 9d)

Having introduced a constraint that is assumed to be part of UG, let us now turn to an alternative explanation in terms of properties of the performance systems.

Recently, Walkden (2009), Biberauer *et al*. (2010), and Sheehan (2010, to appear) have pointed out that Hawkins' (1994, 2004) Performance-Grammar Correspondence Hypothesis provides a potential processing account of FOFC. Hawkins' theory of early immediate constituents provides an alternative to the formal accounts that claim that FOFC is part of UG (for a more detailed description of what follows, cf. Sheehan to appear: 13–19).[3] In particular, the following performance-based efficiency principle correctly predicts that both (9a) and (9b) are strongly preferred (cf. Hawkins 1994: 58–59; 77):

(11) a. *Early Immediate Constituents (EIC)*

The human parser prefers linear orders that maximize the IC-to-non-IC ratios of constituent recognition domains.

b. *Constituent Recognition Domain (CRD)*

The CRD for a phrasal mother node M consists of the set of terminal and non-terminal nodes that must be parsed in order to recognize M and all ICs of M, proceeding from the terminal node in the parse string that constructs the first IC on the left, to the terminal node that constructs the last IC on the right, and including all intervening terminal nodes and the non-terminal nodes that they construct.

The EIC is a local complexity metric that predicts linear orderings. Given (11a) and (11b), the IC-to-word [= non-IC] ratio can be calculated as in (12).

[3] Of course, in performance-oriented linguistics, Hawkins' locality theory is controversial and it faces the same fundamental issues as, for instance, Gibson's (2000) theory sketched in section 2 (cf. Konieczny 2000 for a prominent critique). However, even alternative theories operating with constrained activation rather than with locality-driven complexity metrics point out, with reference to locality theories like Hawkins', that "it is clear that locality plays a critical role in sentence comprehension" (Vasishth & Lewis 2006: 788). Given the programmatic nature of our paper, we will not be concerned with (nor will we subscribe to) all the details of Hawkins' theory. However, we assume that Hawkins' approach can be regarded as an influential theory of the performance systems that should be taken into account when dealing with third-factor explanations.

(12) IC-to-word ratio = $\frac{\text{Number of ICs in domain}}{\text{Number of words in domain}}$

Applying this metric to our cases in (9), harmonic ordering such as (9a) and (9b) is preferred (cf. Hawkins 1994: 96–97, Sheehan to appear: 16):

(13) a. $[_{VP}$ V $[_{PP}$ P NP]]IC-to-word ratio = 1/1, 2/2, => average: 100% (9a)
b. $[_{VP}$ $[_{PP}$ NP P] V]IC-to-word ratio = 1/1, 2/2, => average: 100% (9b)

As pointed out by Hawkins (1994: 96), both (13a) and (13b) have optimal IC-to-word-ratio ratios of 1/1 and 2/2 (average: 100%). More specifically, in (13a), V constructs the first IC (V), resulting in a ratio of 1/1. Since P occurs immediately to the right of V and constructs the second IC (PP), the adjacent constituents V and P both construct two ICs (V and PP), thus resulting in the second IC-to-word-ratio of 2/2. In sum, the number of words is equal to the number of ICs built at each structural level. The same holds for (13b), except that in these cases of head-final languages, a bottom-up parse takes place (for elaboration on this, cf. also Sheehan to appear: 16). Let us now look at the disharmonic constructions given in (9c) and (9d).

Hawkins (1994) discusses the following disharmonic structures, where NP complements of P are within the CRD of VP. Concerning these 'non-optimal CRDs,' Hawkins (1994: 82) calculates the IC-to-word ratios from left to right in order to make the appropriate discriminations among these configurations. He gives the following (Left-to-Right) IC-to-word ratios (cf. Hawkins 1994: 255, Sheehan to appear: 17):

(14) a. $[_{VP}$ V $[_{PP}$ $[_{NP}$ Det N] P]] IC-to-word ratio = 1/1, 2/4, => average: 75% (9c)
b. $[_{VP}$ $[_{PP}$ P $[_{NP}$ Det N] V]] IC-to-word ratio = 1/3, 2/4, => average: 42% (9d)

In (14a), as in (13a), the first word V constructs the first IC (V), resulting in an IC-to-word ratio of 1/1. The IC-to-word ratio of the second IC (PP), however, is 2/4, since PP is constructed by the fourth word (i.e. P) in the CRD. Taken together, (14a) has an average ratio of 75%. In contrast to (14a), which corresponds to the configuration (9c), (14b) corresponds to the FOFC-violating ordering (9d). Since the three words (P, Det, N) dominated by PP (first IC) fall within the CRD, they are counted both in construction of PP (ratio = 1/3) and, together with the word V, in construction of the two ICs VP and PP (ratio = 2/4). Taken together, (14b) has an average ratio of 42% (for more elaboration, cf., again, Sheehan to appear: 17).

Accordingly, the EIC correctly predicts that the FOFC-violating order (14b = 9d) is more difficult to process and thus dispreferred. More recently Hawkins (2004, to appear) reformulates this left-to-right calculation procedure in terms of a separate principle of Maximize On-line Processing (which penalizes (14b) for the processing delay in the long first IC, the PP, compared to the short V in (14a)) and so defines a similar discrimination between (14a) and (14b). EIC is mean-

while converted to a more general principle stating that all structural domains that need to be accessed in the processing of grammatical relations of combination and dependency are preferably minimal, i.e. Minimize Domains. These two principles predict that the harmonic orders, (9a) and (9b) above, will be much preferred relative to the two disharmonic ones, (9c) and (9d), with (9c), e.g., (14a), having some on-line processing advantages compared with (9d), e.g., (14b). Hawkins argues that the relative quantities of language-particular grammars that exemplify the different ordering possibilities for e.g. the head-complement parameter can be predicted from these processing efficiency calculations. In sum, Hawkins' processing theory can account for FOFC and explain the facts without referring to UG, and instead his theory predicts the distribution of language-particular grammars, including FOFC, from systematic properties of the performance systems. Note that EIC and the more general Minimize Domains "is a comprehension-oriented principle of production, with numerous […] correspondences between ease of comprehension and ease of production" (Hawkins 1994: 427). Thus, it can be viewed as a systematic property of the performance systems that provides, in Fodor's terms, a 'full-blooded Type 3 explanation,' which does not resort to UG and provides independent, 'third-factor' motivations for FOFC.

However, while this explanatory power of efficient processing is acknowledged, Walkden (2009: 69–71) goes on to recast the metrics of Hawkins (1994, 2004) in order to fall back to an "UG-based FOFC." Accordingly, by reformulating the metrics of Hawkins in order to make no reference to processing, Walkden (2009) proposes a 'type 2 explanation,' insofar as he assumes that the parser benefits from a UG-constraint, that is, the parser operates more efficiently if it incorporates the constraint than if it does not.

In the following, we want to depart from Walkden's (2009) (and other's) reasoning by taking issue with their objections to an explanatory account based on processing. We want to argue in favor of a third-factor explanation that refers to systematic properties of the performance systems that are supported by experimental processing data and by corpus studies. It appears that the reason why Walkden reformulated Hawkins' account is in order to avoid any reference to processing that is not well grounded.

The first problem pointed out by Walkden (2009: 68, cf. also references cited there) is that cases like (15) exist, where O-V and D-NP are no less common than V-O and NP-D, and so, there is no evidence that FOFC holds for DP complements of V.

(15) *German*

Johann	hat	[$_{VP}$ [$_{DP}$ den	Mann]	gesehen]
John	*has*	*the*	*man*	*seen*

'John has seen the man.'

According to the classical formulation of FOFC (cf. (8)) and according to the EIC, Walkden argues that the ordering O-V and D-NP are not predicted. The non-existence of FOFC effects between DP and V is regarded a problematic case for an account based on Hawkins' processing principles, since formal UG-approaches can now deal with these exceptions (cf. Biberauer *et al*. 2007 *et seq.*), while the

processing theory of Hawkins cannot. However, as Hawkins (to appear) points out, there are also exceptions in typological samples such as Dryer (1992) to the current UG approaches to FOFC:

(16) V–O & VP–T disharmonic order & violating FOFC (= 9d)

Accordingly, further refining constraints have to be added to UG anyway, which is methodologically undesirable, given that UG should be reduced to a minimum in biolinguistics. Moreover, why should exceptions such as (15) pose a serious problem for a processing account at all? To our mind, it is precisely the strength of theories referring to processing preferences that they define a preference scale and a frequency ranking and predict, in contrast to UG-accounts, that violations like (15) can occur, since they only state that they are much less frequent and certainly less frequent than the harmonic orders (9a) and (9b) and less than the inverse-FOFC order (9c).

The second problem Walkden (2009) mentions is that the more absolute cases such as *V-O & S-TP (see (10d) above) seem to point in the direction of a UG-explanation, because Hawkins' principles cannot make any claim about absolute non-occurrence. He argues that "[f]or such cases a prohibition within UG […] is more satisfactory" (Walkden 2009: 69). Again, we don't see the plausibility of this argument. First, even in the 'absolute' cases, there seem to exist exceptions that force scholars to qualify their statements. For instance, Zwart (2009) argues that in the 214 languages he has taken into account, he finds no 'true' final coordinating conjunctions in head-initial languages. Of course, he has to introduce a definition and then (a restriction) of what counts as 'true'. Furthermore, as Biberauer *et al*. (2007) point out themselves, there do seem to be some — if only very few — exceptions. Accordingly, the FOFC seems to reflect a tendency anyway and does not lend itself to being an 'absolute' statement.

Based on the above objections, our suggestion is that it is more in the biolinguistic spirit to assume that FOFC, as a distinct constraint, is simply not located in the grammar anyway. Instead of assigning additional refinements to the grammar, we concur with Hawkins (to appear: 17) that "stipulations of formal models can become less stipulative by shifting their ultimate motivation away from an innate UG towards (ultimately innate and neurally predetermined) processing mechanisms."

To sum up, after having shown, in section 2, that systematic properties of the performance systems can provide additional evidence for common views of UG, we have discussed the possibility that a linguistic constraint that is widely assumed to be part of UG — FOFC — can possibly be relegated to independently motivated principles of efficient processing. In contrast to the 'type 2 explanation' proposed by Walkden, Biberauer, and colleagues, implying that the parser benefits from a UG-constraint, we have argued in favor of a 'type 3 explanation,' which relegates some language universals to the independently motivated systematicity of the performance systems. To our mind, this is in the spirit of Chomsky's three-factor parcellation, which aims at reducing UG to a minimum.

Up to this point, we have been arguing that there are constraints and strategies that are not part of UG, but show systematic properties and determine,

in interaction with UG, both how we understand and how we produce sentences. Our arguments were mainly based on methodological grounds, however. In particular, we argued that, according to both Fodor (1985) and Chomsky (2005), it is reasonable to reduce UG to a minimum when aiming at a 'deep' explanation of language design. In the next section, we will present empirical evidence that implicit knowledge of the human language performance systems is systematic and is needed anyway in the context of acceptable ungrammaticality.

4. The Performance Interface and Acceptable Ungrammaticality

The claim that an adequate theory of language design needs to take into account a systematic level of performance principles that is not transparent to the grammar goes back to Fodor *et al*. (1974), who presented "a body of phenomena which are systematic but not explicable within the constructs manipulated by formal linguistics" (Fodor *et al*. 1974: 369).

Recently, however, there has been a tendency in generative linguistics to return to the axioms of the derivational theory of complexity. To our mind, the clearest statement in this direction is formulated by Phillips (2004), who tries to show that the crucial arguments against the derivational theory of complexity are not as compelling as one might think (for similar discussion of what follows, cf. Marantz 2005, Boeckx 2009: 133-146). Let us briefly illustrate this reasoning and then argue that it cannot account for the findings to be presented in this section.

One prominent argument, discussed by Phillips (2004: 23–26), for the separation of grammar and parser has been that the systems for comprehension and production are operating in time and are thus prone to errors, while the grammar is defined to be precise. The famous garden path sentences are prominent cases posing a comprehension breakdown (cf. Bever 1970):

(17) The horse raced past the barn fell.

As is well known, the sentence yields an improper parse because of the tendency to interpret *The horse raced past the barn* as a complete clause, not as an NP containing a modifying clause. However, Phillips (2004) argues that, in these cases, hearers do not construct hypotheses that violate grammatical rules or principles, since the grammar clearly allows building structures such as *The horse raced past the barn*. In other words:

> Garden path sentences arise in circumstances of structural ambiguity, where two or more possible grammatical analyses are available. If the parser makes the wrong choice and subsequently breaks down when it becomes clear that the choice was the wrong one, this reflects lack of telepathy, not lack of grammatical precision. (Phillips 2004: 263)

According to Phillips (2004: 264-265), a more serious issue for the claim that hearers do not construct hypotheses that go against the grammar are sentences investigated by Christianson *et al*. (2001) and Ferreira *et al*. (2001):

(18) While the man hunted the deer ran into the woods.

Speakers go down the garden path here, since they misinterpret *the deer* as the object of *hunted*. What is crucial here, however, is that even after realizing this wrong interpretation, Ferreira and colleagues report that participants continue to believe that the man hunted the deer. Accordingly, they seem to interpret *the deer* as both the object NP of the embedded clause and the subject NP of the main clause. Since this is, of course, a grammatically impossible representation, Phillips (2004: 264) points out that "[i]f true, these findings present a serious challenge to the widespread assumption that the parser constructs only grammatically sanctioned representations."

In the following, we will present empirical evidence showing that such discrepancies between performance systems and grammar are more widespread than often assumed. We will argue that this evidence supports our general claim that we have to assume systematic performance systems that are independent from the grammar and that could, therefore, lend themselves to third-factor explanations, in the sense of Chomsky (2005).

4.1. Missing-VP Effect

Multiple center-embedding normally leads to processing breakdown when the degree of center-embedding exceeds a rather small limit, and sentences containing multiply center-embedded clauses therefore tend to be judged as ungrammatical despite being derivable by the mental grammar. Surprisingly, however, multiple center-embedding can also have the reverse effect. As first discussed in Frazier (1985) (based on an observation attributed to Janet Fodor), a sentence as in (19) seems to be grammatical at first sight.

(19) The patient the nurse the clinic had hired ____ met Jack.

In fact, however, sentence (19) is ungrammatical because it does not contain a VP for the NP *the nurse*. As also pointed out by Frazier (1985), this grammatical illusion only arises if the middle VP (the VP of the higher relative clause in sentences with a doubly center-embedded relative clause) is missing. If either the VP of the superordinate clause or the VP of the lower relative clause is omitted, the ungrammaticality is detected easily.

The missing-VP effect was later confirmed experimentally. In the first experimental investigation of this effect, Gibson & Thomas (1999) had participants rate the complexity of sentences like those in (20) on a scale ranging from 1 ("easy to understand") to 5 ("hard to understand").

(20) a. *All three VPs present* (mean rating = 2.90)

The ancient manuscript that the graduate student who the new card catalog had confused a great deal was studying in the library was missing a page.

b. *VP of the higher RC missing* (mean rating = 2.97)

The ancient manuscript that the graduate student who the new card catalog had confused a great deal ____ was missing a page.

Further experimental confirmations of the missing-VP effect were provided by Christiansen & MacDonald (2009) and Vasishth *et al*. (2010) for English and by Gimenes *et al*. (2009) for French. For the case of VO languages, the missing-VP effect is thus well established.

The only OV-language for which experimental evidence on the missing-VP effect is available, as far as we know, is German. In addition to English sentences, for which they adduced evidence for the missing-VP effect, Vasishth *et al*. (2010) also investigated German sentences as in (21) with the strikethrough verb either included or omitted.

(21) *German*

Der Anwalt, den der Zeuge, den der Spion betrachtete, ~~schnitt~~,
the laywer who the witness who the spy watched avoided
überzeugte den Richter.
convinced the judge
'The lawyer that the witness that the spy watched avoided convinced the judge.'

With both self-paced reading and eye-tracking, Vasishth *et al*. (2010) found increased reading times in the region following the higher relative clause for sentences with a missing VP in comparison to complete sentences, indicating that readers detected the ungrammaticality caused by the missing verb. Vasishth *et al*. (2010) hypothesize that the reason for this purported difference between English and German is that because of the head-final nature of German, readers of German have a stronger expectation of a VP and are therefore less prone to overlook the fact that a verb is missing.

The results of Vasishth *et al*. (2010) contrast with experimental results of Bader *et al*. (2003) and Bader (2012) as well as findings from the corpus study of Bader (2012). These experiments made use of the procedure of speeded grammaticality judgments, which we already introduced in section 2. One of the reasons for using this method for investigating the missing-VP effect is that this effect is one of a number of grammatical illusions, that is, ungrammatical sentences which are nevertheless perceived as grammatical under certain conditions. By using a method that explicitly asks for judgments of grammaticality, it is possible to obtain quantitative evidence on how often a grammatical illusion is experienced by native speakers.

All sentences investigated in Bader *et al*. (2003) had the head-noun of the complex relative clause located within the so-called German midfield, that is, the part of the sentences between C^0 and the clause-final verb(s) (the complex relative clause always consisted of a higher relative containing a lower relative clause in a center-embedded position). Two sample sentences illustrating this for the case of main clauses are shown in (22).

(22) a. *German*

Extraposed: Complete

Heute ist das Programm abgestürzt, das den Programmierer
today is the program crashed which the programmer
geärgert hat, der die Dokumentation ohne irgendeine Hilfe
annoyed has who the documentation without any help

erstellen musste.
write had-to
'Today the program crashed which had annoyed the programmer who had to write the documentation without any help.'

b. **Center-embedded: Complete or missing-VP**

Heute ist das Programm, das den Programmierer, der die
today is the program which the programmer who the
Dokumentation ohne irgendeine Hilfe erstellen musste,
documentation without any help write had-to
~~geärgert hat~~, abgestürzt.
annoyed has crashed
'Today the program crashed which had annoyed the programmer who had to write the documentation without any help.'

In (22a), the relative clauses have been extraposed. In (22b), the relative clauses occur center-embedded. Sentences as in (22b) were presented to participants either completely or with the struckthrough verbal complex omitted.[4] Complete center-embedded sentences, (22b), were judged as grammatical less often than extraposition sentences, (22a), although they still received a majority of grammatical responses. When the VP of the higher relative clause in center-embedded position was missing, sentences were judged as grammatical about half of the time. Other experiments showed acceptance rates of similar size. This indicates that comprehenders of German often, although not always, perceive missing-VP sentences as grammatical.

In the experiment reported in Bader (2012), a further comparison concerned the position of the complex relative clause that was missing the higher VP. Here, a striking difference between sentences with extraposed and sentences with center-embedded relative clauses showed up. When the relative clauses were extraposed, participants rarely overlooked the fact that a VP was missing. In contrast, when the relative clauses were center-embedded, participants often did not notice that the sentences were incomplete and therefore ungrammatical. More than half of the time (58%), participants judged sentences of this type as grammatical.

In sum, for sentences in which the head noun of the higher relative clause was located in the middle field, the experimental evidence shows that the missing-VP effect also occurs in the head-final language German. Although most of this evidence comes from experiments using grammaticality judgments, a recent experiment using self-paced reading (cf. Bader & Häussler 2012) supported the same conclusion. The different conclusions arrived at in the experiments of Vasishth *et al*. (2010) and in our experiments thus do not seem to be caused by different experimental procedures. The differences are probably due to the different syntactic positions of the relative clauses. A relative clause in SpecCP seems to be easier to process than a relative clause in the middle field (cf. Bader & Häussler 2010 for corpus evidence). This seems to make it easier to notice that a VP is miss-

[4] Other experimental conditions cannot be discussed here for reasons of space. See Bader *et al*. (2003) for further information.

ing in sentences like those investigated by Vasishth *et al*. (2010).

Further evidence for the reality of the missing-VP effect in German comes from the corpus study of Bader (2012), which was already discussed in section 2. In 15% of all corpus instances with a doubly center-embedded relative clause, the VP of the higher relative clause was missing, as in the original corpus example in (23).

(23) *German*

Missing-VP example

Dieser	Typ	entsteht,	wenn	lin-3	oder	ein	Gen,	das	für	die
this	*type*	*emerges*	*when*	*lin-3*	*or*	*a*	*gene*	*that*	*for*	*the*
Induktion,	die	von	der	Ankerzelle	ausgeht,	_____		mutiert	ist.	
induction	*that*	*from*	*the*	*anchor-cell*	*originates*			*mutated*	*is*	

'This type emerges when lin-3 or a gene that _____ for the induction that originates from the anchor cell has mutated.'

(Dewac-1/95201, http://www.zum.de)

In the other three sentences types investigated in Bader (2012), VPs were also sometimes missing, but with a substantially lower rate ranging from 0-2%. Of the three VPs involved, the VP of the lower relative clause was almost never omitted. The VP of the superordinate clause was missing in a small number of cases, but only in sentences with center-embedded relative clauses and never in sentences with extraposed relative clauses. The VP of the higher relative clause was missing in a substantial number of cases in doubly center-embedded relative clauses and also sometimes when the higher relative clause was extraposed but the lower relative clause still center-embedded within the higher relative clause. When the lower relative clause was extraposed behind the higher relative clause, the VP of the higher relative clause was never missing. The generalization that emerges is that VPs are missing only under circumstances of high processing load. Processing load is highest in sentences containing doubly center-embedded relative clauses, and the rate of missing VPs is accordingly highest in these sentences. Processing load is lowest in sentences in which the higher relative clause is extraposed behind the superordinate clause and the lower relative clause behind the higher relative clause, and there was not a single missing VP in these sentences.

In light of the overall pattern of missing VPs, the claim that the high rate of VPs missing from the higher relative clause in doubly center-embedded relative clauses is just a side effect of such sentences being particularly prone to grammatical errors in general can be rejected. Thus, we conclude that the missing-VP effect, which had previously only been reported for language comprehension, also occurs during language production.

As discussed above, Vasishth *et al*. (2010) have proposed that due to experience with the head final order of German, German comprehenders may maintain a prediction of an upcoming verb in a more highly activated state permitting the prediction to persist longer than in a head initial language. As shown by the corpus data reviewed above, language producers of German regularly forget the prediction of a VP and thus produce incomplete sentences. Thus, the prediction

of a VP in German is clearly not strong enough to prevent the omission of a grammatically required VP. Since comparable data are not available for English, we do not know whether producers of English forget to produce all VPs even more frequently, as would be predicted by the hypothesis of Vasishth *et al*. (2010).

In sum, the fact that the missing VP2 phenomena appears in both English and German shows that the regularities are deep and not a reaction to the particular configuration created by the word order of one language. If it were otherwise, it might lead to rather dramatically different processing systems in different languages, making the biolinguistic program somewhat less plausible. But, as in other domains, the processing system looks largely the same across languages, modulo differences in the grammar itself.

4.2. Mismatch Ellipsis

After having demonstrated that the missing-VP effect is not due to particular ordering in specific languages but, instead, points toward deep regularities that belong to the biologically grounded performance systems, we now turn to another case of acceptable ungrammaticality: mismatch ellipsis. As in the case of the missing-VP effect, we will present evidence from both production and comprehension, thereby supporting our view that the properties that can be attested for this case of acceptable ungrammaticality are part of an abstract knowledge of the performance systems that constrains both production and comprehension. Let us first introduce the phenomenon we are dealing with here.

Focusing on Verb Phrase Ellipsis, it is well known that the grammar requires the elided constituent and its antecedent to match syntactically, apart from certain morphological features (Sag 1976, Williams 1977). Counter-examples to this claim include the prominent example in (24):

(24) This information could have been released by Gorbachov, but he chose not to.

(Daniel Shorr, NPR, 10/17/92, reported by D. Hardt)

Examples without a matching antecedent raise two problems for the approach advocated here. One problem is to explain why listeners and readers tend to accept certain mismatch ellipsis examples like (24) if indeed they are ungrammatical, and one problem is to explain why speakers equipped with a grammar prohibiting 'mismatch ellipsis' sentences like (24) would produce them anyway. In what follows, we will argue that the solutions to these two problems are related: speakers utter mismatch ellipsis examples as speech errors and listeners repair such errors, finding them relatively acceptable under particular conditions where they are easy to repair, they sound like a form the human language production system would produce, and the repaired meaning is plausible. Let us first turn to evidence for repairs and show that the acceptability of mismatch ellipsis depends on the number of repairs and on the amount of evidence for each repair.

When an elided VP has a syntactically mismatching antecedent, the processor attempts to repair the antecedent. If this can be done easily (with only a small

number of operations for which there is plentiful evidence in the input), then the ellipsis will be repaired. This predicts gradient acceptability depending on the number of repairs. Arregui *et al*. (2006) provided experimental evidence for this prediction, showing in a written acceptability judgment study that acceptability drops as one moves from VP ellipsis examples containing a matching VP antecedent in predicate position (25a), to a VP in subject position (25b), to examples requiring a trace to be replaced by its ultimate binder (25c) to very low acceptability for examples where the required VP antecedent could only be built by deconstructing a word to create the verb needed to head the VP antecedent (25d).

(25) a. None of the astronomers saw the comet, /but John did.
(Available verb phrase)

b. Seeing the comet was nearly impossible, /but John did.
(Embedded verb phrase)

c. The comet was nearly impossible to see, /but John did.
(Verb phrase with trace)

d. The comet was nearly unseeable, /but John did.
(Negative adjective)

Percentage Acceptable Responses, Experiment 1	
Condition	% Acceptable
a. Available verb phrase	82.8
b. Embedded verb phrase	66.1
c. Verb phrase with trace	43.9
d. Negative adjective	17.1

Table 2

In subsequent studies, Arregui *et al*. showed that the results could not be due to the antecedent alone, but implicated repair of the antecedent, and they presented further findings, e.g., showing that VP ellipsis examples with verbal gerundive antecedents were more acceptable than ones with nominal gerundive antecedents.

Fanselow & Frisch (2006) show that processing difficulty may decrease the acceptability of a sentence in uncontroversially grammatical sentences such as object-initial German sentences. So it is not terribly surprising that repair difficulty influences rated acceptability if comprehension of the sentence involves identifying and making the hypothesized repair(s). Though the complexity of the repair operation (the number of repairs and the amount of evidence for them) is clearly related to acceptability, it is insufficient to give a full picture of the acceptability of mismatch ellipsis. For this we need to consider what is known about acceptability judgments in other cases of ungrammatical sentences (see Otero 1972 for an early example involving confusion among different *se*s in Spanish).

In general, acceptability ratings are higher if even one example of a structure has been encountered before rating a novel example. In five experiments, Luka & Barsalou (2005) had participants read grammatical sentences first and then rate novel sentences. Mere exposure to a sentence, or to a sentence structure with different words, resulted in higher ratings. For example, *Egor lugged Dr.*

Frankenstein the corpse was rated higher after reading unrelated sentences with double object structures, and *What the pharmacist recommended is to read the directions* was rated higher after reading sentences with a pseudo-cleft structure.

Frazier (2008a) considered the question of whether a speech error, essentially a blending of two forms, would be rated as more acceptable if it sounded like a natural or motivated speech error than if it did not. In a small experiment with Chuck Clifton, participants were asked to rate the acceptability of two items like (28), where (28a) is an actual speech error, heard on National Public Radio. The actual error (28a) involves a switch from impersonal *you* to *we* that would avoid the unwanted implication that the addressee was deluding himself. The unmotivated error in (28b) involves the same switch of subjects, now from *we* to *you*, but suggests that the speaker has gone out of his way to insult the addressee.

(28) a. If you think this is going to solve the terrible problems in Najaf, we're deluding ourselves.
b. If we think this is going to solve the terrible problems in Najaf, you're deluding yourself.

Acceptability judgments (1–5, where 5 means perfectly acceptable) were indeed influenced by how natural the error was: Natural errors like (28a) received a mean rating of 4.05, which was significantly higher than the mean of the unnatural errors (3.37). The implication of these studies is that familiarity of novel forms, and even how natural a particular form is as an output of the human language system, influences acceptability. The studies thus reinforce the conclusion that acceptability ratings reflect the judged goodness of utterances based on both grammatical and performance factors. By contrast, the classification of an utterance as grammatical or ungrammatical is a theoretical matter. The status of an utterance as being grammatical or ungrammatical follows from the best most explanatory overall theory of language, that is, if the judged badness of an utterance follows from independently needed grammatical constraints, the utterance is ungrammatical; if its badness follows from independently known performance factors, the utterance is unacceptable.

In the case of mismatch ellipsis, it is clear why speakers might utter an antecedent clause and an elided clause that don't match. Memory may lead them to misremember. Since it is known that passives are misremembered more often as actives than the other way around (Mehler 1963), this predicts an asymmetry: Passive-active mismatches, as motivated errors, should be more acceptable than active-passive mismatches. This prediction was confirmed (cf. Arregui *et al*. 2006). With coreferential subjects, conjoined clause antecedents might be misremembered as conjoined VP antecedents. This predicts that listeners and readers might choose conjoined VP antecedents for an elided VP even when one didn't actually occur. This too has been confirmed (cf. Frazier & Clifton 2011a).[5]

[5] Whether argument structure alternations other than passive-active ones give rise to acceptable mismatches has not been investigated thoroughly. For example, 'X and Y collided' might compete with 'X collided with Y' resulting in a sentence like *Sue and Mary will collide, I think, and John will with George*, which sounds relatively acceptable to us. However, on the present account, only alternations that are alternative expressions of the SAME proposition

Moving beyond ellipsis, we will now see that the notion of repair or speech error reversal advocated here is not intrinsically tied to ellipsis and that repairing syntactic blends can be established in other domains as well: when alternative linguistic forms compete in production, the listener repairs the input based in part on the speaker's likely intent.

Frazier & Clifton (2011b) investigated doubled quantifiers. In a language like English, speakers must choose between using a determiner quantifier (*everyone, nobody*) or an adverbial quantifier (*always, never*). Sometimes both show up in the same (blended) utterance, as in (34).

(34) *Doubled quantifiers in attested blends*

a. Many people often thought that you use whipped cream pie.
(National Public Radio, discussion of clowns and pie throwing)

b. Typically when I meet people I often ask people what they would talk about if this wasn't a job talk.
(Introduction to a University of Massachusetts colloquium, 3-22-10)

c. …and it might not require scientific research to infer that the majority of sarcasm one encounters is usually spoken.
(Undergraduate paper, University of Massachusetts, Spring, 2010)

In a written interpretation study, participants were asked to choose the interpretation they gave to sentences like (35) with both a determiner and an adverbial quantifier. Four types of examples were tested: *many–often*, as in (35), *every–always*, negation examples of various types, and *few–seldom*. The data are presented in Table 3.

(35) Many students often turn in their assignments late.
What did that mean?

a. The number of students who turn in their assignments late is large. (Undoubled)

b. The number of students who frequently turn in their assignments late is large. (Doubled)

Percentage Choices of Undoubled Paraphrases, by Item Set (with Standard Errors in Parentheses) Item Set			
many (many-often)	universal (every-always)	negation (no-never)	few (few-seldom)
77	64	36	73

Table 3

Apart from negation, the majority of interpretations undoubled the quantifier, as would be expected if participants reversed the speech error before interpreting the sentence. Notice that without the speech error reversal, only the interpreta-

should be acceptable. Hence, *John drove the car*/*The car drove well* or *Ina melted the ice*/*The ice melted* would not be expected to give rise to blended utterances and thus not be expected to give rise to acceptable ungrammaticality.

tion like (35b) is grammatical according to the compositional semantics (cf. Frazier & Clifton 2011 for problems with an attempt to interpret the doubled forms as emphatic).[6]

Another example of speech error reversal, pointed out to us by Greg Carlson, involves sentences like (36). People fail to notice the grammatical interpretation of the sentence, where the mother kills the child by preventing her from almost drowning.

(36) Mother saves child from nearly drowning.

(37) *Prevent X from happening/X almost happened*
  a. Mother saved the child from nearly drowning.
  b. Mother saved the child from drowning.

In a written study (Clifton & Frazier, in progress) where participants indicate whether sentences were "o.k., acceptable," overwhelmingly participants accepted sentences like (37a) as well as sentences like (37b). This result, like the quantifier undoubling result, suggests that comprehenders reverse common or natural errors, assigning interpretations that would be unacceptable if the reversal did not take place.

To sum up, various sources of evidence suggest predictable speech errors involving a blend of two competing forms are repaired by listeners. The interpretation that goes with the repaired utterance is only accepted as a possible interpretation when it is a plausible interpretation. This stands in stark contrast with unrepaired utterances that are paired with their meanings by the compositional semantic interpretation of the actual utterance. In other words, the performance based pairing of form and meaning is token-based, and it relies on the performance systems (competing morphological or syntactic forms in production, comprehension repair mechanisms based in part on knowledge of the speaker's probable intent) together with the compositional semantics to pair form and meaning. In the case of mismatch ellipsis, the repair involves licensing of a later form based on the unselected form of the antecedent clause, as illustrated in (40). Given a particular message, two forms are available for expressing the message (active-passive; conjoined VP-conjoined clause). The speaker chooses one form but a later (ellipsis) clause is licensed only by the unchosen form.

(40) Meaning — Form 1–Speaker chooses Form 1
  — Form 2–Licenses later form (e.g., ellipsis)

By hypothesis, it is implicit knowledge of human language performance systems that allows the comprehender to repair the form, as if 'reading through' the error.[7] Adopting this explanatory strategy, certain attested utterances are explained

[6] We suspect that the double negative examples were treated differently because American students are taught in school to avoid doubling negation. Perhaps if we were to test less educated subjects, the rates of undoubling for negation would be on a par with those for the other quantifiers.

[7] Error reversal repair is NOT a form of sloppy or 'good enough' processing. Ferreira & Pat-

outside the grammar proper. This permits a much simpler grammar than would otherwise be possible, as in the case of mismatch ellipsis. It also offers a more nuanced account of the data, explaining why particular tokens of a structure may be acceptable even though other tokens are not. In the case of doubled quantification and prevent-near-culmination sentences (37), it is an ungrammatical interpretation that is acceptable.

To sum up, the findings presented in this section demonstrate that alongside the type based grammatical system for pairing forms and meaning, we must recognize the existence of a token based system that allows certain utterances to be patched up before they are interpreted. Let us now broaden the perspective again and ask how cases of acceptable ungrammaticality like the missing-VP effect and mismatch ellipsis can contribute to a biolinguistic explanation of the design features of human language.

4.3. The Performance Interface and Acceptable Ungrammaticality

In the sections above, we presented empirical evidence that implicit knowledge of the human language performance systems is systematic and is needed in the context of acceptable ungrammaticality. Together with the methodological benefit of relegating certain grammatical constraints to the performance systems, which we have demonstrated in the context of center-embedding (section 2) and linear ordering (section 3), this is a strong argument to factor performance systems into the biolinguistic approach to language, since the overall goal of this approach is to reduce UG and to focus on third-factor explanations instead.

Both the missing-VP effect and the case of mismatch ellipsis are grammatical illusions, a class of phenomena that have not been systematically studied until recently. For many scholars, grammatical illusions are a good reason to distinguish between structures built on-line and structures generated by the grammar. In other words, the phenomena subsumed under the term 'grammatical illusions' seem to contradict the view that grammatical constraints are transparently implemented in real-time language processes. Accordingly, the recent attempts to return to the axioms of the derivational theory of complexity mentioned at the outset of section 4 seem to be challenged by grammatical illusions.

Phillips *et al*. (2011), however, take issue with this class of phenomena and argue in favor of a "systematic account of selective fallibility that can predict the on-line effects of an individual [grammatical] constraint based on its structural description" (Phillips *et al*. 2011: 168). In other words, committed to the general view that 'the parser is the grammar' (Phillips 1996), they assume that grammatical illusions can be ultimately traced back to properties of the grammar. As the research overview by Phillips *et al*. (2011) shows, grammatical illusions is a matter of intense investigation, with a lot of specific proposals, and we cannot do justice to them here. We do not deny the view that both parsing and production make heavy use of the same syntactic mechanisms in the grammar, as Phillips

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son (2007) among others have argued that the processor assigns only as much structure or interpretation to a sentence as is required for a particular task (cf. Frazier 2008b for a critique).

and colleagues claim. However, both tasks are subject to several external constraints and we advance the claim that (many of) these constraints can be accounted for in terms of systematic properties of the performance systems rather than by resorting to a single grammatical module with a strong (and rich) predictive component that is embedded in a noisy cognitive architecture (cf. Phillips to appear). At this point, we would like to argue that this reasoning is unwanted, given the biolinguistic framework to reduce UG. As we already have made clear at the outset of the paper, approaches that try to translate formal models of grammar into performance models by assuming constraints in the grammar that could also be accounted for by a 'type 3 explanation,' that is, by no reference to the grammar at all, are not in line with the general impetus of biolinguistics to ascribe as many properties of language design as possible to third factors. More concretely, since Phillips & Lewis (to appear: 15) claim that "'generate-and-filter' mechanisms are familiar from many grammatical theories […], and hence are plausible components of a real-time grammar," they try to provide an account of grammatical illusions that unnecessarily complicates the grammar, thereby deviating from the methodological standards set by both Fodor (1985) and Chomsky (2005).

In contrast to Phillips and colleagues, we argue that what makes illusory cases acceptable is not located in the grammar at all but is due to systematic properties of the performance systems. In this context, let us recall that Chomsky (1965: 4) pointed out that the actual behavior of a speaker-hearer is "the interaction of a variety of factors, of which the underlying competence of the speaker-hearer is only one." That is, the competence grammar interacts with other cognitive components when language production and comprehension take place. One of these components is the implicit knowledge of human language performance systems. Taking this perspective of interacting systems seriously has an interesting consequence for the subject of acceptable ungrammaticality and for one of the main data sources of theoretical linguistics — acceptability judgments — in general: Since judging a sentence is also an interaction effect, it may well be that the fact that speakers have remarkably stable judgments about a large amount of sentences is not per se an indication of the nature of the grammar. As we saw in this section, other cognitive components such as the performance systems may well boost the acceptability of sentences. In other words, judgments always involve both the grammar and the processor. It's just in many simple examples it's harmless to ignore the contribution of the processor. But once one gets into more complicated examples (longer, more complex, less well understood), it becomes apparent that grammar and processor are always implicated in judgments.

5. Conclusion

In this paper, we have shown that systematic properties of performance systems can play an important role within the biolinguistic perspective on language by providing third-factor explanations for crucial design features of human language. In particular, we have demonstrated that the performance interface in language design contributes to the biolinguistic research program in three ways:

(i) it can provide additional support for current views on UG, as shown in section 2 in the context of complex center-embedding; (ii) it can revise current conceptions of UG by relegating widely assumed grammatical constraints to properties of the performance systems, as shown in section 3 in the context of linear ordering; (iii) it can contribute to explaining heretofore unexplained data that are disallowed by the grammar, but can be explained by systematic properties of the performance systems.

At the outset of our paper, we referred to Berwick & Weinberg (1984) as a prominent case of attempting to directly translate formal models of linguistic competence into a performance model. Recently, many scholars point out that a revitalized version of the derivational theory of complexity may be the best way for the Minimalist Program to move forward (cf. Marantz 2005). It strikes us as particularly interesting that the recent minimalist literature appeals to notions of 'efficiency' and 'computational economy' and refers to derivations as 'actual computations'. The most telling case in this regard might be the notion of 'phase', basically (re)introducing the concept that syntactic derivations proceed in incremental chunks. In particular, Chomsky (2000a: 106) claims that "at each stage of the derivation a subset […] is extracted, placed in active memory (the 'workspace')." As we have exemplified throughout the paper, there are, according to Fodor (1985), roughly two explanatory strategies concerning such cases where the nature of linguistic constraints obviously suggests a connection between the grammar and the properties of the performance systems: (i) assuming that properties of the performance systems provide a functional explanation for the constraints in the grammar (type 2 explanation), or (ii) taking into account that the constraints might be motivated independently and do not exist in the grammar (type 3 explanation). In this paper, we provided a new perspective on 'type 3 explanations'. Specifically, we introduced a notion of performance systems that dovetails well with the biolinguistic methodology of reducing UG by referring to third-factor explanations. We hope our paper thereby encourages addressing other features of current syntactic theory from the perspective of the performance interface in language design.

Importantly, our notion of performance properties does not contradict basic axioms of linguistic theory. As we highlighted in section 4, linguistic behavior like acceptability judgments, are, according to Chomsky (1965), interaction effects. Crucially, however, according to the biolinguistic perspective, which is characterized by focusing on how the language faculty is biologically grounded in the brain, "[t]here is good evidence that the language faculty has at least two different components: a 'cognitive system' that stores information in some manner, and performance systems that make use of this information for articulation [and] perception" (Chomsky 2000b: 117). Consequently, within biolinguistics, the theory of grammar and the theory of performance characterize two objects at the same level of description, since interaction in terms of information flow is postulated between the grammar and the performance systems. In this paper, we have strengthened this view that the performance systems, beside the grammar, constitute a distinct cognitive component of biolinguistic inquiry by showing that these systems are not random but characteristic. Performance in our sense involves systematic properties of the language processing system, not just the

study of phenomena like errors made when drunk or after stubbing a toe. In other words, non-trivial systematic processing phenomena will be part and parcel of understanding the grammar (its boundaries, i.e., what it must account for and what not). To uncover these systematic properties, the examples discussed in the paper involve both comprehension and production (the role of errors in acceptability judgments and repair, corpus and comprehension experiments for center-embedding and the missing-VP effect).

In sum, this paper contributes to the biolinguistic explanation of language design by shifting away from a rich innate UG towards ultimately innate and neurally determined processing mechanisms that belong to the domain of third-factor effects — a domain that offers a promising perspective for future collaboration and cross-fertilization of linguistic theory and psycholinguistics.

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The Talking Neanderthals: What Do Fossils, Genetics, and Archeology Say?

Sverker Johansson

Did Neanderthals have language? This issue has been debated back and forth for decades, without resolution. But in recent years new evidence has become available. New fossils and archeological finds cast light on relevant Neanderthal anatomy and behavior. New DNA evidence, both fossil and modern, provides clues both to the relationship between Neanderthals and modern humans, and to the genetics of language. In this paper, I review and evaluate the available evidence. My conclusion is that the preponderance of the evidence supports the presence of at least a spoken proto-language with lexical semantics in Neanderthals.

Keywords: archeology; DNA; fossils; language; Neanderthal

1. Introduction

That modern humans have language and speech, and that our remote ancestors did not, are two incontrovertible facts. But there is no consensus on when the transition from non-language to language took place, nor any consensus on the species of the first language users. Some authors regard language as the exclusive province of anatomically modern humans [AMH] (Klein 1999, Skoyles & Sagan 2002, Crow 2005, Lanyon 2006, among others), whereas others argue that at least proto-language in some form, if not full modern language, can be found in some earlier species (Mithen 2005, Bickerton 2009, Corballis 2002, among others).

Neanderthals[1] have a key position in this debate, being a late major side branch in human evolution with human-like capacities in many other respects, notably a brain at least as large as ours. Their capacity for language or speech has been discussed in numerous papers over the years, stretching from Lieberman & Crelin (1971) over Schepartz (1993) to Benítez-Burraco *et al*. (2008) and Barceló-Coblijn (2011). The latter offers what is presented as a "biolinguistic approach" to the issue, but unfortunately the approach is neither comprehensive nor stringent.

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[1] Ever since Neanderthals were discovered in the 19th century, there has been a lively debate over whether they are a separate species from us or not — *Homo neanderthalensis* or *Homo sapiens neanderthalensis*? I am not going into the naming debate here, as the name *per se* is irrelevant to the topic of this article; instead I will call Neanderthals 'Neanderthals', and call the people indistinguishable from ourselves 'anatomically modern humans' [AMH].

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In this paper, I will explore what fossil, archeological, genetic, and other evidence can, and cannot, say about Neanderthal language. A fuller discussion of many related issues can be found in Johansson (2005), and specific constraints on the timing of language emergence in Johansson (2011).

All modern human populations have language, obviously, and there is no evidence of any difference in language capacity between living human populations. Given that language has at least some biological substrate, parsimony (see Section 2.1) implies that the most recent common ancestor of all modern humans had language, and had all the biological prerequisites for language.

The fossil record of AMH goes back to nearly 200,000 years ago in Africa (MacDougal *et al*. 2005, Marean 2010). The molecular data likewise strongly support a common origin for all extant humans somewhere around 100–200,000 years ago (Cann *et al*. 1987, Ayala & Escalante 1996, Wood 1997, Bergström *et al*. 1998, Cavalli-Sforza & Feldman 2003, Fagundes *et al*. 2007, Atkinson *et al*. 2008). The relation between population divergence times and genetic coalescence times is non-trivial (Hurford & Dediu 2009), but it is hard to reconcile the genetic data with a common ancestor of all modern humans living much less than 100,000 years ago. This is consistent also with fossil and archeological evidence indicating that modern humans had spread across much of the Old World more than 50,000 years ago. It follows that the origin of the human language faculty is very unlikely to be more recent than 100,000 years ago (Johansson 2011).

This 100,000-year limit brings us back to a time when Neanderthals and AMH were living side by side, with similar material culture, and quite possibly encountering each other in the Middle East. Did only one of them have language, or both?

2. Methodological Issues in Reconstructing Neanderthal Capacities

As noted in just about every paper ever published on language in prehistory, language does not fossilize. Thus the evidence bearing on Neanderthal language is necessarily indirect, and bridging theories (Botha 2008) are required in order to make inferences about the presence or absence of language in an extinct species. A few general methodological issues are discussed in this section.

2.1. Parsimony

Parsimony as a general concept is basically the same as Occam's razor — do not multiply entities needlessly, keep theories as simple as possible, and in the choice between two alternative explanations that both explain the data prefer the simpler one.

In the context of inferring the evolutionary history of a group of organisms, parsimony has the more specialized meaning that the simplest history should be preferred, simplest in the sense of requiring the smallest amount of evolutionary change. The main use of parsimony is in choosing between several alternative hypotheses about the branching pattern of the family tree — the pattern minimizing the total amount of evolutionary change is to be preferred. The

general idea is quite old, but it was formalized and elaborated by Hennig (1966) under the label *cladistics*.

A byproduct of the use of parsimony in the choice of family tree hypothesis is that it also supplies inferences about the features of the common ancestor at each branching point of the tree.

Parsimony is based on the assumption that evolution is unlikely to repeat itself. In the case of complex features, dependent on multiple co-evolved genes, this is a highly reliable assumption. The evolution of a complex feature is a rare occurrence, so it is very unusual for the same complex feature to evolve twice in different organisms.[2] The corollary of this is that if we do observe the same complex feature in two related organisms, we can safely assume that it evolved only once, and that their common ancestor possessed it already (Byrne 2000). In the case of language, this means that any language-related features displayed by for example chimpanzees today, were most likely present already in the common ancestor of us and chimpanzees, and did not evolve for human-level linguistic purposes. All such features would be part of the FLB *sensu* Hauser *et al*. (2002).

This has the corollary that if chimps and modern humans share a feature, then all other species that are also descended from the common ancestor of chimps and modern humans, notably all extinct hominins (including Neanderthals), most likely also possessed that feature. In the absence of positive evidence to the contrary, we can thus safely assume that all features shared by chimps and modern humans were also present in Neanderthals.[3]

When we get to the genetic evidence, however, it should be noted that with the minor genetic changes that are typically analyzed in molecular phylogeny (DNA-based family tree reconstruction), the parsimony assumption is frequently violated. Random DNA changes happen often enough that evolutionary reversals and repetitions may add significant amounts of noise to the data. Worse: Non-random changes, mainly driven by natural selection, may add systematic bias that can skew the results in unpredictable ways. Edwards (2009) briefly reviews different ways in which this may happen, some of which likely apply to human evolutionary history.

For this reason, molecular phylogenetic reconstruction is an art nearly as much as a science, as it requires informed judgment on which data to include, what assumptions to make in the analysis, and how to interpret the results. The inferred human evolutionary history can appear quite different depending on which part of our DNA is used in the analysis, and caution is urged in interpreting the results of single studies; cf. Section 4.1 below.

The same applies to non-complex anatomical features, for example simple quantitative changes in the dimensions of some bone. Such features may also display substantial reversal and repetition, making them less informative about

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[2] This does not mean that it never happens. Convergent evolution of the "same" complex trait in distantly related organisms does happen occasionally. Classical examples are the similar body shapes of dolphins, sharks, and ichtyosaurs, and the wings of birds, bats, and pterosaurs. But in complex traits it is rare enough, and recognizable enough, that parsimony remains a useful heuristic.

[3] Note that this applies regardless of whether Neanderthals are classified as the same species as us or not (cf. Section 5 below). The inference is valid as long as Neanderthals are also descendants of the last common ancestor of chimps and us, which is indubitably the case.

phylogeny. Parallel evolution of the same feature in related lineages — homoplasy — is a non-trivial issue in primate evolution (Lockwood & Fleagle 1999).

2.2. What is Language?

Language is a complex concept, not easy to define in any stringent manner even in modern humans, and there is a regrettable lack of consensus among linguists, to the extent that the field can be called poly-paradigmatic (Zuidema 2005). The links between linguistic theory and neurological observables in the brain are also tenuous at best (Poeppel & Embick 2005, Deacon 2006, Fedor *et al*. 2009). This means it is prudent to avoid too theory-laden definitions of language and its components in a study such as this one.

Nevertheless, some definitions are needed, and we do have a core of real linguistic phenomena around which to define language. There is consensus that syntax is an important component, and on the reality of some syntactic patterns and generalizations, even if linguists disagree on their theoretical description (Számádo *et al*. 2009). There is also consensus that lexical semantics is an important component, though with similar theoretical disagreements, and also disagreements on whether there is a sharp division between syntax and lexicon, or not (Jackendoff 2011). Hockett (1960) compiled a longer list of 13 design features of language, about which there is also general agreement.

Many of Hockett's features concern the externalization of language. The normal modality of externalization among modern humans is vocal speech, but language can be used also in other modalities, notably sign language. In recent times, a large fraction of all language use is in a written modality. As language is usable in a variety of modalities, modality-specific features should not be part of the definition of language. But if language is used for communication, some form of externalization is obviously necessary. Likewise, language acquisition in the child would be impossible without externalized language in the environment. Chomsky (2010) argues that the computational core of the language faculty was used for purely internal purposes at first, with externalization coming at a later stage. This is a defensible conjecture, though I do not regard it as likely (cf. Lewis *et al*. in press). But such purely internal use of the computational machinery alone, unlike our everyday internal use of external language forms ("inner speech"), would fall outside my definition of language.

The relation between speech and language deserves some further comments. The conflation of speech and language is a common mistake in language origins studies (Botha 2009), and a substantial part of the literature on aspects of Neanderthal language actually concerns proxies for Neanderthal speech. It is clear from the modality-independence of language that the absence of speech does not entail the absence of language. But what conclusions can be drawn from the presence of speech (or proxies thereof)? Most mammals have some form of vocalizations, that we do not call 'speech'. I would argue that the label 'speech' is normally used for, and should be specifically reserved for, vocal externalization of language, not for non-linguistic vocalizations. Given that definition, the presence of speech trivially entails the presence of language. But that just moves

the problem one step: How do we infer the presence of speech, as distinct from other vocalizations? A major feature distinguishing speech from other primate vocalizations is its digital, combinatorial nature, with utterances formed from the unlimited combinability of a modest number of discrete phonemes. This places stringent demands both on the acoustic capacity of the vocal tract to form a sufficient number of distinct sounds, and on the ability to control the vocal apparatus with sufficient precision and rate. Proxies for the presence of enhanced vocal capabilities and vocal control are thus reasonable proxies for speech. Birdsong, however, shares many of these features with speech, and requires comparable vocal capabilities; it is possible to argue that the selection pressures driving the evolution of vocal capabilities in humans were due to birdsong-like activities somewhere along the human lineage, and that our vocal abilities were only later exapted for speech (cf. Mithen 2005). The force of the inference from vocal capabilities to speech to language thus depends on the plausibility of alternatives like singing.

In the context of Neanderthal language, there are further issues to be considered. We can all agree that the main communication system used by modern humans is language, pretty much by definition. Most of us also agree that the various communication systems used by non-human primates in the wild are *not* language (*pace* Kanzi). It is established far beyond reasonable doubt that humans evolved from ape-like ancestors. If we go back far enough in history, our ancestors were undoubtedly language-less by any reasonable definition. This means there must have been a transition from non-language to language during the course of human evolution. But there is little consensus on the nature of this transition — was it a sharp single-step leap (e.g. Piattelli-Palmarini 2010) or a gradual evolution in many small steps (e.g. Johansson 2005, 2006; Jackendoff 1999, 2011) — nor any consensus on at what stage to start applying the label 'language'.

Chomsky (2010) and Piattelli-Palmarini (2010) appear to argue that unbounded Merge is the key defining component of language, and that the notion of some form of partial Merge evolving gradually is either incoherent or silly, leading to the conclusion that the transition must have been sharp. This argument has some force, but only if it is assumed that the conjecture of Hauser *et al*. (2002) that recursion is the *sine qua non* of language is correct. And even within this paradigm, there is no consensus that partial Merge is impossible. In contrast, Fujita (2009) proposes precisely a gradual evolution of Merge with precursor stages, connecting it with Action Grammar. Also Boeckx (2011), Progovac & Locke (2009), and Bolender *et al*. (2008) argue for a decomposable Merge, with the latter proposing that External Merge may have preceded Internal Merge. A sharp transition to perfect language is also problematic in the light of evolvability considerations (Kinsella 2009).

Outside the Chomskyan paradigm, there is no strong reason for postulating any limits in principle on the decomposability of language into different components that may have been added one after the other, and that may have been refined gradually, during the course of language evolution. Numerous proposals for such decomposition exists, for example Jackendoff (1999) or Johansson (2006).

As noted earlier in this section, I find it imprudent to use a theory-laden

approach to Neanderthal language, when there is no consensus on the underlying theory. This places me in the decomposability camp; I do not see any reason to exclude *a priori* the possibility that Neanderthals may have had a form of language that lacked some of the features of modern human language.

If we thus assume that a variety of communication systems are possible that possess some, but not all, of the features of human language, the next question becomes: What labels we should use for systems with different combinations of features. What is required for a system to deserve the label 'language'? Should the label 'proto-language' be used and, if so, for what class of systems? Are more labels needed? For example, 'semilanguage' has also been proposed (Stade 2009).

One possible position is that the label 'language' should be reserved for full modern human language, with unbounded recursion and all the bells and whistles. But I would argue that this is neither proper nor in accord with actual usage of the word 'language'. There are many restricted systems that we nevertheless call, and should call, 'language'. Suppose for a moment that Everett (2005) is accurate in his assessment that Pirahã lacks recursion[4] — would we then stop calling Pirahã a language? I don't think so. Both Botha & de Swart (2009) and Givón (2009) consider various other restricted linguistic systems (pidgins etc.). While these systems may or may not be informative of the phylogeny of language, they do provide a proof-of-existence of partial systems that lack one or more component of full modern human language but nevertheless are functional communication systems with lexical and propositional semantics, in which people can and do manage coherent multi-propositional discourse. These systems also deserve the label 'language', in my opinion.

For me, the *sine qua non* of language is a symbolic communication system that is not fixed; extensibility is an integral part of the system. This amounts to the presence of something like lexical semantics, flexibly and learnably mapping forms to meanings. A system that lacks word-like units, or only has a fixed set of 'words' (e.g., vervet monkey alarm calls), is not language. A system that has units that are combined in syntax-like patterns, but that lacks a mapping to meanings, such as birdsong, is likewise not language.

A system possessing lexical semantics but not syntax I would call a proto-language. Piattelli-Palmarini (2010) argues that such a system is inconceivable, but his argument has merit only within a specific theoretical paradigm. From a less theory-laden perspective, semantics without syntax cannot be excluded *a priori*, and indeed forms the basis of various proposed proto-languages, for example Bickerton (2009). Note that with the definitions that I use, proto-languages are a subset of all languages, so when I talk about "some form of language", this includes proto-language.

The question from the end of Section 1: "Did only one of them [AMH & Neanderthals] have language, or both?" thus becomes too black-or-white simplistic. A more reasonable question is what features of language we can find evidence for in Neanderthals.

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[4] There is considerable doubt about that assessment; see e.g., the counterarguments of Nevins *et al*. (2009).

2.3. Which Types of Observables may be Informative of Neanderthal Language Features?

When trying to determine whether Neanderthals had language, in the absence of direct evidence we need usable proxies for language, or for specific features of language. Criteria for a useful proxy include:

(1) Among living species, the distribution of the proxy must coincide with the distribution of language. A feature that is shared between humans and language-less non-humans is not a useful proxy for language. This criterion eliminates large parts of the FLB, for example most aspects of sound perception, and also the much-hyped mirror neurons (see Section 3.4).

(2) The state of the proxy in Neanderthals must be knowable; in practice, this means it must be a feature that is preserved in fossils or archeology. This criterion eliminates most soft anatomy, as well as those behaviors that do not leave archeologically visible traces.

(3) There must be a sufficiently solid bridging theory connecting the proxy to language, so that the presence of the proxy entails the presence of language with an acceptable degree of certainty. Preferably, the entailment should be two-sided, so that the absence of the proxy likewise entails the absence of language. We must also be careful to distinguish proxies for language from proxies for specific features of language. Botha (2009) discusses this at some length, noting that the inference from symbolic behavior to syntactical language (e.g., Henshilwood & Marean 2003) is imperfectly supported. Another proxy that has been extensively invoked in the literature is vocal tract anatomy, but the connection from vocal tract anatomy to language is much less firm than has been believed (see Section 3).

2.4. Which Null Hypothesis is Appropriate in Neanderthal Studies?

One aspect of parsimony that can be sensitive in studies of human origins, is that any similarities between humans and other apes are most parsimoniously explained as having a common origin, and also a common mechanism (Hume 1739, quoted in de Waal 2009). But postulating that features of other apes, especially behavioral and cognitive ones, are basically the same as the corresponding human features, risks running afoul of another methodological principle: that anthropomorphism should be avoided in studies of animal behavior (Asquith 2011), and that animal behavior should be explained in the simplest possible terms, not postulating more advanced capabilities than is absolutely necessary (Morgan 1903). But yet other researchers argue that a similar principle, seeking the simplest explanation first, ought to be the norm also in the study of human behavior (Buchanan 2009).

It is important here to avoid unconscious bias in either direction, which can easily lead to circularity. Especially taxonomic bias — allowing conclusions from data to be colored by which species generated the data — can be a pitfall in language evolution research (d'Errico *et al*. 2009b, d'Errico & Henshilwood 2011).

This can be a problem both in archeology, where identical artifacts may be given different interpretations depending on whether they were manufactured by AMH or Neanderthals — or on whether the interpreter classifies Neanderthals as *Homo sapiens neanderthalensis* or *Homo neanderthalensis* — and in experiments with living subjects, where similar vocalizations are given different interpretations depending on if they are uttered by a human baby or by a chimp or a parrot.

In the case of hominins other than AMH this issue is a significant concern. Should we place the burden of proof on anybody arguing that they do have language or other human cognitive traits? Or should we regard them as fundamentally human, and thus shift the burden of proof to anybody arguing that they are not like us? The answer is not self-evident, and it would appear most prudent to proceed without a null hypothesis, judging the issue by the preponderance of the evidence, and keeping the possibility of bias firmly in mind.

3. Fossil Anatomy

Our habitual use of spoken language is reflected in certain aspects of our anatomy that can be studied in fossils. Speech adaptations can potentially be found in our speech organs, hearing organs and brain, and in the neural connections between these organs.

There are several issues to be considered before making inferences from either the presence or the absence of speech adaptations:

- Are the apparent speech adaptations actually adaptations, or are they byproducts of adaptation for other purposes — exaptations — or the result of non-adaptive evolutionary processes? The substantial choking risks associated with a permanently lowered larynx (Darwin 1859, D. Lieberman 2008)[5] implies the existence of even larger benefits, otherwise loss of fitness would weed out any such mutations, which would argue for them being adaptations.
- Are the apparent speech adaptations actually adaptations to speech, or to some other form or aspect of vocalizations? As apes manage to vocalize just fine with their vocal apparatus, candidates here include only those human vocal activities that apes can't do. Apart from speech, singing (cf. Mithen 2005) and vocal imitation (cf. Lewis 2009) are possible activities that might have driven selection for better vocal abilities. Basically nothing is known about either musical or imitative abilities of Neanderthals, weakening any inference between vocal anatomy and language.
- The presence of speech, as discussed in Section 2.2 above, implies the presence of language, but not vice versa. As shown by sign language, it is perfectly possible to engage in communicative language use without speech, and it cannot be excluded that sign language (and thus our language

[5] Clegg (2004) argues that the risk is evolutionarily negligible, but according to statistics from NSC (2009), the number of choking deaths in present-day U.S. is around 4,000 per year, which is far from negligible, higher than the number of deaths from e.g., drowning or fires.

faculty) preceded speech in phylogeny (e.g., Armstrong *et al*. 1995 or Corballis 2002). Furthermore, if Chomsky (2010) were correct in his conjecture that internal use of language preceded externalization, this would likewise entail language preceding speech in phylogeny. The presence of apparent speech adaptations in a fossil species would thus provide at least some support for the presence of language in that species. But the converse does not apply; the absence of speech adaptations does not imply the absence of language.

- The absence of speech adaptations in the vocal apparatus doesn't even imply the total absence of *speech* — with a human brain in control, a non-adapted vocal tract would be quite adequate for simple speech (Fitch 2005) — much less the absence of language.

Taken together, these issues mean that anatomical adaptations in the vocal apparatus are not highly informative about the presence or absence of language. At best, the presence of clear speech adaptations would add some modest level of support towards inferring the presence of language in a form that required a rich repertoire of vocalizations with fine-grained distinctions. It would not be helpful in determining other features of that language, like whether it had syntax or not.

3.1. Speech Organs

The shape of the human vocal tract, notably the lowering of the larynx already in infancy, is very likely a speech adaptation. Speech would not be totally impossible even with an ape vocal tract, but it would be less expressive, with fewer vowels available (P. Lieberman 2008, de Boer & Fitch 2010). The 1:1 proportion between the horizontal and vertical part of the human vocal tract, together with our abilities to manipulate the width of both the horizontal and vertical part independently, broadens the spectrum of articulatory possibilities (Aiello 1998, Lieberman & McCarthy 1999, Lieberman 2007b), enabling us to produce more distinct speech sounds. The risks associated with the permanent lowering of the larynx are substantial, as noted above, implying substantial selective pressure behind the changes in the vocal tract; a strong selective pressure towards richer speech abilities is one plausible driver of this apparent optimization of the vocal tract for speech.

Having the larynx permanently lowered is commonly believed to be unique to adult humans, but some other mammals do possess a lowered larynx (Fitch 2009), notably big cats (Weissengruber *et al*. 2002). Furthermore, the vocal tract in many living mammals is quite flexible, and a resting position different from the human configuration does not preclude a dynamically lowered larynx, giving near-human vocal capabilities during vocalizations (Fitch 2009). The reason for the lowered larynx in other species likely has to do with the lowering of voice pitch, making the animal appear larger than it really is. This is a plausible explanation also for the additional lowering of the larynx occurring in human boys at puberty — human females do find a deeper male voice more attractive, according to Collins (2000) and Feinberg *et al*. (2004, 2005). But sexual

selection for a deep male voice is hardly a plausible explanation for the larynx descent in toddlers of both sexes, especially as sexual selection on the female voice is in the opposite direction, towards higher pitch. Human males prefer female voices with higher than average pitch (Feinberg 2008, Jones *et al*. 2008), which implies that sexual selection on females works against the observed larynx descent. Whatever caused larynx descent in humans must be potent enough to override sexual selection in females. Speech adaptation remains the most likely cause here.

The vocal tract itself is all soft tissue and does not fossilize, but its shape is connected with the shape of the surrounding bones: the skull base and the hyoid.[6] Already *Homo erectus* had a near-modern skull base (Baba *et al*. 2003), but the significance of this is unclear (Fitch 2000, Spoor 2000), as other factors than vocal tract configuration, notably brain size and face size (Bastir *et al*. 2010), strongly affect skull base shape.

Hyoid bones are very rare as fossils, as they are not attached to the rest of the skeleton, but one Neanderthal hyoid has been found (Arensburg *et al*. 1989), as well as two hyoids from likely Neanderthal ancestors, attributed to *Homo heidelbergensis* (Martínez *et al*. 2008). All these hyoids from the Neanderthal lineage are within the range of variation of the hyoid of modern humans, leading to the conclusion that Neanderthals had a vocal tract adequate for speech (Houghton 1993, Boë *et al*. 1999, Boë *et al*. 2007, Granat *et al*. 2007), but see also Lieberman (2007a) and Fitch (2009). The vocal tract of Neanderthals could probably not have precisely the 1:1 proportions between vertical and horizontal part of the modern human vocal tract, due to their longer oral cavity and slightly shorter neck (Lieberman 2007a), but as noted by Fitch (2005) this is not strictly needed for speech.

The hyoid of *Australopithecus afarensis*, on the other hand, is more chimpanzeelike in its morphology (Alemseged *et al*. 2006), and the vocal tract that Granat *et al*. (2007) reconstruct for *Australopithecus* is basically apelike.

A puzzling aspect of larynx evolution in humans is the loss of the laryngeal air sacs that most of our primate relatives have, and that are especially well developed in great apes (Nishimura *et al*. 2007). Many mammals have similar air sacs in a variety of locations, but their function is not well understood (Fitch 2006). The sacs almost certainly play a role in the vocal communication of apes, but little is known of the details, and other explanations remain tenable (Nishimura *et al*. 2007). A possible function of the sacs is hinted at by the fact that the sacs make it possible to produce vocalizations that are both louder and lower in frequency than the same animal could produce without sacs (de Boer 2008, Hombert 2010). Louder and lower vocalizations make the animal seem bigger than it is, which may be advantageous in many contexts. But this does not make it less puzzling why humans lost their sacs. Hombert (2010) proposes that the lowered larynx replaced the sacs, as an adaptation to the ecological change from forest to open terrain during our evolution.

According to Hewitt *et al*. (2002), the enhanced breathing control that has evolved in humans (see Section 3.3 below) may have made the sacs superfluous.

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[6] And also the cervical column, but its shape is not to my knowledge invoked as a speech indicator.

Nevertheless, the loss of a vocal adaptation in such a vocal species as ours is odd (Fitch 2000). Possibly the results of de Boer (2010), that the simulated presence of air sacs make vowels less distinct, may point in the right direction?

As far as the fossil record is concerned, the apelike shape of the hyoid bone of *Australopithecus afarensis* is interpreted as consistent with retained air sacs (Alemseged *et al*. 2006), whereas Neanderthals, as noted above, had a humanlike hyoid.

3.2. Hearing Organs

As noted by Barceló-Coblijn (2011), Johansson (2005), and many others, basic acoustic processing, including such seemingly speech-related aspects as formant perception, is widely shared among primates. As with other such shared features, their presence in Neanderthals is highly likely but not highly informative (cf. Section 2.3).

Some fine-tuning appears to have taken place during human evolution to optimize speech perception, notably our improved perception of sounds in the 2–4 kHz range. The sensitivity of ape ears has a minimum in this range, but human ears do not, mainly due to minor changes in the ear ossicles (Martínez *et al*. 2004), the tiny bones that conduct sound from the eardrum to the inner ear. This difference is very likely an adaptation to speech perception, as key features of some speech sounds are in this region. The adaptation interpretation is strengthened by the discovery that a middle-ear structural gene has been the subject of strong natural selection in the human lineage (Olson & Varki 2004). These changes in the ossicles were present already in the 400,000-year-old fossils from Sima de los Huesos in Spain (Martínez *et al*. 2004), which are likely to be Neanderthal ancestors. In the Middle East, ear ossicles have been found both from Neanderthals and from early *Homo sapiens*, likewise with no meaningful differences from modern humans (Quam & Rak 2008).

Hawks *et al*. (2007) and Hawks (2008) present evidence of ongoing adaptive evolution in several hearing-related genes in modern humans. The functional significance of these genes is, however, unknown, as is their state in Neanderthals. That selection is still ongoing nevertheless indicates a quite recent change in selective pressures on human hearing, which Hawks (2008) connects with a recent origin of language.

3.3. Neural Connections

Where nerves pass through bone, a hole is left that can be seen in well-preserved fossils. Such nerve canals provide a rough estimate of the size of the nerve that passed through them. A thicker nerve means more neurons, and presumably improved sensitivity and control. The hypoglossal canal, leading to the tongue, has been invoked in this context (Kay *et al*. 1998), but broader comparative samples have shown that it is not useful as an indicator of speech (DeGusta *et al*. 1999, Jungers *et al*. 2003). A better case can be made for the nerves to the thorax, presumably for breathing control (Fitch 2009). Both modern humans and Neanderthals have wide canals here, whereas *Homo ergaster* had the narrow canals

typical of other apes (MacLarnon & Hewitt 1999, 2004), indicating that the canals expanded somewhere between 0.5 and 1.5 million years ago.

3.4. Brain

The general size and shape of the brain and the gross anatomy of the brain surface can be inferred from well-preserved fossil skulls (Bruner 2004). Neanderthal brains are at least as large as the brains of modern humans, but distinct in shape, lower and longer, whereas AMH brains are larger in the parietal area (Bruner 2008). The functional significance, if any, of this shape difference is not well understood, and there is no consensus in the literature. Bruner cautions that some brain shape differences may be constrained by differences in the facial and basicranial skeleton, rather than driven by neurological changes, but notes that the parietal area is less constrained. Frontal widening around Broca's area is shared between Neanderthals and AMH, going back to 2 Mya (Bruner 2007), which adds some support for Neanderthal language. But other apes have brain structures with the same gross anatomy as both Broca and Wernicke (Gannon *et al*. 1998, Cantalupo & Hopkins 2001), so the support is not strong.

The rewiring of neural circuits within the brain that really could be informative about language does not leave any fossil traces. Barceló-Coblijn (2011) invokes two different kinds of neurons, *von Economo neurons* and *mirror neurons*, as possibly informative about Neanderthal speech, and at least the mirror neurons are commonly invoked in the context of language origins as well (e.g., Arbib 2012). But the arguments of Barceló-Coblijn are not persuasive, for several reasons:

- There is no direct evidence — but especially in the case of mirror neurons a deplorable amount of speculation and hype — supporting a central role for these neurons in the human speech system, or for that matter any other aspect of our language faculty.
- Both kinds of neurons are present in apes and/or monkeys, who do not have either language or speech. Their presence in a species can therefore not be used to infer the presence of language or speech (cf. Section 2.3):
  - The von Economo neurons are shared between humans and other great apes (Nimchinsky *et al*. 1999), and it thus follows directly from parsimony that Neanderthals most likely had them as well — no need to spend several pages arriving at that conclusion, especially as their presence is uninformative.
  - Mirror neurons are known to be present in monkeys, who do not have language or speech. Mirror neurons are *not* known to be present in humans (Lingnau *et al*. 2009, Turella *et al*. 2009), who do have language and speech. This does not add up to a strong case for mirror neurons having any role in either speech or language origins.

The relationship between these neurons and the human language faculty is an interesting enough topic in itself, once you get beyond the hype, but it is

irrelevant for the question of Neanderthal language. Any valid argument inferring speech or other aspects of language from neural features would have to be based on neural features that differ between humans and apes. The status of any such features in Neanderthals is unknown, apart from the gross brain anatomy mentioned above. Neural features that are shared between species with and without language remain uninformative.

In the traditional picture of language processing in the human brain, language is handled entirely in the left hemisphere, in a region centered on the classical areas identified by Broca and Wernicke in the 19th century. As shown by recent results from both neuroimaging and traditional lesion studies, this picture is a gross simplification, with language actually handled by much more complex networks spread out over a substantial fraction of the brain (Stowe *et al*. 2005, Fisher & Marcus 2006), including a modest level of right-hemisphere involvement (Fonseca *et al*. 2009). But the classical model does retain a kernel of truth, both in that the perisylvian region remains important, and in that language processing remains strongly asymmetric between the left and the right hemisphere, both quantitatively and qualitatively. Right hemisphere activity is commonly present in language tasks, but the left hemisphere is consistently *more* activated (see e.g., the numerous left-right image pairs in the review of Stowe *et al*. 2005). There is also a division of labor between left and right, in that core aspects of language like syntax, phonology and basic lexical semantics are strongly left-lateralized, whereas the right hemisphere plays a larger role in prosody, pragmatics, discourse handling, ambiguity resolution, non-literal meaning, and other ancillary tasks (Fonseca *et al*. 2009).

This lateralization of language processing may be connected with the anatomical asymmetries displayed by the human brain. But there is no clear-cut increase in general lateralization of the brain in human evolution — ape brains are not symmetric (Balzeau & Gilissen 2010) — and fossils are rarely undamaged and undistorted enough to be informative in this respect.

A possible alternative proxy for lateralization in the brain is handedness. Among apes there may be marginally significant handedness,[7] but nothing like the strong population-level dominance of right-handers that we find in all modern human populations. As language is handled by the same brain hemisphere as the dominant hand in most people, the rise of handedness and the rise of language may possibly be connected. For makers of stone tools, handedness can be inferred from asymmetries in the knapping process, the use-wear damage on tools, and also in tooth wear patterns (Uomini 2009, Frayer *et al*. 2010), which may provide circumstantial evidence of lateralization, and possibly language (Steele & Uomini 2009). Evidence for a human handedness pattern is clear among Neanderthals and their predecessors in Europe, as far back as 500,000 years ago (Frayer *et al*. 2010), and some indications go back as far as 1 million years ago (Uomini 2009). To what extent conclusions can be drawn from handedness to lateralization for linguistic purposes is, however, unclear.

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[7] This is a long-debated issue; see, for example, Palmer (2002), Humle & Matsuzawa (2009), Llorente *et al*. (2011), Hopkins *et al*. (2011). The only clear outcome is that the population-level handedness in apes, if any, is less than 2:1 or so, unlike the 8:1 or more that is typical for human populations.

3.5. Anatomical Conclusions

In conclusion, the fossil anatomical evidence indicates that at least some apparent speech adaptations were present in Neanderthals. No single one of these indications is compelling on its own, but their consilience strengthens the case for some form of Neanderthal speech. What little we know about Neanderthal brains is at least consistent with the presence of language, but the support is quite weak.

4. Genes

During the 1990s, DNA amplification methods developed to the point where even the minute amounts of DNA preserved in fossils could be recovered and sequenced. The range is still limited to the past 100,000 years or so — beyond that the DNA is too deteriorated to be recoverable — and also limited to cool climates, but that is adequate to put Neanderthals within reach, as well as cave bears and mammoths and other Ice Age fauna. Neanderthal DNA can be used both for inferring the precise relationship between Neanderthals and modern humans, and for determining if any language-related genes in modern humans are shared with Neanderthals. The language-related gene that has received the most attention is FOXP2 (see Section 4.2 below), but it is far from the only one; there is in general a substantial genetic contribution to human language abilities today, with many genes involved (reviewed in Stromswold 2010), but the genetic details are not well understood.

4.1. Neanderthal DNA Studies

Early genetic evidence from fossil mitochondrial DNA in Neanderthals clearly supported their separateness from *Homo sapiens*, and indicated that the last common ancestor lived at least 400,000 years ago (Krings *et al*. 1999, Höss 2000, Beerli & Edwards 2002, Knight 2003, Caramelli *et al*. 2003, Hodgson & Disotell 2008, Endicott *et al*. 2010), though the limited number of individuals tested made it impossible to exclude a modest level of admixture.

A draft sequence of the full Neanderthal genome was presented recently (Green *et al*. 2010), in which substantial similarities were found between the Neanderthal sequence and modern Eurasians. Green *et al*. (2010) interpret this as strong evidence of gene flow from Neanderthals into the common ancestor of modern Eurasians, around 100,000 years ago, but caution is in order as it is extremely difficult to exclude contamination with modern human DNA during excavation and processing of the fossils (Lalueza-Fox 2009). Wall & Kim (2007) found evidence of both severe contamination and other problems in earlier work by Green *et al*. (2006). And even if the DNA data are taken at face value, their interpretation depends on what assumptions are made about for example, ancient population structure. Eriksson & Manica (2012) show that the data are compatible with scenarios without interbreeding, whereas Sankararaman *et al*. (2012) instead find further support for interbreeding as recent as 37,000–86,000 years ago. Firm conclusions should await replication.

4.2. FOXP2

When mutations in the gene FOXP2 were found to be associated with specific language impairment (Lai *et al*. 2001), and it was shown that the gene had changed along the human lineage (Enard *et al*. 2002), it was heralded as a "language gene". But intensive research has revealed a more complex story, with FOXP2 controlling synaptic plasticity in the basal ganglia (Lieberman 2009) rather than language *per se*, and playing a role in vocalizations and vocal learning in a wide variety of species, from bats (Li *et al*. 2007) to songbirds (Haesler *et al*. 2004). Nevertheless, the changes in FOXP2 in the human lineage quite likely are connected with some aspects of language, even if the connection is not nearly as direct as early reports claimed. The deficiencies in people with the FOXP2 mutation appear to involve both motor-related speech problems, and problems with language itself (Fisher & Marcus 2006, Lieberman 2010). The language deficits appear similar to Broca's aphasia (Vargha-Khadem *et al*. 2007), which would indicate that FOXP2 is involved with syntax.

Relevant for the issue of Neanderthal language is that the derived human form of FOXP2 is found also in fossil Neanderthal DNA (Krause *et al*. 2007, but see also Benítez-Burraco *et al*. 2008, 2012, and Coop *et al*. 2008). According to Enard *et al*. (2002), the selective sweep driving that form to fixation was quite recent, less than 200,000 years ago, but Diller & Cann (2009) identify flaws in the analysis of Enard *et al*. (2002) and show that the sweep likely took place as much as a million years ago or more, well before the split between *Homo sapiens* and Neanderthals.

The FOXP2 gene generated too much excitement and hype when it was discovered. In biology in general, there is no such thing as *the* gene for a complex trait, with a single genetic change conferring language, *contra* for example, Chomsky (2010);[8] the relationship between genes and phenotype is much more complex and indirect (West-Eberhard 2003). But careful research has nevertheless produced a good case for FOXP2 being involved in the ontogeny of vocalizations in a variety of species, and for the changes in the human FOXP2 version having something to do with language. The presence of human FOXP2 in Neanderthals is by no means incontrovertible proof that Neanderthals had complex language (cf. Benítez-Burraco & Longa 2012), but it does add some additional weight to the case for Neanderthal language. But as FOXP2 apparently plays a role in both speech and syntax, the interpretation of its presence in Neanderthals is ambiguous.

As is well known, the gene sequence of protein-coding genes such as FOXP2 is by no means the sole determinant of the features of an organism. To begin with, most of our DNA is regulatory, involved in a complex network regulating the time and place of expression of the minority of protein-coding DNA (ENCODE 2012). Furthermore, all DNA expression both takes place in, and is

[8] "Evolution in the biological sense would then be restricted to *the* mutation that yielded the operation Merge along with whatever residue resists explanation in terms of the strong minimalist thesis, [...]" (Chomsky 2010: 61, emphasis added). Chomsky has sometimes argued that his postulated saltational origin is an abstraction and idealization (cf. Fujita 2009), but why then talk explicitly about single mutations?

instrumental in shaping, a developmental context, differences in which may have major effects on the actual developmental outcome (West-Eberhard 2003).

Concerning FOXP2, the expression pattern of this gene and its orthologs is similar across a wide variety of vertebrates (Fisher & Marcus 2006). Looking more generally at the expression patterns of many genes in the developing brain, the patterns are stable enough to be phylogenetically informative even within hominoids (Uddin *et al*. 2004), though some additional evolutionary changes have taken place within the human lineage, with a fairly small number of regulatory changes having major effects on brain development (Nowick *et al*. 2009). We have no direct information on Neanderthal gene expression patterns, but given the comparative evidence available, it would be both unparsimonious and purely speculative to propose, as Benítez-Burraco & Longa (2012) seem to do, that FOXP2 might do something totally different in Neanderthals than in modern humans.

5. Interbreeding or Not — Does It Matter for Neanderthal Language?

Neanderthals may or may not have been a separate species from us, but as already noted in footnote 1, I do not regard this as an interesting question in this context; the answer will depend as much on your choice of species definition as on the biological facts of the matter. But a related and more interesting question is whether and to what extent Neanderthals and AMH interbred and exchanged genes when they met, and whether this had any lasting genetic impact on the surviving human population — us.

As noted in Section 4.1 above, the genetic evidence is not unanimous, with multiple mitochondrial studies supporting separateness, but the single report of the full genome supporting interbreeding.

The fossil evidence is likewise not unanimous. On one hand, the last surviving Neanderthals appear 'pure', with no visible admixture of AMH features (Hublin *et al*. 1996, Hublin & Bailey 2006), and the earliest AMH in Europe more resemble Africans than Neanderthals, as observed in body proportions by Holliday (1997, 1999) and in general morphology by Tyrrell and Chamberlain (1998).

On the other hand, there is a report of a fossil find of a possible Neanderthal/AMH hybrid child in Lagar Velho, Portugal (Duarte *et al*. 1999, Zilhão 2002, Bayle *et al*. 2010), which would argue for a closer relationship between the two if its hybrid status were confirmed. Archaic features in some early modern human fossils in Europe may possibly also be interpreted as evidence of hybridization (Rougier *et al*. 2007, Trinkaus 2007, Soficaru *et al*. 2006). But a fossil with similar 'hybrid' features has also been found in South Africa (Grine *et al*. 2007) where hybridization with Neanderthals is unlikely.

What would the implications be for Neanderthal language if interbreeding took place? Or if it didn't?

- *No interbreeding*: No strong inference should be made from the lack of interbreeding *per se*; it may simply be the case that Neanderthals died out before AMH moved in, so that the two never actually encountered each other. Or they may have been reproductively incompatible for any number

of reasons unrelated to language. But if it is found that the preponderance of the evidence supports Neanderthal language, a lack of interbreeding would entail that we can infer from parsimony that language ought to have been present already in the common ancestor of Neanderthals and AMH, more than 400,000 years ago.

- *Interbreeding*: If the results of Green *et al*. (2010) are correct, a substantial amount of interbreeding took place. And since their results indicate the presence of Neanderthal genes among us today, a significant number of hybrids must have become reproductively successful members of the AMH population. Furthermore, since their results also indicate that Neanderthal genes are present in Eurasians but not in Africans, the interbreeding must have taken place after the time of the last common ancestor of all modern humans, which means that the AMH populations involved in interbreeding must have had a fully modern language faculty; cf. Section 1 above.

I would argue that it is highly unlikely that a person without language would be reproductively successful in a group where everybody else had language. This implies that the hybrids most likely had a functioning language faculty. It follows either that Neanderthals also had a language faculty, or that a genetic endowment heterozygous for the relevant genes is sufficient. The evidence from FOXP2 does not support the latter possibility, as the language impairment caused by FOXP2 mutations was identified in heterozygotes, but in other language-relevant genes the modern-human version may nevertheless be dominant.

Furthermore, the existence of the hybrids entails that a number of pure-bred Neanderthals found reproductive success with AMH partners.[9] What conclusions can be drawn from this? That depends on the mating system and social context of early modern humans, about which we have very little evidence. In a system with long-term pair bonds and a social context where both men and women are expected to be active participants in their peer groups, the Neanderthal partner would have to be socially accepted in the AMH group — unlikely without Neanderthal language abilities — but with a different mating system, for example a strongly patriarchal one, or just simply a Neanderthal and an AMH having sex (consensual or not) during a chance encounter in the forest, no social acceptance is necessarily entailed.

Evidence of successful interbreeding would thus add some modest weight to the case for Neanderthal language, despite some caveats about heterozygotes and mating systems. But it is not clear what form of language is supported. From modern human societies, there are plenty of examples of outsiders marrying into a group and becoming reproductively successful despite a rudimentary pidgin-level grasp of the community language; this may be taken as evidence that a capacity for proto-language is adequate.

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[9] If both the results of Green *et al*. (2010) and the mitochondrial studies are taken at face value, the interbreeding would have been between Neanderthal men and AMH women. But taking unreplicated results at face value would be imprudent; the evidence is nowhere near solid enough for such conclusions.

6. Archeology

Language use in itself is not archeologically visible, but other forms of symbol use may be visible, and may be used as indicators that some level of semiotic abilities had been reached. Invoking ancient art, including pigments and personal ornaments, as indicators that the artists were capable of symbolic thought, or even as an indicator that language had evolved, is fairly common (Mellars 1998, Henshilwood & Dubreuil 2009). Ceremonial burials and music are sometimes also considered. The precise connection between e.g., ornaments and specific forms of language is, however, not worked out in detail, and not well supported (Botha 2008).

Non-symbolic aspects of behavior have also been invoked as proxies for language, including the somewhat vague notion of 'modern human behavior', referring to the whole package of behavioral traces left by typical Upper Paleolithic populations. Some hunting techniques that require complex planning and mental time travel, such as snares and traps, may also be evidence of modern human cognition (Wadley 2010), and a possible proxy for the displacement characteristic of human language. Camps & Uriagereka (2006) and Balari *et al*. (2012) propose a specific connection between language and the ability to tie knots, as grammar and knot theory have the same level of computational complexity, but Lobina (2012) finds this unconvincing. The knots would in that case be a proxy specifically for syntactic abilities, unlike the other proxies discussed in this paper.

But any inferences from archeology to mental and cognitive abilities are fragile. Notably, all living human populations have effectively the same mental and cognitive abilities, including language, but there are vast differences in what kind of archeologically visible traces different cultures would leave behind. Some cultures produce large and salient artifacts in durable materials like stone; others have a material culture dominated by perishable materials like plant fibers. It would be an error to infer the presence of language only in the stone-using culture but not in the fiber-using culture.

A more proper methodology would be to consider the full range of archeological traces left behind by different cultures of known language users, and compare with the full range of archeological traces left behind by different species of non-language users. The archeology of known language users stretches from the minimal tool kits used by, for example, Tasmanian aborigines (Roebroeks & Verpoorte 2009), to the pyramids of Egypt and Maya. The archeological traces of known non-language users, such as non-human primates, are for the most part non-existent; chimpanzee nut cracking is an exception, as traces have been recovered both in pseudo-archeological excavation studies of known sites of chimpanzee tool use (Mercader *et al*. 2002, McGrew *et al*. 2003), and as real archeological finds of 4,300-year-old chimpanzee tools (Mercader *et al*. 2007). If the archeology of an unknown population falls within the range of modern human archeology, this would support the presence of language (and modern human cognition in general) in the unknown population, whereas if its archeology is no more complex than that of chimps, the absence of language is supported. Intermediate cultural complexity leaves the issue undecided.

This methodology may in principle be applied to any proposed archeological proxy for language, be it symbols or knots or just general tool-kit complexity. If the archeology of Neanderthals contains at least as much traces of the proxy as the minimal amount present in any modern-human archeological context, this would support the case for Neanderthal language.

But the issue of which potential archeological language proxies are actually valid indicators of language remains open and contentious. Instead of relying on any single one, a more prudent approach is to see if there is a common pattern to be found among multiple proxy-candidates.

6.1. The Revolution that Wasn't

The archeological record has frequently been invoked as support for the late, sudden appearance of language, due to the perception of a technological and creative revolution around 40–50,000 years ago (e.g., Binford 1989, Klein 1999, Bar-Yosef 2002, Li & Hombert 2002, Skoyles & Sagan 2002).

This was originally construed as a revolution taking place in Europe. The supposedly sudden appearance of advanced art and advanced tools in the caves of Europe about 40,000 years ago was often, and is still sometimes, taken as evidence of a cognitive leap (Klein 2008), indicating the origin of language. However, the appearance of a sudden dramatic 'cultural revolution' around 40,000 years ago has turned out to be largely an illusion caused by the former predominance of European sites in the documented archeological record, and possibly some Eurocentrism among archeologists (Henshilwood & Marean 2003). AMH did indeed invade Europe rather suddenly about 40,000 years ago, bringing along an advanced toolkit — but that toolkit had developed gradually over the course of more than 200,000 years (McBrearty & Brooks 2000, d'Errico *et al*. 2003, Van Peer *et al*. 2003, McBrearty & Tryon 2006, Marean 2010). Some aspects, such as blade technology (Johnson & McBrearty 2010), and possibly pigment use (Watts 2010), go back as far 500,000 years. Discoveries of works of abstract art (Henshilwood *et al*. 2002, Texier *et al*. 2010), pigment use (Barham 2002, Henshilwood *et al*. 2009, Watts 2009), and personal ornaments (Bouzouggar *et al*. 2007, d'Errico & Vanhaeren 2009, Henshilwood & Dubreuil 2009), all substantially older than 40,000 years, add further support to the long timescale of McBrearty & Brooks (2000). The African evidence is mostly from the southern part of the continent, but some finds have also been made in North Africa (d'Errico *et al*. 2009a).

There was indeed a substantial and rapid increase in the *frequency* of modern human behavior in the Early Upper Paleolithic, but that increase may well be caused by demographic factors (Zilhão 2007, Cartmill 2010) rather than any cognitive changes. Norton & Jin (2009) suggest that symbolically organized behavior may not become evident until the population is dense enough to require group-distinguishing ornaments.

The notion of a biologically based sudden late revolution is untenable, not only because of the earlier record in Africa, but also because the proposed time postdates the last common ancestor of modern humans, and people were already dispersed over most of the world at that time. Even if the revolution were purely cultural, it would still have taken a significant amount of time for any cultural

innovation to spread to the whole population (Raimanova *et al*. 2004).

But the origins of symbol use and other modern behaviors can also not be regarded as a straightforward 'Out of Africa' matter (d'Errico *et al*. 2009b, Conard 2010), with modern behavior originating together with modern anatomy in Africa and then being carried with the AMH exodus. As reviewed in the next section, there is quite enough evidence of early modern behavior among Neanderthals to make such a simple model untenable.

Also in Asia, there is some evidence for a gradual origin of modern behavior and continuity between Middle and Upper Paleolithic industries, notably in Siberia (Derevianko 2010), where the Denisova hominin was found (Dalton 2010, Krause *et al*. 2010, Yuan & Li 2010). In Denisova a number of ornaments were found in the same strata as the hominin remains, including a beautifully worked bracelet (Derevianko *et al*. 2008), in a largely Mousterian-like context.[10] Likewise in South Asia there is no discontinuity marking the arrival of modern behavior (James & Petraglia 2005). The relevant record in East Asia is plagued by dating uncertainties, but symbolic behavior may go back as far as 130 kya (Norton & Jin 2009). Some evidence of early pre-40k symbolic behavior has also been found in the Middle East (Mayer *et al*. 2009). Many aspects of the 'behavioral modernity' package are thus found not just in the Upper Paleolithic, but also in the earlier Middle Paleolithic record in Africa, Europe, and Asia (d'Errico *et al*. 2003).

There is also possible evidence for simple art that actually predates the appearance of both Neanderthals and AMH (Marshack 1997, Bednarik 2003), in Acheulean cultural contexts. But all such finds are highly contested, and there is no consensus on whether they are art, or just rocks with a funny shape.

6.2. Neanderthal Culture and Mind

The extent to which Neanderthals had a human mind, human cognition, and human culture remains controversial — see Moro Abadia & Gonzalez Morales (2010) for a discussion of the conceptual and definitional issues involved. Throughout most of their existence, Neanderthals used Mousterian tools, a Middle Paleolithic industry comparable in archeologically visible complexity both with Middle Paleolithic *sapiens* in Africa, and with recent Tasmanian aborigines (Roebroeks & Verpoorte 2009). Late Neanderthals are associated with the Châtelperronian culture, a Middle-Upper Paleolithic "transitional" industry. There is fairly good evidence of Neanderthals using adhesives for hafting tools (Mazza *et al*. 2006, Pawlik & Thissen 2011, Cârciumaru *et al*. 2012). The adhesives themselves are non-trivial to make, requiring a multi-step process involving careful control of temperature, implying sophisticated Neanderthal cognition (Koller *et al*. 2001). Furthermore, in known Stone Age cultures hafting typically does not rely on adhesives alone, but also on tying, which requires knots — cf. the arguments of Camps & Uriagereka (2006) — but there is no direct evidence of knot-making among Neanderthals.

[10] According to Zilhão (2010), the stratigraphy in the cave is disturbed, making the context of the bracelet uncertain.

Different exploitation patterns of the fauna have been invoked as indicators of cognitive differences, with both specialization and diversification contradictorily taken as signs of modern behavior by different authors, with little consensus on how exploitation patterns should be interpreted (Schepartz 1993). There is in any case no strong evidence that exploitation patterns actually differed significantly between Neanderthals and coeval AMH (Britton *et al*. 2011).

There is some evidence of ceremonial burial of (and presumably by) Neanderthals (Trinkaus & Shipman 1993; d'Errico *et al*. 2003, 2009b), which might imply a human level of awareness of self and others, but the evidence is still contested (Gargett 1999, Davidson 2003), and there is no clear evidence of grave offerings in Neanderthal graves — whereas among Upper Paleolithic AMH ceremonial burials are both common and unambiguous (Formicola *et al*. 2001).

Neanderthals apparently cared for their elderly and crippled members, as fossils have been found of Neanderthals with healed injuries and chronic diseases, who must have lived for years with impaired ability to fend for themselves (Trinkaus & Shipman 1993, Lebel *et al*. 2001). Similarly, one pre-Neanderthal child with a disfiguring birth defect survived for several years about half a million years ago (Gracia *et al*. 2009, Hublin 2009). There have, however, also been several cases reported of apes with similar disabilities surviving in the wild for extended periods (Hublin 2009). Are the other apes compassionate enough to help, or are the disabilities not as disabling as we think? The inference from surviving cripples to behavioral modernity may not be reliable; see also the counterarguments of DeGusta (2003) and Cuozzo & Sauther (2004).

Fragments of what may have been musical instruments have also been found associated with Neanderthals (Turk *et al*. 1995), but d'Errico *et al*. (2003) argue that the Neanderthal 'flutes' are most likely just carnivore-damaged bones.

A stronger case for Neanderthal symbolic behavior can be found in their artefacts. Objects of the same type that are interpreted as evidence of symbolic behavior when found in an AMH context, have also been found associated with Neanderthals in Europe (Zilhão 2007, Langley *et al*. 2008, d'Errico 2008, d'Errico *et al*. 2009b, Watts 2009, Zilhão *et al*. 2010, Peresani *et al*. 2011, Morin & Laroulandie 2012), as well as objects that can reasonably be interpreted as art, mainly engravings (Appenzeller 1998, d'Errico *et al*. 2003, Wynn & Coolidge 2004, d'Errico *et al*. 2009b). There are no cave paintings that can be unambiguously assigned to Neanderthal artists, but the recent dating results of Pike *et al*. (2012) are intriguing in this respect; one painting in Spain has a minimum age beyond 40,000 years, which is just barely compatible with the arrival of AMH in the region but a more comfortable fit with the residence period of Neanderthals.

Ornaments appear to be mainly a fairly recent development among late Neanderthals, mostly found in Châtelperronian contexts. Whether Neanderthals may have copied such modernities from encroaching AMH (Mellars 2005) or developed them independently (d'Errico 2003) has been debated, and Higham *et al*. (2010) question the dating of some of these Châtelperronian finds, casting some doubt on the Neanderthal association (Mellars 2010), but see also the response by Caron *et al*. (2011). And there are also finds, notably the painted shells of Zilhão *et al*. (2010), that are unaffected by these critiques, as they predate the arrival of AMH by a fair margin. Pigment use by Neanderthals likewise goes

back at least 200,000 years (d'Errico *et al*. 2009b). Unlike the predominantly red pigments used by their contemporaries in Africa, the ones found in Neanderthal contexts are mainly black. Possibly this can be connected with the likely whiter skin color of Neanderthals compared with African *Homo sapiens*, making black pigment more salient against pale Neanderthal skin (Gilligan 2010).

These finds are simpler and less frequent than the ornaments and figurative art of later Upper Paleolithic AMH in Europe (e.g., Bahn & Vertut 1997 and Conard 2003). However, the apparent symbol explosion in Aurignacian Europe is not echoed by AMH everywhere. Some undeniably modern human populations (e.g., Tasmanians) left archeological records resembling those created by Middle Paleolithic Neanderthals (Roebroeks & Verpoorte 2009). The Australian archeological record in general shows a remarkable shortage of the kind of evidence taken as indicators of modern behavior (Brumm & Moore 2005), from which it can be concluded that demographic, ecological and energetic factors, rather than cognitive differences, may explain the sparseness of the Neanderthal artistic record. It can also be concluded that sparseness of archeological evidence for modern cognition is not evidence of absence.

Sparse as they may be, the traces of symbol use among Neanderthals nevertheless exist, and are adequate to infer a symbolic capacity comparable to that of coeval AMH (d'Errico *et al*. 2009b), and well within the range of archeologically visible symbol use by known language-using cultures. This adds substantial support to the case for Neanderthal language in some form, consistent with the anatomical evidence. More specifically, the archeological evidence of symbol use would entail the presence of an ability to handle symbols and map them to meanings, which would support the presence of lexical semantics which requires similar abilities, but does not tell us anything about syntax (cf. Botha 2009).

7. Conclusion

Both fossil and DNA evidence of speech adaptations in Neanderthals or earlier hominins, and archeological indications of symbolic behavior in Neanderthals, support the presence of some form of Neanderthal language. Note, however, that the anatomical evidence outside the brain concerns proxies for speech, whereas the archeological and to some extent the genetic evidence concerns proxies for features of language. These are not synonymous, as discussed in Section 2.2. Language need not have started in a spoken modality; sign language may have been the original language (e.g., Corballis 2002). The presence of speech supports the presence of language, but not vice versa.

My conclusion concerning the speech capacities of Neanderthals is basically in agreement with that of Barceló-Coblijn (2011), though he expresses it as "Neandertals were probably able of vocalizing voluntarily, with communicative intentions and in a sophisticated way" (2011: 286) and "had a physical structure which would not disable them in order to emit articulate sounds, very similar to that we modern humans produce when talking" (2011: 322) instead of calling it 'speech', and our routes to that conclusion differ somewhat.

It remains a controversial issue whether the Neanderthals were actually a

separate lineage, or just a subspecies of *Homo sapiens*, and whether there was any significant admixture when modern humans replaced Neanderthals in Europe (Herrera *et al*. 2009, Sankararaman *et al*. 2012), or possibly earlier (Green *et al*. 2010). But a modest amount of admixture near the time of Neanderthal extinction would not materially affect the argument here, as both the speech adaptations and the "symbolic" archeology predate the proposed time frame for admixture, and at least the speech adaptations likely predate even the common ancestor of Neanderthals and AMH. And if Neanderthals and modern humans did mix and interbreed freely at some point, so that Neanderthals were assimilated rather than replaced, this in itself would argue against any major differences in cognitive and communicative abilities.

While the evidence I have reviewed does indicate the likely presence of language in some form in Neanderthals, Barceló-Coblijn (2011: 322) does not draw the same conclusion. This may be because he did not review the archeological evidence, and because his article focused mainly on speech, but also because of his stated belief that "these [syntactic/morphological] capabilities are essentially bound to computational capacities proven until now only in *H. sapiens*". But the fact that it is only in *H. sapiens* that such capacities have been proven *among extant species* is not informative of the capacities of the non-extant Neanderthals, and should not be used as an argument against Neanderthal language.

But even though there is enough evidence to conclude that Neanderthals likely had some form of language, there is little evidence indicating just how complex their language may have been. The evidence from speech indicates that they likely had a spoken language that was complex enough to require fine-grained vocal distinctions. The evidence from symbolic archeology indicates that they had the capacity to handle symbols, which supports the presence of lexical semantics. The interbreeding data is inconclusive, but if the genetic data supporting interbreeding is confirmed, this would support the presence of at least proto-language in Neanderthals. There is no real evidence one way or the other concerning syntactic abilities. Taken together, this means that Neanderthals had at least a spoken proto-language; whether they had syntactic language can be neither confirmed nor refuted.

As discussed in Section 2.2 there is no consensus in the linguistic literature on whether language is an all-or-nothing affair (e.g., Piattelli-Palmarini 2010), or whether intermediate stages of proto-language are possible (e.g., Johansson 2005, Mithen 2005, Bickerton 2009). As noted above, a fair case can be made for Neanderthal language being complex enough to require fine-grained vocal distinctions, and rich enough to support at least a modest symbolic culture. This in itself need not imply anything like modern grammar; a proto-language like that envisioned by for example Bickerton (2009) is quite enough to explain the Neanderthal data. But if Piattelli-Palmarini (2010) were correct in that no intermediate proto-language is possible, that either you have unbounded Merge or you don't have language at all, this would entail that Neanderthals most likely did have a fully human language faculty, as the total absence of any form of Neanderthal language is difficult to reconcile with the data.

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★ FORUM ★

Genetic Factors and Normal Variation in the Organization of Language

Roeland Hancock & Thomas G. Bever

1. Introduction and Summary

In this essay we present two themes. The first is a factual review of the behavioral and neurological differences in language and cognition between people with and without familial left handedness: These differences begin to justify the claim that there is a continuum of how language and cognition are represented in the brain, reflecting a quantitative difference in the role of the right hemisphere, and consequent potential qualitative differences.

The second theme involves the implications of this finding. Various cases of rare neurological organization for language have called into question the idea that there is a single form of representation: These include cases of left-hemispherectomy in which the patients with a lone right hemisphere can grow up to be normal linguistically (Curtiss *et al*. 2001, Devlin *et al*. 2003) with normal developmental stages (Curtiss & Shaeffer 1997) as well as unique instances such as the infamous formerly hydrocephalic mathematician whose neocortex was a thin layer of tissue lining the skull (Lewin 1980) — clearly the topology and connections of different cortical areas are very different in these cases from the norm. Even classic and recent studies call into question the unique location and function of a linguo-central structure such as Broca's and Wernicke's areas (Penfield & Roberts 1959, Bogen & Bogen 1976, Anderson 2010, Rogalsky & Hickok 2011). But people with familial left-handedness comprise 40% of the population, so we cannot consign their unique behavioral and neurological structures to an odd distaff 'minority'.

A profound implication for language of these considerations is the possibility that the existence of language is not causally dependent on any particular unique neurological organization. Rather, especially the sentence construction mechanism of syntax is a computational type that recruits different neurological structures. On this view the possibility for syntax emerges as a function of the availability of propositional relations, combined with an explosive growth in the number of lexical items that can externalize the internally represented categories. The syntactic computational architecture is represented neurologically via co-option and integration of multiple brain regions that are collectively suited to the type of computation that language requires. On this view, there can be significant lability of how language will be represented in an individual's brain, if there is significant variability in how the computationally relevant areas function or are interconnected.

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We will show that there is significant variability in functional cortical organization differentiating individuals with familial sinistrality (FS+) and those without (FS–), along a continuum of genetic effects associated with left-handedness. The steps in this argument are first to review some general considerations about the basis for cerebral asymmetries for higher functions in general; we then turn to a brief review of current evidence that people with and without familial left-handedness (sinistrality) have characteristic differences for language behavior — people with familial left-handedness appear to access lexical items more readily than other; these behavioral differences are reflected in some recent studies of brain imaging, which show both qualitative and quantitative effects. We suggest that the apparent qualitative effects may result from the quantitative differences, but leave open the possibility that the qualitative differences reflect real differences in how language is realized neurologically.

2. Cerebral Asymmetries — Computational Differences in the Hemispheres and Alternative Explanations

A number of cognitive functions and processing characteristics have been traditionally ascribed to a specific cerebral hemisphere, notably verbal, relational and/or sequential processing in the left and nonverbal, spatial, prosodic, and holistic processing in the right. It has become increasingly clear that these dichotomies are not entirely accurate and show considerable individual variability. Since substantial and quantitatively predictable shifts from left hemisphere language processing may be present in up to 40% of the general population, consideration of these differences is not only theoretically illuminating but also relevant to biolinguistic research programs in practice. That is, these individual differences in hemispheric specialization can provide considerable insight into the computational flexibility of the brain and computational options of how behavioral interfaces might be represented. We first review some of the more reliable evidence for moderate hemispheric specialization in several cognitive and linguistic domains in relation to theories of lateralization. Then we discuss the relation between functional and behavioral asymmetry and evidence for a common genetic influence. Finally, we consider how variable cerebral asymmetries may inform theories of syntax–semantics and syntax–phonology interfaces.[1]

It is uncontroversial to assume that hemispheric specialization is largely driven by the anatomical separation of the brain by the longitudinal fissure and interconnecting axon tracts — the corpus callosum, connecting cortical areas and the anterior commissure, connecting subcortical areas. The corpus callosum is a dense tract of approximately 200 million topographically organized axons

[1] Some theories of cerebral lateralization hinge exceptionally on a close connection between language and motor function, for instance suggesting that the population-level bias for right-handedness (from e.g., mother-child bonding or a hand-to-hand combat advantage) and an essential link between fine motor skill in speech (for articulation) ultimately establishes a left hemisphere bias for language processing (e.g., Jonas & Jonas 1975). We omit a discussion of such theories since they lack both evidence and utility, instead focusing on lateralization theories that address the connection between brain function and computational properties.

connecting the two hemispheres, with many of the fibers dedicated to motor cortex. The corpus callosum introduces a significant conduction delay in interhemispheric communication since the human brain is larger than other primate brains, but axons in the human corpus callosum are not proportionally larger (Olivares *et al*. 2001). Morphological changes in the corpus callosum diameter have been associated with left-handedness (Dunham & Hopkins 2006), developmental (Duara *et al*. 1991, Hynd *et al*. 1995) and psychiatric disorders (see Innocenti *et al*. 2009 for a review).

Ringo *et al*. (1994), among others, proposed a 'callosal distance hypothesis', that interhemispheric communication delays through the corpus callosum are critical to functional lateralization, particularly in large brains where conduction delays may be on the order of tens of milliseconds (Aboitiz *et al*. 2003). This theory receives substantial support from a range of neuroanatomical evidence and computational models. Using simple self-organizing neural networks, Levitan and Reggia (Levitan & Reggia 1999, 2000; Shevtsova & Reggia 1999) demonstrated that lateralization of self organized neural maps, akin to functional specialization, depended on the properties of a connecting simulated corpus callosum, in addition to the number of neurons each hemisphere and their dynamical properties (e.g., excitability). Many fibers in the corpus callosum are thought to be inhibitory, such that a functional bias in one hemisphere will inhibit recruitment of corresponding regions in the opposite hemisphere for the same function.

Empirically, recent studies suggest that a measure as simple as overall skull size can predict the strength of behavioral asymmetries — smaller brains have less lateralization for language (but not visual processing) than large brains, as would be predicted by the callosal distance hypothesis (Tzourio-Mazoyer *et al*. 2010). This distinction between language and vision is also consistent with connectionist models that suggest the degree of hemispheric specialization may depend on the complexity of the task, showing greater emergent unilateral involvement for complex tasks (e.g., language; Monaghan & Pollmann 2003). The callosal distance hypothesis thus provides a clear neuroanatomical mechanism for establishing functional cerebral asymmetry. In the next section, we consider possible mechanisms for a left-hemisphere bias in language lateralization.

2.1. Processing Speed and Relational/Holistic Processing

Language is arguably the most complex of cognitive processes, requiring rapid analysis and integration at multiple levels of complex structure to support natural speech. Thus, many historically important ideas about cerebral asymmetries start with language as the critical case. In the late 19th century, Hughlings Jackson suggested that the basis for language asymmetries is not modality-specific, but rather follows from an essential difference between the hemispheric 'styles' of processing: The left hemisphere is 'propositional', while the right hemisphere is 'associative' (Hughlings-Jackson 1878, 1879). Bever (1975) reformulated this as a more general distinction between 'relational' and 'holistic' processing, relating it to the relative number of representations that are integrated at one time. Bever (1980) suggested that the basis for such an essential difference could be resolved to a (potentially very small) left hemisphere superiority in computational power — in conjunction with a developmental lateralization process involv-

ing complementary inhibition, this computational difference would produce the observed left hemisphere superiority for relational processing. Relational processing requires at least two distinct representations to be stably maintained in order to be interrelated: By definition, this involves more representations than holistic processing, which can be processed one representation at a time.

Greater 'computational power' in the left than right hemisphere could be reflected in anatomic asymmetries that might suggest more computational power. An obvious candidate for this is the asymmetry of the planumtemporale, a region of auditory cortex that is often substantially larger in the left hemisphere than the right. Early (around 30 weeks gestation) anatomical asymmetries of the planumtemporale (Geschwind & Levitsky 1968), in addition to more developed cortical folding in the right hemisphere partially motivated the Geschwind & Galaburda (1985) theory of cerebral asymmetry. The Geschwind-Galaburda theory rests on the apparent developmental origin of cerebral asymmetry — namely that the left hemisphere matures more slowly than the right — and the hypothesis that rate of maturation is mediated by the intrauterine environment. Noting that left-handedness is more prevalent in males, Geschwind and Galaburda hypothesized that testosterone is a key environmental factor that influences the maturation of the fetal brain. This hypothesis also offers an explanation for (Geschwind & Behan 1982, 1984) and others' finding that immune and developmental disorders are more prevalent in left-handers and individuals with familial left-handedness.

The testosterone hypothesis has broad implications beyond cerebral asymmetries that could support it: They note that the development prevalence of asthma in each sex reverses around puberty (from being more common in males during childhood to more common in post-pubescent females), consistent with an immune-testosterone link. However, Vink *et al*. (2010) found no association between hormone levels and the change in the prevalence of asthma in a large Dutch sample and suggested that other sex-dependent developmental factors, such as differential lung development or obesity, may account for the reversal. While not fatal to the Geschwind-Galaburda theory, this evidence undermines a major feature of the theory, suggesting that non-hormonal sex effects may be responsible for the apparent link between left-handedness and immune disorders.

Using hemispheric neural network models, Shevtsova & Reggia (1999) found that lateralization is biased towards larger networks, consistent with the Geschwind-Galaburda theory. On the other hand, there are well-known examples of neural growth in response to usage, even in adults (Maguire *et al*. 2000). Since fetal hearing develops as early as 24 weeks (Birnholz & Benacerraf 1983), some six weeks before planumtemporale asymmetry emerges, it is plausible that this asymmetry reflects an earlier predisposition for left-hemisphere language and/or auditory processing.

Aside from differences in the anatomical size of specific brain structures, a number of empirical hypotheses suggest that the functional basis for a computational superiority of the left hemisphere could be increased processing speed. Differential processing speed between cerebral hemispheres, has been proposed as an adaptive energy-conserving mechanism that will naturally arise when a split neural network must support at least one highly demanding task while

minimizing energy consumption (Grushin 2005). Empirically, some support has been found for the view that a deficiency in processing speed may explain certain cases of Specific Language Impairment behaviors (Leonard *et al*. 2007).

Poeppel (2003) has focused on differences in processing speed for auditory input in particular: He suggested that language lateralization is related to intrinsic differences in hemispheric specialization for processing auditory input on different timescales. On his 'asymmetric sampling in time' (AST) hypothesis, initial auditory processing occurs bilaterally, with later resolution of auditory input into laterally distinct timescales of information integration: a short (20-40ms; gamma EEG frequency band) time window more prominent in the left hemisphere and a long (150-250ms; theta band) window more prominent in the right hemisphere. These timescales reflect two components of the speech signal: rapid spectral changes associated with formant information and slower spectral changes associated with prosodic information. To provide support for this hypothesis Poeppel and colleagues have shown greater gamma band activity in the left than right auditory cortex and greater theta band activity in the right auditory cortex at rest (Giraud *et al*. 2007). Boemio *et al*. (2005) also found greater sensitivity to long (>85ms) frequency modulated segments in the right superior temporal sulcus than the left. Gamma band oscillations have been suggested to have a critical role in the binding problem (e.g., Engel *et al*. 1991, Miltner *et al*. 1999, Uhlhaas *et al*. 2010). Since a 'faster' hemisphere might be expected to bind representations more rapidly, increased gamma activity in the left hemisphere could be consistent with differential processing speed.

Greater processing speed could be the result of a larger number of parallel computation units (e.g., more neural assemblies), faster low-level processing (e.g., at a higher oscillator frequency, say gamma vs. theta) or an increase in the efficiency of processing and reduction in the time needed to converge to a stable state in a neural network. We now consider an alternative to the processing speed/capacity as the sole explanation for the hypothesized computational superiority of the left hemisphere: The critical distinction between the hemispheres may be the relative level of neural noise in processing that results from functional differences. Conceptually, if noise effects each representation equally, the effect on relational tasks involving many simultaneous representations will increase geometrically, compared with minimal effect on holistic tasks involving only one representation at a time.

The signal-to-noise ratio (SNR) describes the power of a stimulus-induced electrophysiological response to the non-induced fluctuations around the signal (not to be confused with the ratio of induced signal power to resting activity). More generally, this is the fidelity with which an encoding signal can be reproduced. SNR can be non-invasively measured with EEG by comparing the average evoked response to some repeated stimulation to the level of trial-by-trial variability (Möcks *et al*. 1988). SNR has important theoretical implications for neural processing, for instance bounding the information capacity of a channel, under certain assumptions (Shannon & Weaver 1949), the memory capacity of a neural population (Ganguli *et al*. 2008) and coupling properties of neural oscillators. Thus, it is directly relevant that SNR has already been shown to be higher in the left hemisphere than the right (Winterer 1999).

High SNR is generally considered to be a desirable property for computational efficiency. However, the optimal level of SNR, may not always be the highest, but can be intermediate. In the brain, neural noise has been proposed as beneficial to neural processing by inducing stochastic resonance. Stochastic resonance is the slightly counterintuitive phenomenon in which noise actually increases SNR by essentially lowering the neural response threshold, thus increasing sensitivity (and response to) to low amplitude signals. For example, in the human visual system, monocular subthreshold visual stimulation was found to evoke changes in scalp potentials when accompanied by visual noise (to the other eye, but overlapping visual field), suggesting that the brain does benefit from stochastic resonance, at least in sensory processing (Mori & Kai 2002). Language processing is poorly described in terms of low amplitude thresholds at this level of abstraction, but neural computation properties such as SNR do have relatively direct relevance for dynamical systems models of language acquisition (Andrews 2003, Hancock 2009) and processing (Tabor & Tanenhaus 1999, Tabor & Hutchins 2004) and general theories of binding in vision science and linguistics. Conceptually, language may be preferentially lateralized to the hemisphere having (close to) a computationally optimal SNR.

Hemispheric differences in the modulatory action of dopamine in the cortico-striatal-thalamic loop present a possible and theoretically appealing source of SNR asymmetry. The corticostriatal loop, consisting of largely ipsilateral parallel 'direct' and 'indirect' pathways within the basal ganglia, plays a key role in most aspects of cognitive processing and motor control. Thus, asymmetries in this pathway may have broad effects on functional lateralization and an intrinsic link to motor lateralization. The dominant types of striatal dopamine receptors, D1 and D2, have excitatory and inhibitory modulatory effects on cortical projections, respectively, and together provide a contrast enhancement mechanism affecting cortical SNR.

We propose that a left-right asymmetry in D2 receptor activity produces a corresponding asymmetry in cortical SNR, thus affecting the degree to which noise-sensitive cognitive processes are lateralized. Several lines of indirect evidence support this hypothesis:

1. In normal, right-handed adults, there is evidence of greater D2 activity in the left striatum than the right (Larisch *et al*. 1998, Vernaleken *et al*. 2007).
2. Dopamine activity, especially D2 activity, is known to bias motor activity (e.g., turning preference) in animals (Giorgi & Biggio 1990) and may be similarly linked to hand preference in humans (Mohr & Lievesley 2007, Mohr *et al*. 2003)
3. Dopamine D2 function is closely linked to a number of psychiatric disorders that have been linked to non right-handedness and reduced cerebral lateralization (e.g., Abi-Dargham *et al*. 2000).
4. The major candidate gene associated with handedness (LRRTM1) is notably expressed in the human striatum, where it may interact with dopaminergic synapses.

While non-dopaminergic neural changes could produce similar effects (e.g., increased cortical excitability as proposed in ADHD), these effects would not be expected to have the same close link to motor lateralization.

3. Familial Handedness and Heritable Variation in the Neurological Representation of Language

The usual reasons in today's science to study genetic effects on language is to demonstrate evidence that language is 'innate' in some interesting sense, that differentiates it from heritability of 'general cognition', 'communicative capacity' and so on. Thus, there are alleged investigations of spared syntactic capacity in Williams' Syndrome children (Bellugi *et al*. 1994, Clahsen 1998, Zukowski 2004), as well as children with severe motor disabilities: Conversely, there are forms of selective impairment of language in, for example, Turners syndrome (Curtiss 2012) and 'FoxP2' (Watkins *et al*. 2002) children. In each of these cases, the usual method (in principle) is to isolate a particular genetic abnormality, and relate it to the selective sparing or selective impairment of language ability, thereby making more specific the claim that language is 'innate'.

In our behavioral research of many decades and recent neurolinguistic studies, we have adopted a different method to provide converging information about the heritability of how language is used and represented. In particular, we have tracked the effects of familial left-handedness in right-handers. Many thousands of questionnaires have shown that about 40% of all college students are right-handers with familial left-handedness, and an equal percentage of right-handers without familial left-handedness. Thus, we can use familial handedness as a tool to explore differences in how language is used and represented in two equally large 'normal' populations. Of course, there is no guarantee that there is any effect, any more than would be found by differentiating people by height. But we have in fact found significant effects of familial handedness: This presumably is mediated by differences in neurological organization, particularly asymmetries, as affected by genetic tendencies towards cerebral symmetry, even in phenotypic right-handers. Below we review some findings from others as well as our research.

Loss of linguistic ability results from damage to specific areas of the left neocortex. The fact that normal language depends on (rather small) specific areas suggests that it may be critically 'caused' by those areas. However, certain aspects of language may have considerable latitude in their neurological representation. For example, Luria (1970) and colleagues noted that right-handed patients with left-handed relatives (FS+) recover faster from left-hemisphere aphasia, and show a higher incidence of right-hemisphere aphasia than those without familial left-handers (FS–). They speculated that FS+ right-handers have a genetic disposition towards bilateral representation for language, which often surfaces in their families as explicit left-handedness. In individuals of both left- and right hand preference, familial sinistrality may account for some of the variability seen in aphasia symptoms (Ettlinger *et al*. 1956, Subirana 1958) and language symptoms in individuals with unilateral lesions (Hécaen *et al*. 1981). Individuals with crossed aphasia show symptoms similar to those with left-hemisphere damage, but have a higher incidence of FS+ (Coppens *et al*. 2002).

We have found a consistent behavioral difference between the two familial groups in how language is processed, which may explain Luria's observation. Normal FS+ people comprehend language initially via individual words, while FS– people give greater attention to syntactic organization. A simple demonstration is that FS+ people read sentences faster and understand them better in a visual word-by-word paradigm than a clause-by-clause paradigm: The opposite pattern occurs for FS– people. Another example is that if words in a short essay alternate in isolation between the ears at a normal rate, FS+ people understand the essay better than if the words are presented all monaurally: The converse is true for FS– people. Bever *et al*. interpreted this as a result of the relative segregation of each word from the adjacent ones in the alternating ear condition, making it easier for FS+ people to recognize each word separately (these studies and others are reported in Bever *et al*. 1987, Bever 1988). In another set of studies, Townsend and colleagues reported that recognition of an auditory probe word from a just-heard sentence fragment is faster in FS+ people than FS– people, while the latter are more sensitive to the overall grammatical structure of the sentence fragment (main vs. subordinate clause; Townsend *et al*. 2001).

The bilateral representation of language in FS+ people may be specific to lexical knowledge, since acquiring that is less demanding computationally than syntactic structures, and hence more likely to find representation in the right hemisphere. On this view, FS+ people have a more widespread representation of individual lexical items, and hence can access each word more readily and distinctly from syntactic processing than FS– people (Bever *et al*. 1987, Bever *et al*. 1989). This hypothesis would explain the relative ease of processing lexical items in FS+ people.

This leads to a prediction: Lexical processing is more bilateral in FS+ right-handers than FS– right-handers, but syntactic processing is left-hemisphered for all right-handers. Recently, we tested this using fMRI brain imaging of subjects while they are re-ordering word sequences according to syntactic constraints or according to lexico-semantic relations between the words. We found suggestive evidence that the lexical tasks activated the language areas bilaterally in FS+ right-handers, but activated only the left hemisphere areas in the FS– right-handers: All subjects showed strong left-hemisphere dominance in corresponding syntactic tasks (Chan 2007). This confirms our prediction, and supports our explanation for Luria's original clinical observations. It also demonstrates that there is considerable lability in the neurological representation of important aspects of language.

Familial handedness and the critical period offers further evidence for the genetic differentiation of language representation. The notorious case of the critical period is syntactic knowledge of an explicit language, which is neither determined by sensory/motor learning nor related directly to universals of thought. Bever has argued that the critical period for syntax learning is a natural result of the functional role that syntax plays in learning language — namely, it assigns consistent computational representations that solidify perceptual and productive behavioral systems, and reconciles differences in how those systems pair forms with meanings (Bever 1975, 1981). On this view, the syntactic derivational system for sentences is a bilateral filter on emerging perceptual and

productive capacities: Once those capacities are complete and in register with each other, further acquisition of syntax no longer has a functional role, and the syntax acquisition mechanisms decouple from disuse, not because of a biological or maturationally mechanistic change (see Bever 1988) for a demonstration of the hypothesis that grammars act as cognitive mediators between production and perception in adult artificial language learning).

This interpretation is consistent with our recent finding that the age of the critical period differs as a function of familial handedness: FS+ deaf children show a younger critical age for mastery of English syntax than FS– children (Ross & Bever 2004). This follows from the fact that FS+ people access the lexical structure of language more readily, and access syntactic organization less readily than FS– people: FS+ children are acquiring their knowledge of language with greater emphasis on lexically coded structures, and hence depend more on the period during which vocabulary grows most rapidly (between 5 and 10 years; itself possibly the result of changes in social exposure, and emergence into early adolescence).

4. The Genetics of Cerebral Asymmetries

Remarkably little is known about the genetics of asymmetries as reflected in phenotypic handedness. Part of the difficulty lies in the paucity of reliable behavioral measures of asymmetries other than subject-reported or measured hand dominance and the associated debate over whether handedness should be viewed as a strictly categorical trait or a quantitative trait, and if so, how to measure the continuum. Dichotic listening tests (Kimura 1961) have been widely used as a non-invasive, behavioral method of assessing cerebral dominance at a cerebral level: These measures are generally consistent with sodium amytal tests and fMRI measures of functional language lateralization (Zatorre 1989, Hund-Georgiadis *et al*. 2002). Overall, the majority of subjects typically show a right ear advantage (REA) in dichotic listening studies, with a tendency for reduced REA in left-handed subjects. Zurif & Bryden (1969) found that the latter effect was moderated by familial sinistrality: Both right-handed and FS– left-handed subjects showed a REA while FS+ left-handed subjects showed minimal asymmetry effects. Zurif & Bryden (and Hines & Satz 1971) also found similar visual field dominance effects, with reduced right visual half field (VHF) superiority for digits in left-handed FS+ than FS–.

Studies of familial sinistrality effects on complex cognitive functions have yielded extremely mixed results, likely reflecting the statistically underpowered nature of many studies. Considering the moderate heritability (~20–30%) of non-right-handedness, unreliability of self-report and problems introduced by variable family size (Bishop 1990), sample sizes of several hundred subjects are needed to attain acceptable statistical power (>.8). Power can be increased substantially with the use of non-categorical measures of familial sinistrality (Corey & Foundas 2005), but these are not widely used. Even when more genetically informed familial handedness measures are used, these are sometimes based on a particular theory of genetic transmission and expression, thus confounding familial handedness effects with a specific, and likely incorrect, genetic model (e.g., McManus 1995).

Two major genetic models of handedness have been proposed: Annett's Right Shift theory and McManus's Dextral Chance theory. Both models propose that a single locus, dominant for left-hemisphere specialization, controls asymmetry, with random factors or minor alleles producing right-hemisphere shift.

The Right Shift theory (Annett 1985) proposes that a single gene, rs, handicaps language processing in right hemisphere (through unspecified mechanisms). The majority of the population is expected to be rs++, homozygous for the right shift allele, and thus strong right-handed with left-language hemisphere. Heterozygotes and those lacking the allele (rs—) have a reduced left hemisphere handicap and lateralization becomes subject to random factors. Later versions of the right shift theory include an 'agnosic' modification to the right shift gene that removes the specificity to the right hemisphere in an attempt to account for the possible links between left-handedness and autism and schizophrenia (Annett 1999).

The Dextral Chance model (McManus 1985, 1995; Annett & Alexander 1996) proposes dextral (D) and chance (C) alleles (the latter being the minor allele). Only one allele (with equal chance of being expressed) contributes to the phenotype in this model: The D allele produces right-handedness and left language lateralization; the C allele produces random handedness and lateralization, independently.

Neither the Dextral Chance or Right-Shift theories have been supported by complex segregation analysis of family and twin data (Medland *et al*. 2009, 2006), nor have candidate genes for handedness been robustly identified. The absence of a candidate gene, despite genome wide association efforts, suggests that a simple, single locus model of direct genetic influence on handedness is inadequate and complex polygenic models of small effects (Francks *et al*. 2002, 2007; Medland *et al*. 2009) should be pursued.

Rather than relying on single-gene models of handedness, we have applied a more general Bayesian multifactorial model to estimate the genetic effects of familial handedness in subjects. Categorical phenotypes can be mapped to a continuous latent variable using a standard multifactorial threshold model (Falconer 1965), a particularly useful method when Mendelian inheritance patterns are not observed, as in handedness. Under this model, the probability of expressing a phenotype in a given category is function of an unobserved liability for a phenotype, in part the sum of additive genetic effects at an unknown number of loci. A variety of maps from liability to phenotype are possible; we use the simplest case in which a phenotype is categorically expressed (or not) if the liability is above (below) a threshold.[2] We have applied such a binary threshold model to proband-reported handedness pedigrees, estimating genetic effects and latent liabilities (see Sorensen & Gianola 2002 for a technical description of the algorithm). This model produces two useful results: an estimate of the heritability of handedness under a basic genetic model and estimated liabilities (and underlying genetic effects) for our experimental subjects. As expected, this method estimates the heritability of handedness at h^2=.22–.36 (95% CI), consistent with Medland's (2009)

[2] Since liability is unobserved, a threshold for a binary trait may be chosen for convenience, e.g., zero.

maximum likelihood estimate of .20–.27 from a much larger twin study.[3]

The use of estimated liabilities, like other methods of quantifying the degree of familial sinistrality (Corey & Foundas 2005, Karev 2010), yields a substantial power increase over dichotomous FS+/– methods and does so with minimal genetic assumptions. In addition, the Bayesian nature of our method produces liability distributions, rather than point estimates of familial sinistrality. This not only produces an implicit measure of uncertainty for each individual, but also avoids the common confounding of familial sinistrality measure and family size (Bishop 1990), since dispersion, rather than the mean, is largely affected by family size. Emerging results from our laboratory using this measure, in conjunction with EEG measures promise to identify familial handedness effects more robustly than previous behavioral studies. In an event-related potential (ERP) version of the Townsend *et al*. (2001) word probe study, we have found evidence for variability mediated by familial sinistrality in the lateralization of the P2 ERP component, a possible marker for early lexical processing (Hancock & Bever 2010). This lends initial support and validity to this approach, and to its significance for functional processing of language.

Of course, there are differences in neurological organization mediated by familial sinistrality in modalities other than language. For example, we recently found that an early right hemisphere negativity (ERAN) to musical anomalies is significantly stronger in FS– than FS+ right-handed subjects (Sammler *et al*. 2012). The same FS– subjects showed only a trend for a stronger early left hemisphere negativity (ELAN) to grammatical anomalies. However, almost all FS– subjects showed both an ERAN for music and an ELAN for language, while almost no FS+ subjects showed this differential pattern. This suggests further that the neurological organization for complex behaviors is less differentiated and lateralized in FS+ right-handers (for recent empirical findings related to our research, see also Fisher *et al.* 2012, Hancock 2012).

Why are there these effects of familial sinistrality? In the case of language, our recent findings lend support to Bever *et al*.'s (1987) hypothesis that lexical representations are relatively more available in the right hemisphere in FS+ people. That hypothesis reasoned that the lexicon may be more susceptible to widespread neurological representation than syntactic processing: The latter is more demanding computationally, and thus may be relegated to the computationally more powerful hemisphere. But if FS+ people have less lateralized brains, as suggested by various facts, this would allow for more right hemisphere representation and processing for the simpler aspects of language, in this case the lexicon. In a more general framework, the SNR concept of the basis for cerebral asymmetries would suggest that genetically-mediated more equilateral dopamine D2 activity in FS+ people reduces the bias towards left hemisphere language function typical in FS– people. Under this model, non-linguistic effects of familial handedness are also expected, based on the extent to which the basal ganglia are involved in non-linguistic tasks.

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[3] Heritability here is the ratio of variance explained by genetic factors to variance explained by genetic, familial and environmental factors.

5. Implications of Genetic Variation in Language Organization and Representation

The empirical premise behind the differences between FS+ and FS– people is that FS+ people have reduced left hemisphere lateralization, and correspondingly weaker differential lateralization for language and other complex behaviors: In some cases, this (by hypothesis) initially quantitative difference results in apparent qualitative categorical effects. There are several different kinds of implications of our findings that support these results.

a) Implications for clinical research and therapies

Virtually every clinical study of language dysfunctions and special language behaviors reports the handedness of the patients. Yet, despite Luria's classic findings on aphasia in FS+ patients, and the established association between familial left-handedness and psychiatric disorders, almost no studies of language dysfunction report familial handedness of the patients. Our 30 years of behavioral research and our recent modeling and imaging results argue strongly that it is critical to differentiate patients according to the familial-handedness-based likelihood that they are, or would have been, left-handed. Our current model of family pedigree effects offers an opportunity to enrich clinical research in this way.

b) Implications for experimental research and theory

Psycholinguistic behavioral and neurolinguistic research continues today in attempts to build models of language acquisition and language performance. The behavioral differences between FS+ and FS– people we have outlined is sufficient reason alone to keep track of this variable: If FS+ people consistently access lexical items more readily than FS– people, and conversely for syntactic patterns, this will surely interact with many specific kinds of experiments. Our recent neurological findings go further to substantiate the importance of familial handedness, since today's model building often refers to potential neurological concomitants of the postulates of the models.

c) Implications for the genetics of handedness: a refined phenotype

As we have noted, remarkably little is known about the genetics of left-handedness, despite its frequency and substantial heritability. In part this may be because handedness in general is multiply determined; it is also made more difficult to study because at least some left-handedness has been shown to be the result of acquired brain damage in people who are genetically right handed: Conversely, many 'right' handed people may be genetically left handed, but forced by social pressures to be right-handed. In sum, the phenotypic differentiation of left and right-handedness is not sharp, which complicates any search for specific polymorphisms or other genetic effects that influence handedness. An outcome of our research, using the model that estimates additive genetic effects associated with left-handedness as a function of family background, will be to sharpen the cognitive and neurological phenotypes of explicit left-handers, as well as explicit right-handers, with high and low genetic loadings for left-handedness. The result will be a better chance that DNA assays will reveal poly-

morphisms associated with handedness phenotypes than current case-control studies that consider only phenotypic hand preferences.

d) Implications for linguistic theory and the biological foundations of language

Finally, it is clear that there are different mechanisms for the expression of language in behavior, at least in the quantitative contributions and interactions between the hemispheres. This raises the question for linguistic theory and the genetics of language as to whether the quantitative differences result in actual qualitative differences in how language is represented neurologically and processed computationally. To put it bluntly:

(i) Is there more than one 'normal' form of neurological representation for language?
(ii) If so, is there more than one 'normal' computational architecture for grammars?
(iii) If so, is there more than one normal system for language behavior?

To decide the answer to (i) and (ii) requires a fuller analysis of what kind of lexical information is in fact relatively strongly represented or accessed in the right hemisphere in FS+ people. It could be information directly relevant to syntactic representations, such as lexical category, morphological structure, and relevant to phonological theory if it is represented in abstract phonological terms. In this case, we would have to conclude that indeed there is more than one normal form for neurological representation of language. On the other hand, the relevant 'lexical' information in the right hemisphere of FS+ people could be associative information, information that would facilitate lexical access in behavior, but not be directly relevant for syntactic computations.

There are corresponding options for the implications of the differences for the actual structure of linguistic grammars. For example, a current controversy in linguistic theory has to do with whether lexical representations are simply special cases of idioms which in turn are special cases of sentence constructions and conversely (e.g., Goldberg 1995, Boas & Sag 2012, Fillmore *et al*. 1988; see also Culicover & Jackendoff 2006). On such theoretical views of grammar, different representational systems for relating the lexicon to larger compositional structures would definitely imply different kinds of computational architectures in different groups of people.

The largest question has to do with the implications of our findings for the causal relation between neurological structures and the structure of language. It is often implicit in biolinguistic discussions that a critical contributor to the structure of language is in the details of the biological basis for its acquisition, representation and use. On this view, the biopsychoneurological basis for language exists (whether by exaptation, saltation, or selection) in such a way to make possible language as we see it neurologically organized. Our findings of the considerable lability of that organization suggest another possibility: that the basis for language lies not in any specific neurological set of centers and connections, but in the availability of various cerebral components that can carry out the kind of computations required for the external mapping of sequences of internal propositional relations.

This view is a particular implementation of the recent 'minimalist' program, on which syntax is a direct expression of a system that efficiently relates propositional structures to externalized serial representations (Chomsky 2000, Boeckx 2006). On this view, the neurological organization for the best mapping system follows otherwise available computational centers and connections between them — the neurological organization in each case is itself the best available implementation. But what is the 'best' implementation will differ as a function of larger tendencies and constraints on how the different computational components of the brain are 'best' connected. Our research suggests that there is systematic normal variation in what is 'best'.

There are at least two implications of this idea for the biological foundations for language. The more conservative assumption would be that all people share a fundamental form of neurological capacity for language, but differ in the emphasis on lexical versus compositional mapping processes. This would mean that attested languages must convey substantial structural information both in their lexicon and in syntactic patterns, to accommodate to each of the major normal populations. This may underlie the apparent fact that languages are often structurally redundant in the corresponding way — both lexical and compositional structures are evident in actual sentences.

A more radical interpretation of the normal variation in neurological organization for language is that the unique biological capacity for language is rooted more deeply in human neurophysiology than in specific computational centers and connections between them. While this may seem mysterious or at least radically speculative, stranger things have turned out to be true (for more extensive discussion of these issues, see Bever, in press).

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What Connects Biolinguistics and Biosemiotics?

Prisca Augustyn

This paper reviews the background, fundamental questions, current issues, and goals of biolinguistics and biosemiotics. The purpose of this paper is to give a brief history of these movements, to clarify common objectives and areas of overlap, to evaluate recent articulations of their respective future agendas, and to address some aspects of focus and disciplinary prejudice that may stand in the way of productive collaboration concerning the biology of language.

Keywords: biolinguistics; biosemiotics; Chomsky; Jacob; Lorenz; Peirce; Sebeok; Uexküll

1. Origins of Biolinguistics and Biosemiotics

While the scholarly agendas of biolinguistics and biosemiotics may seem very different in scope, they unequivocally share a common interest in human language as a species-specific cognitive tool. They also share a philosophical core that is anchored in the concepts of Peircean *abduction* and Uexküllian *Umwelt* (cf. Augustyn 2009) on the one hand, and an interest in the building blocks of life and its underlying principles that has connected language to research in cell biology (cf. Barbieri 2010) on the other hand.

Uexküll's concept of *Umwelt* — the subjective species-specific world created by an organism — is central to the ethological approach to human language shared by biolinguists and biosemioticians; and both movements have interacted in different ways with molecular biology to explore the *Bauplan* of human language and/or the semiotic capacities of various species. Examining the ways in which these interests intersect and diverge in biolinguistics and biosemiotics is the main objective of this paper.

Like Peirce, Uexküll approached nature and culture through the analysis of signs and sign processes; and his concept of *Funktionskreis* has been reinterpreted as a general model of semiosis. The semiotics of Charles Sanders Peirce and Uexküll's *Umweltlehre* are regarded by many, but not by all practitioners, as the theoretical and philosophical core of biosemiotics. Peirce is equally important to the origins of Chomskyan biolinguistics, but most likely also not valued to the same degree by all of its current practitioners.

Contemporary reviewers referred to Jakob von Uexküll (1864–1944) as a *Kantian biologist* (Wirth 1928). Trained as a zoologist and physiologist, Uexküll

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first focused on the sense perception of organisms, mostly of marine animals. Throughout his career, Uexküll applied what he observed in his studies of muscular physiology to the semiotic capacities of the organism as a whole; and his *Umweltlehre* evolved into a general theory of life as semiosis. Uexküll is, therefore, generally regarded as the forerunner of ethology and comparative psychology; and Konrad Lorenz owed the foundational insights that informed his experiments with graylag geese, jackdaws, and dogs to Uexküll (G. von Uexküll 1964: 198).

Jakob von Uexküll's radical constructivism is exemplified in his statement that "[no] matter how certain we are of the reality that surrounds us, it only exists in our capacities to perceive it. That is the threshold we have to cross before we can go any further" (J. von Uexküll 1902: 213 [my translation]). Thure von Uexküll outlined the main aspects of Jakob von Uexküll's Umwelt theory as follows (T. von Uexküll 1982: 4–8):

(A) [True] reality (Jakob von Uexküll calls it *Natur*) that lies beyond or behind the nature that physicists, chemists, or microbiologists conceive of in their scientific systems reveals itself through signs. These signs are therefore the only true reality, and the rules and laws to which the signs and sign-processes are subject are the only real laws of nature. […]

(B) The methodology of Umwelt-research, which aims to reconstruct this 'creating' of [reality] […] means, therefore reconstructing the Umwelt of another living being. […]

(C) The aim of Umwelt research is to create a theory of the composition of nature […] [by exploring] the sign-processes that govern the behavior of living subjects.

Chomsky's interest in Uexküll and ethology was a result of discussing alternatives to the dominant paradigms in linguistics and behavioral psychology with Eric Lenneberg and Morris Halle in the early 1950s. The biolinguistic program, therefore, derives its general approach to human language from ethology and comparative psychology; and Konrad Lorenz played an important role in its evolution (Jenkins 2000: 10). Especially Eric Lenneberg's (1964) *Biological Foundations of Language* "anticipated many themes of the coming decades" (Jenkins 2000: 3); and Chomsky concluded in a famous interview that "[linguistics] is really a theoretical biology" (Sklar 1968: 218). Uexküll would have been pleased with biolinguists for "making [linguistics] a biological science" as he once suggested to a linguist friend in a letter (Kull 2001: 3). This is the point of view from which Sebeok's biosemiotics approaches human language.

Based on these common ideas on the biological foundations of language and thought, both Chomsky and Sebeok emerged from the dominant paradigms in linguistics in the middle of the 20th century to follow new theoretical paths in linguistics and semiotics. Both Chomsky and Sebeok's fundamental ideas about human language were connected to the work of ethologists and comparative psychologists like Uexküll, Lorenz, and Tinbergen (cf. Lenneberg 1964); and their general views on human language have been consistently similar. They both see

human language foremost as a cognitive tool (because the species was capable of *communication* before it emerged). Agreement on this issue is far from trivial and its pronouncement bound to raise eyebrows among many linguists. Sebeok called language a *secondary modeling system* that allows the species to create models of reality in addition to the species-specific perceptual system, the *primary modeling system* (cf. Anderson & Merrell 1991, Sebeok & Danesi 2000). He believed that language served primarily "the cognitive function of modeling, and, as the philosopher Popper as well as the linguist Chomsky have likewise insisted […], not al all for the message swapping function of communication. The latter was routinely carried on by nonverbal means, as in all animals, and as it continues to be in the context of most human interactions today" (Sebeok 1991: 334).

Chomsky likewise sees language as a tool of thought that is based on principles that are not specific to language. They consequently also share the view that language is an *exaptation*; and they both see organism–environment-interaction (i.e. species-specific *Umwelt*) as a crucial component of the growth of language in the individual. This is a view that separates them from a strong evolutionary psychology of language (e.g., Pinker 1994, 2003).

While the semeiotic of C.S. Peirce clearly provided the foundational philosophical background for the "vast life science" that Sebeok saw in his *global semiotics* (cf. Sebeok 2001b), the Peircean concepts of *abduction* and *habit-taking* also play an essential role in Chomsky's *generative grammar*. He recently referred to the analysis of the deep structure of abstract operations of formal grammar as a "Peircean logic of abduction" (Chomsky 2006: 86).

To different degrees, practitioners of both biolinguistics and biosemiotics connected with molecular biologists during the 1970s. An MIT conference in 1974 solidified the affinities between the work of French molecular biologist François Jacob and Chomsky's theory of *principles and parameters*. While this connection resulted in a reciprocal exchange of ideas between theoretical linguistics and molecular biology, Sebeok and his followers established their connection with biochemistry more indirectly by interpreting such work as Marcel Florkin's (1974) "intracellular semiotics" (Kull 1999: 387), the work of Sorin Sonea and Maurice Panisset (1983), and Lynn Margulis (1998) (cf. Sebeok 2001b). Sebeok's interaction with Thure von Uexküll (Jacob von Uexküll's son), the founder of psychosomatic medicine in Germany, established the field of *endosemiotics* (e.g., T. von Uexküll *et al.* 1993); and towards the end of the millennium, biosemiotics found two molecular biologists to carry the project forward along somewhat different trajectories; Marcello Barbieri's *code biology* on the one hand (e.g., Barbieri 2003), and Jesper Hoffmeyer's *biosemiotics* on the other hand (e.g., Hoffmeyer 2008).

The connection between cell biology and biosemiotics came from two distinct origins, but they both grew out of the desire of molecular biologists to overcome the limitations of mainstream biology to address fundamental questions revolving around concepts like *signal, information,* or *code*. The molecular biologist Jesper Hoffmeyer had turned to philosophy and connected with Sebeok in the early 1980s at a time when, after exploring the semiotic capacities of other animals in his *zoosemiotics* (e.g., Sebeok 1972), Sebeok wanted to establish a semiotics that sees life as semiosis on all levels. The biosemiotics

that Sebeok later considered to be his "principal contribution to semiotics" (Sebeok 2001b: 180) was one that included all levels of nature and culture beyond the boundaries that Umberto Eco had drawn in his *Theory of Semiotics* (Eco 1976), and he expanded semiotics to a vast life science down to the level of the cell. Eco had already drawn semiotics away from a focus on communication and established the primacy of signification, but declared *zoosemiotics* "the lower limit of semiotics" when drawing the "political boundaries" of the field (Eco 1976: 9). While some practitioners of semiotics welcomed Sebeok's global (bio)semiotics that extends across all levels of life, others see it as a perhaps premature extension of *zoosemiotics* to all life forms that does not account for the different types of semiosis that exist in the living world.

Jesper Hoffmeyer organized the first of the *Gatherings in Biosemiotics* in Copenhagen in 2001, unfortunately the year of Sebeok's death. After the fourth *Gatherings* in Prague, the *International Society for Biosemiotic Studies* was founded in 2005, with Hoffmeyer as its inaugural president. The Prague meeting in 2004 had brought together an energetic group of molecular biologists, theoretical biologists, embryologists, physicists, linguists, information scientists, philosophers of science, and others who agreed that the most fundamental characteristic of life is sign action/*semiosis*; and the movement gained the necessary moment-um to create an organization and found its journal *Biosemiotics* in 2005. The editor of this journal is the embryologist Marcello Barbieri, a scientist who had articulated a line of research on the *organic codes* of life he had previously called *semantic biology* (Barbieri 2003).

What separates these two distinct connections between molecular biology and semiotics is often reduced to an argument over whether *the sign* and *semiosis* (Sebeok/Hoffmeyer) or *codes* and *code-making* (Barbieri) should be considered primary in biosemiotics. Perhaps, the nature of these two currents that connect biosemiotics with molecular biology can best be characterized by different intellectual/scientific styles, one seeking a biology with a philosophical integrity that places the life sciences firmly within semiotic theory (Sebeok/Hoffmeyer), the other rooted in the natural sciences while at the same time promoting careful collaboration across the disciplines (Barbieri).

Meanwhile, the *International Network in Biolinguistics* had crystalized out of the work Chomsky, Halle, and Lenneberg had begun in the late 1950s. This scholarly organization had many precursors in different places at different times (cf. Jenkins 2000); but in its most recent configuration held its first meeting at the University of Arizona in Tucson (February 2008), organized by Massimo Piattelli-Palmarini. The online Journal *Biolinguistics* published its first volume in 2007. Both movements have recently articulated formative statements (Fitch 2009, Kull *et al*. 2009), which will be addressed in more detail after first establishing an important characteristic that undeniably connects biolinguists with biosemioticians.

2. Biolinguists and Biosemioticians Have Never Been Modern

In his essay *We Have Never Been Modern*, Bruno Latour lays out what he calls the Modern Constitution that separates "three regions of being" (Latour 1993: 39),

nature — *politics* — and *discourse* through the processes he calls *purification* and *mediation*. While the work of *purification* separates nature from society and keeps the natural sciences as the domain of explaining natural phenomena separate from the social sciences as the domain of explaining the social order of things; the work of *mediation* explains how "mixing biology and society" makes it possible that "[all] of nature and all of culture get churned up again every day" (p. 2). The work of *purification* is characterized by working within the strict disciplinary boundaries of the natural sciences, so that the facts of nature are, in fact, created in the laboratory. Practices of *purification* rely on "two different ontological zones: that of human beings on the one hand, and that of non-humans on the other" (p. 10). It is a consequence of this Modern Constitution that non-humans have come to make much better informants in the lab.

The work of *mediation* is the work of *hybrids*. The paradox of the Modern Constitution is that the separation of nature and society (= *purification*) both makes *mediation* possible, but marginalizes it and renders it invisible at the same time. But only *hybrids*, says Latour, "can change the future" (p. 11). Mainstream linguists and mainstream biologists who suffer from the illusions of the Modern Constitution practice purification so that nature and society must remain absolutely distinct. This includes the first illusion that even though we construct nature, nature is as if we did not construct it, and another one, that even though we do not construct society, it is as if we construct it (Latour 1993). More importantly, Latour shows us that the Modern Constitution entails, besides the dichotomy of *purification* and *mediation*, the separation between non-humans (as nature) and humans (as culture).

Hybrids who reject the Modern Constitution, because they practice *mediation* (such as, for instance, anthropologists who study non-Western cultures or ethologists who study the physiological and cognitive capacities of different species) are seen as outsiders of the purified disciplines of the mainstream. This becomes especially apparent when anthropologists study cultures in the West, or when ethologists, biologists, linguists, or semioticians study the cognitive capacities of humans.

Chomsky's and Sebeok's grounding in Peircean semeiotic and Uexküllian *Umwelt* theory clearly makes them *hybrids* (sensu Latour 1993). *They have never been modern*. The difficulty of their position within the field of linguistics (or semiotics, even though *purification* is much less of an issue there) is that their work is prone to gross misinterpretation, precisely because the mainstream lives by the illusions that uphold the Modern Constitution. As Latour explains, "[the] essential point of this Constitution is that it renders the work of mediation that assembles hybrids invisible, unthinkable, unrepresentable" (Latour 1993: 34).

This can be explained with the predominant folk-definition of *Universal Grammar*, an unfortunate misinterpretation that can be attributed to the artificial dichotomies that are the result of the disciplinary purification that wants to see the field of linguistics in the social sciences or the humanities (culture) rather than — as Chomsky and Sebeok would have it — as a domain of biology, that approaches the study of human language as a phenomenon of nature. The folk-definition of *Universal Grammar* is something like an equivalent of linguistic universals or the things that are shared by all languages, a definition that does

not depend on the ethological perspective and is not in contradiction with the laws of the Modern Constitution.

For most students of linguistics, it is difficult to understand Chomsky's definition of *Universal Grammar* right away as the properties of the initial state of the human faculty of language that are specific to the species. For those who live by the Modern Constitution, the *hybrid* character of this concept remains nebulous, "unthinkable, unrepresentable" (Latour 1993), because they want to ground everything in the Modern Constitution, keep language in the domain of culture, and the field of linguistics separate from biology. For those who understand the philosophical background behind the faculty of language as a combination of innate capacities, organism-environment interaction (*Umwelt*), and abstract principles that are not specific to the faculty of language (cf. Chomsky 2005: 6), the *hybrid* character of this concept is quite uncontroversial.

Modernity has made it impossible for some to take the ethologist's perspective on our species, to mediate instead of separating nature and culture. Maybe that fog will begin to lift when recent articulations of *posthumanism* or *posthumanities* will have penetrated the mainstream and hybrids gain critical mass in traditional academic disciplines such as linguistics and biology.

Chomsky's (2009 [1966]) *Cartesian Linguistics* likewise defies the paradoxes of the Modern Constitution, because it begins with the unresolved questions of the 17th century. Because the very title of Chomsky's *Chapter in the History of Rationalist Thought* is perpetually mischaracterized and misinterpreted, especially by those who don't care to read it and prematurely associate its title with a folk definition of the Cartesian mind/body dualism, the introduction to the 2009 edition explains that Descartes "was among the first to recognize the importance of this 'ordinary' form of linguistic creativity […] for the study of the human mind" (p. 1) that is the central focus of biolinguistics. This hybrid concept of language as a natural object therefore characterizes biolinguistics as a natural science (cf. Boeckx 2005).

On the one hand, the *cognitive revolution* of the mid twentieth century is a renewal and further development of the cognitive revolution of the 17th century, while another influential factor in the renewal of the cognitive revolution was the work of ethologists, a field that defies the principles of the Modern Constitution. For the biolinguistic program, Chomsky adapted "[the] framework of ethology and comparative psychology […] to the study of human cognitive organs and their genetically determined nature, which constructs experience — the organism's *Umwelt*, in ethological terminology — and guides the general path of development, just as in all other aspects of growth of organisms" (Chomsky 2006: x).

Sebeok's last articulations of biosemiotics appeared in the year of his passing in his collection of essays entitled *Global Semiotics* (Sebeok 2001b). He attributes the origin of biosemiotics, his "principal contribution to general semiotics" (p. 180), to his rediscovery of Uexküll's *Umweltlehre*, which inspired his definition of "[semiosis as] the processual engine which propels organisms to capture 'external reality' and thereby come to terms with the cosmos in the shape of species-specific internal modeling systems" (p. 15).

This non-species-specific terminology is the hallmark of Modeling Systems Theory, an approach he articulated in *The Forms of Meaning* together with Marcel

Danesi (Sebeok & Danesi 2000), characterizing biosemiotics or global semiotics, as a comprehensive life science of nature and culture; or "nature/culture", as Latour (1993) prefers to write. Sebeok, the linguist whose life work was to turn semiotics into a science of all life, obviously *has never been modern*.

3. The Search for the *Bauplan* of Human Language

Since the formation of their professional organizations, the *International Society of Biosemiotic Studies* and the *International Network in Biolinguistics* have articulated their goals and objectives and several publications stand out as foundational. For the former society, the core publications are the volumes published in a book series on Biosemiotics under the editorship of Marcello Barbieri and Jesper Hoffmeyer. The third volume in the series, *Essential Readings in Biosemiotics,* was edited by Donald Favareau as a rather copious anthology for a field that is, according to the editor, "nothing yet resembling a mature, by which is meant coherent [field]" (Favareau 2010: iii).

Favareau's expertise in the historical background as well as current issues of biosemiotics is evident in his 80-page introduction that takes the reader through "An evolutionary History of Biosemiotics". From Hellenic thought, through the Middle Ages, across Modernity, Favareau narrates the history of concepts in biosemiotics based on the following definition: "Biosemiotics is the study of the myriad forms of communication and signification observable both within and between living systems. It is thus the study of representation, meaning, sense, and the biological significance of sign processes — from intercellular signaling processes to animal display behavior to human semiotic artifacts such as language and abstract symbolic thought" (p. v).

The book has four parts: "Part 1: Sebeok's Precursors and Influences" includes excerpts from Jakob von Uexküll's (1940) *Theory of Meaning*, sections from Charles Sanders Peirce (1931–1958 [1866–1913]), Charles Morris's (1955) *Signs, Language and Behavior*, and Yuri Lotman's (1990) *Universe of the Mind*. "Part 2: The Biosemiotic Project of Thomas A. Sebeok" includes Sebeok's (2001) own account of "Biosemiotics: Its roots and proliferations" and texts by his collaborators Heini Hediger (1981) on "The Clever Hans Phenomenon", Martin Krampen's (1981) "Phytosemiotics", Thure von Uexküll's (1993) "Endosemiotics", Giorgio Prodi's (1988) "Signs and codes in immunology", Rene Thom's (1975) chapter "The animal mind" from his *Structural Stability and Morphogenesis*, and Anderson *et al.*'s (1984) "A semiotic perspective on the Sciences: Steps to a new paradigm". "Part 3: Independent Approaches to Biosemiotics" includes Kull's previously unpublished "Theoretical biology on its way to biosemiotics", Friedrich Rothschild's (1962) "Laws of symbolic mediation", Marcel Florkin's (1974) "Concepts of molecular biosemiotics and of molecular evolution", Gregory Bateson's lecture "Form, substance and difference" (included in Bateson 1972), Howard Pattee's (2005) article on "The physics and metaphysics of biosemiotics", and an excerpt from Terrence Deacon's (1997) *The Symbolic Species*. Lastly, "Part 4: The Contemporary Interdiscipline of Biosemiotics" includes Hoffmeyer & Emmeche's (1991) "Code-Duality and the semiotics of nature" and a chapter from Hoffmeyer's

(2008) book *Biosemiotics: An Examination in the Life of Signs and the Signs of Life*, an article on "Information and semiosis in living systems" by João Queiroz *et al.* (2005), an excerpt from *Readers of the Book of Life* by philosopher of science Anton Markoš (2002), Soren Brier's (2006) "Cybersemiotic model of communication", an excerpt from philosopher Günther Witzany's (2007) *The Logos of the Bios*, and a recent article by Marcello Barbieri (2008) on "Biosemiotics: A new understanding of life" that was published in *Naturwissenschaften*, outlining the coexistence of code-based bio-semiotics and sign-based (or interpretation-based) biosemiotics.

While Barbieri's paper ends by affirming that "all versions of biosemiotics share the view that semiosis is fundamental to life" (p. 791), biosemiotics today is unequivocally characterized by what Anton Markoš calls a "plurality of view" (p. 657); and — while Anderson *et al*. (1981) warned that "optimism for a general or unified approach is bound to invite scorn" (p. 404) — many among its practitioners share Jesper Hoffmeyer's hope for better "transdisciplinary communication" (p. 590) in the future.

For biolinguistics, the organization's website lists Turing's (1952) paper on "The chemical basis of morphogenesis", D'Arcy Thomson's (1945) *On Growth and Form*, and Eric Lenneberg's (1967) *Biological Foundations of Language* as its foundational texts. More recent articulations include Jenkins' (2000) *Biolinguistics* and his edited volume *Variation and Universals in Biolinguistics* (Jenkins 2004), Hauser *et al*.'s (2002) "The language faculty: What is it, who has it, and how did it evolve?", Hauser & Bever's (2008) "A biolinguistic agenda", Chomsky's (2005) "Three factors in language design" and his "Biolinguistic Explorations: Design, Development, and Evolution" (Chomsky 2007), Di Sciullo's paper "A biolinguistic approach to morphological variation" (2008) from a workshop on Linguistic Universals and Linguistic Fieldwork held at Harvard University, and Di Sciullo & Boeckx's (2011) edited volume *The Biolinguistic Enterprise: New Perspectives on the Evolution and Nature of the Human Language Faculty*.

Both movements received formative statements outlining fundamental questions and issues for the future in 2009. W. Tecumseh Fitch articulated the "Prolegomena to a future science of biolinguistics" (Fitch 2009), while biologist/semiotician Kalevi Kull collaborated with biological anthropologist Terrence Deacon, molecular biologists Claus Emmeche and Jesper Hoffmeyer, and the semiotic theorist Frederik Stjernfelt on the eight surprisingly brief "Theses on biosemiotics: Prolegomena to a theoretical biology" (Kull *et al*. 2009).

1. *The semiosic—non-semiosic distinction is coextensive with the life-nonlife distinction, i.e. with the domain of general biology.* […]
2. *Biology is incomplete as a science in the absence of explicit semiotic grounding.* […]
3. *The predictive power of biology is embedded in the functional aspect and cannot be based on chemistry alone.* […]
4. *Differences in methodology distinguish a semiotic biology from non-semiotic biology.* […]
5. *Function is intrinsically related to organization, signification, and the concept of an autonomous agent or self.* […]

6. *The grounding of general semiotics has to use biosemiotic tools.* […]
7. *Semiosis is a central concept for biology that requires a more exact definition.* […]
8. *Organisms create their* umwelten. […]

(Kull *et al*. 2009: 167–173)

Kull and his colleagues were able to agree on some fundamental ideas on "what biology needs to be focused on in order to describe life as a process based on semiosis" (Kull *et al*. 2009: 167). They consider one aim of the movement "to explain how life evolves through all varieties of forms of communication and signification (including cellular adaptive behavior, animal communication, and human intellect) and to provide tools for grounding sign theories" (ibid.).

At least the authors of this document seem united in their search for the basic concepts for a theoretical biology, although this document excludes the concepts associated with Barbieri's view that organic semiosis is defined by *coding*. According to Barbieri, coding and interpretation are both present in nature; however, while organic semiosis gave rise to the organic codes on the cellular level, interpretive semiosis, or interpretation, can only exist in organisms that build internal representations of the world, i.e. in organisms that have a nervous system (Barbieri 2011). While the "Theses on Biosemiotics" (Kull *et al*. 2009) present biosemioticians as united in their desire to transform biology away from mechanistic paradigms towards sign-based theories, they exclude from their agenda a view that sees two distinct semiotic processes on different levels of life (cf. Barbieri 2011).

Fitch's "Prolegomena to a future science of biolinguistics", in contrast, focuses on the problems that stand in the way of a unified approach to biolinguistics. Most may have expected from the prolegomena a unified biolinguistics and an inherently diverse biosemiotics, especially because the scope of biolinguistics appears to be decidedly narrower (because it is *only* concerned with the human language faculty). Jenkins (2000: 1) highlights the fundamental questions of biolinguistics as articulated by Chomsky:

(1) What constitutes knowledge of language?
(2) How is this knowledge acquired?
(3) How is this knowledge put to use?
(4) What are the relevant brain mechanisms?
(5) How does this knowledge evolve (in the species)?

These are questions that unequivocally interest many biosemioticians, especially those practitioners who value the theoretical perspectives provided by Peirce and Uexküll. While the overlap in foundational literature seems small at first glance, a closer look at Jenkins (2000), Chomsky's (2005, 2007) own recent articulations and the bibliographies of their foundational literature may convince many practitioners of biosemiotics with an interest in human language that they have lived with the wrong idea of biolinguistics for too long. Many of them may have been guilty of uninformed criticisms of Chomsky, "whose ideas so many scholars apparently love to hate" (Fitch 2009: 287).

Fitch gives a sobering assessment of the potential for a biolinguistic science, focusing foremost on the sociological, terminological and intellectual impediments. He criticizes the lack of collaboration between linguistic theory and neuroscience, and accuses neuroscientists for "a decade or so of somewhat self-indulgent neo-phrenology" (p. 284). He also sees challenges "concerning terminology, disciplinary turf wars, and struggles for dominance" (p. 285) that may exist among biosemioticians as well.

Among the real challenges, not sociological but intellectual in nature, Fitch points to the theoretical shortcomings in neuroscience and the lack of good collaboration with theoretical linguists, because neuroscientists still "do not understand how brains generate minds" and that "principles underlying brain development and evolution remain only dimly understood" (Fitch 2009: 285). Likewise, neuroscientists do not know how brains generate language, and there is very little collaboration between neurolinguists and theoretical linguists (cf. Andrews 2011).

An important issue for biolinguists, according to Fitch, consists of "questions of meaning" and what he calls "unresolved semiotic challenges [that] pose problems for any aspect of cognition". Maybe Fitch and those who agree with him would find more satisfying theories of meaning in the foundational literature associated with biosemiotics? When Fitch writes "[we] have a good theory of information (Shannon information theory), but we lack anything even approaching a good theory of meaning" (p. 285), he's looking for the same alternative to "many currently popular models and metaphors for understanding genes, brain and language [that] need to be abandoned if [biolinguists] hope to make any substantial progress" (p. 286) that many biosemioticians see in mainstream biology.

Most biosemioticians would see eye to eye with Fitch on that central challenge. In fact, nobody would agree more with this than Jesper Hoffmeyer, who turned to philosophy to address these issues in biology and became involved in biosemiotics after connecting with Sebeok in the 1980s. It is precisely the vagueness of concepts such as *information* or *signal* in biology that drove biologists to philosophy and semiotics and fueled the biosemiotic movement. For Hoffmeyer, "[biosemiotics] does not turn experimental biology to metaphysics but instead replaces an outdated metaphysics — the thought that life is only chemistry and molecules — with a far better, more contemporary, and more coherent philosophy. Life rather than natural law — and signs rather than atoms — must become natural science's fundamental phenomena" (Hoffmeyer 2008: 15).

While Barbieri has reached out to biolinguists to explore common interests and possibilities of collaboration (Barbieri 2010, 2011), Hoffmeyer has relied on popular misconceptions about Chomsky's biolinguistics that lead him, for instance, to reject Chomsky and prefer Bruner (1985) on the issue of language development. (cf. Hoffmeyer 2008) As one of the biosemiotic movement's most prolific and formidable articulators, it is unfortunate that he has turned his back on an intellectual movement that shares so many foundational philosophical parallels, and whose progress depends on much of the same issues and challenges as his own efforts in biology and biosemiotics.

What distinguishes Bruner from Chomsky is the fact that Bruner conducted

empirical research on mother-infant communication to gain a better understanding of language acquisition, while Chomsky has maintained consistently that three factors constitute the human faculty of language: (1) the genetic endowment, (2) organism–environment interaction (species-specific *Umwelt*), and (3) abstract principles not specific to the faculty of language (cf. Chomsky 2005, 2007). To say Bruner has the better theory of language development, because he chose to study mothers and infants in their homes is like accusing Chomsky for not focusing on what he chose *not* to focus on.

Moreover, while the empirical studies of mother-infant interaction make a worth-while research agenda, it is one that supports the Modern Constitution (sensu Latour 1993) in the sense that the homes of families in the New York area in the 1980s are bound to have outcomes that are culture-specific and relevant only for urban middle-class families in the West; while the abstract principles of human language the way they have been studied by biolinguists are not subject to this kind of cultural bias, because they belong to a research agenda that is built on a Galilean-style theory construction (cf. Boeckx 2005) that remains on the ethological level and defies the distinction between nature and culture, and in the sense of Latour (1993) *has never been modern*. To refuse to engage with what Chomsky *has* focused on, because of what he has chosen *not* to focus on (even though he never disputed its relevance to other research agendas) is like criticizing a pianist for never playing the violin.

4. Conclusion

Biolinguists may find ideas for addressing the 'semiotic challenge' (Fitch 2009) in the foundational texts for biosemiotics (e.g., Favareau 2010). Likewise, biosemioticians who are interested in human language simply cannot afford to bypass biolinguistics. Some foundational insights in linguistics have merit on that level of analysis that is the ethological/comparative psychological perspective, even though they may not tap into many physiological, affective, or social aspects associated with human language.

Linguists in the context of semiotic *Gatherings* therefore always run the risk of being perceived as naive or uninformed about the many layers of language and communication the abstractions of mainstream linguistics do not address. But good pianists can appreciate the violin even if they choose not to play it themselves.

Semioticians in the context of linguistics, likewise, have the challenges any *hybrid* faces in the 'mainstream'; but biosemioticians who are interested in finding the *Bauplan* for human language, should find capable collaborators among biolinguists. In both fields, there are likely to be "linguists and biologists, along with researchers in the relevant branches of psychology and anthropology, [who] can move beyond unproductive theoretical debate to a more collaborative, empirically focused and comparative research program aimed at uncovering both shared (homologous or analogous) and unique components of the faculty of language" (Hauser *et al*. 2002: 298).

In the spirit of such a collaborative, empirically focused and comparative

research program, Fitch (2009: 311) sees the future of biolinguistics in formulating testable hypotheses on the biology of language such as the following example concerning language acquisition:

> If human language acquisition is just a special case of a general innate capacity for acquiring culture (Tomasello 1999), then individual progress in acquiring language should be closely correlated, both temporally and across individuals, with their progress in other aspects of socialization and mastery of non-linguistic culture (cf. Markson & Bloom 1997).

Some skeptics may question whether that is, indeed, a good hypothesis; and others may argue over the best way to empirically test it. It may seem unsatisfying or uninspiring to see the big questions about language reformulated as hypotheses such as this one; and, more importantly, they can only be pursued within the institutional structures that allow linguists and psychologists to write grant proposals that are considered 'worthy' within the mainstream that will likely perpetuate the Modern Constitution (Latour 1993) for some time.

It will be difficult for the *hybrids* to establish new paths of collaboration that allow them to truly transcend the practices of *purification* that keep the disciplinary boundaries intact. In his *Biolinguistics*, Jenkins (2000: 18) quotes Medawar & Medawar's (1978: 166) anecdote of Keats denouncing Newton "for destroying all the beauty of the rainbow by reducing it to the prismatic colours". He proceeds by quoting Francois Jacob's famous explanation for why the outcomes of smaller questions are more promising than insisting on the big questions that has become the mantra of biolinguistics. Jacob explained that

> [science] proceeds differently. It operates by detailed experimentation with nature and thus appears less ambitious, at least at first glance. It does not aim at reaching at once a complete and definitive explanation of the whole universe, its beginning, and its present form. Instead, it looks for partial and provisional answers about those phenomena that can be isolated and well defined. Actually, the beginning of modern science can be dated from the time when such general questions as "How was the universe created? What is matter made of? What is the essence of life?" were replaced by such limited questions as "How does a stone fall? How does water flow in a tube? How does blood circulate in vessels?" This substitution had an amazing result. While asking general questions led to limited answers, asking limited questions turned out to provide more and more general answers.
>
> (Jacob 1977: 1161–1162)

While the big questions are what has brought researchers in so many different fields together in biosemiotics, decomposing their common interests into smaller explanatory hypotheses will be much more difficult for them to achieve. Jacob's mantra works for biolinguists; and they have a much better chance at progressing along their chosen path to gain a better understanding of the faculty of language. But the "semiotic challenge" (Fitch 2009) remains for biolinguistics; and it remains to be seen if future cross-disciplinary collaboration will bring forth any *hybrids* who "can change the future" (Latour 1993: 11).

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★ FORUM ★

Review of the 9th International Conference on the Evolution of Language (Evolang9)

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1. Overview

The 1990's have witnessed a resurrection of an interest in the origins of language (in fact, such an interest had never actually faded). Although pin-pointing the exact triggers behind the initial sparkles is difficult, one may advocate for the integration of a number of scientific advances, including the first computer simulations of the self-organized emergence and convergence of linguistic conventions (Hurford 1989, Steel 1996), the significant progress in the systematic analysis of mtDNA or Y chromosome genetic distributions across the world (Cann *et al*. 1987, Underhill *et al*. 2000), the synthesis of the data from genetics, archaeology, and linguistics (Cavalli-Sforza *et al*. 1988, 1992), and many others.

In 1996, the first Conference on the Evolution of Language (Evolang) was held in Edinburgh for the purpose of fostering a dialog between scholars of diverse backgrounds. At the center of discussions — and in opposition to a generativist framework minimizing the value of such an attempt (Chomsky 1972, Berwick 1998) — laid an effort to account for the properties of the faculty of language in light of modern evolutionary theory (Hurford *et al*. 1998). The 9th Evolang conference (Evolang9), which took place in Kyoto 13–16 March 2012, was once again an opportunity for scholars from a wide range of disciplines to gather and bridge their lines of arguments (McCrohon *et al*. 2012, Scott-Phillips *et al*. 2012).

Since the origins and evolution of language have long been the research foci in both evolutionary linguistics and biolinguistics, we provide here a review of the variety of reports that was brought forward during Evolang9. Without being able to pay justice to the wide scope of all contributions that were made, we mainly summarize and frame the primary arguments that echoed during the conference, highlight significant evolutions of the field both in terms of methods and content, and present our opinions on future research in this line.

2. Approaches and Methods

The Evolang series has consistently been characterized by a high diversity of

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biolinguistics
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approaches and fields. Without being exhaustive, contributions usually cover linguistics (sociolinguistics, language acquisition, physiology of speech, syntax, etc.), logic, game theory, mathematical modeling and computer simulations, genetics, ethology, human and comparative psychology, neuroscience, paleoanthropology, archaeology, philosophy, evolutionary psychology, and developmental biology. Trends however channel the relative weights of these fields from one conference to another. We give below five long-term tendencies we deem of special significance.

The first trend is the decrease in modeling approaches which has taken place between the mid-2000's and recent years. Models and simulations (most often self-organizing multi-agents models), for example, made the bulk of the contributions to Evolang5 and Evolang6 respectively held in Leipzig and Roma (Cangelosi *et al*. 2006). The investigations then revolved around (i) the emergence of compositional structures, and most often how a stable order for subjects, verbs and objects could be achieved without central coordination (e.g., Kirby 2000, Smith *et al*. 2003a, Gong *et al*. 2005, 2009), (ii) the impact of embodiment in robots, with noticed endeavors of Luc Steels' teams in Paris and Brussels in building on more sophisticated linguistic theories, such as the fluid construction grammar (e.g., Steels *et al*. 2005, Steels & de Beule 2006, Steels 2011, van Trijp *et al*. 2012), (iii) the impact of socially structured populations (with popular structures, such as scale-free or small-world networks) on the self-organization of linguistic systems or the diffusion of innovations (e.g., Dall'Asta *et al*. 2006, Barrat *et al*. 2007, Gong *et al*. 2008, Ke *et al*. 2008), and (iv) the impact of repeated episodes of learning on the design of linguistic structures (e.g., Kirby 2007, Kirby & Hurford 2002, Smith *et al*. 2003b, Steels 2012). Regarding the last effort, Simon Kirby's *Language Evolution and Computation* team and their *Iterated Learning Model* (ILM) were particularly instrumental in partly shifting models from horizontal linguistic transmission (among a usually 'immortal' population of agents) to vertical transmission (with generations of successively learning and teaching agents shaping a communication system).

Although modeling and robotic approaches were reported during the Kyoto conference (e.g., Gong & Shuai 2012, Smith 2012, Spranger & Steels 2012) — noticeably by plenary speaker Minoru Asada, who emphasized the potential of cognitive development robotics to study language acquisition and more generally simulate child development —, several attendees observed a decline with respect to their former prominence. During a preliminary satellite workshop of the conference, Bart de Boer addressed this issue by stressing three common pitfalls of modeling: (i) fact-free science not referring to outside phenomena, (ii) cargo-cult science, an activity mimicking the procedures of science without delivering results (according to Feynman 1974), and (iii) circularity when a model only explains the data that were used to build it. To avoid these traps and keep modeling successful, de Boer advocated for various strategies. Better validating the models was one of them — with mathematical proofs, sensitivity studies, and model parallelism for internal validation and the prediction of real and non-circular data for external validation. Another direction worth taking was better complementing and re-using existing models, rather than always starting again from scratch — a tendency shared by many modelers. Finally, focusing on ques-

tions raised by non-modelers and attempting at bridging empirical gaps were deemed precious to increase the reliability of modeling (de Boer 2012).

A second trend is the more central position of experimental approaches in the study of language evolution. As noted by Normile (2012), this experimental stance covers a number of fields, from analyzing the online brain activity of stone tool-makers (Stout *et al*. 2008, Stout & Chaminade 2012) to studying how subjects learn an alien language composed of whistles (Verhoef *et al*. 2012). However, one of the most meaningful shifts lies, to us, in the displacement of the iterated learning model from 'silicon-made' subjects to human ones. This step was pioneered among others by Galantucci, with experiments of human subjects learning an artificial language to cooperate in front of a simple task (Galantucci 2005). Interestingly, several talks illustrated how the ILM, which started as a theoretical and modeling framework, had found its way to the experiment room (e.g., Scott-Phillips *et al.* 2010, Kirby 2012, Verhoef *et al.* 2012), perhaps reflecting, in a somehow radical way, de Boer's thinking on models and simulations.

A third evolution of the field relates to the broadening of the spectrum of comparative approaches between human language and animals' communicative systems. For obvious reasons, apes and monkeys have been the center of interest, with many experiments consisting in teaching a human or human-like form of communication (e.g., Patterson 1981, Savage-Rumbaugh 2001) to non-human apes or focusing on their comprehension of others' intentions (e.g., Call & Tomasello 1998, 2008; Heyes 1998; Schmelz *et al*. 2011). Other animal models have however gradually made their way and enjoyed high popularity at the Kyoto venue. Rather distant from humans on the phylogeny of species, birds became center of discussion (Fujita 2012, Katahira *et al*. 2012, Matsunaga *et al*. 2012, Okanoya *et al*. 2012, Sasahara *et al*. 2012, Stobbe & Fitch 2012), with special attention paid on the one side to parrots and keas for their remarkable cognitive abilities (Pepperberg 2010, 2012), and on the other side to a couple of species relevant for their close genetic relationship yet divergent environment (see below): white-rumped munias and Bengalese finches (Takahasi *et al*. 2012). Meanwhile, monkeys and apes were still present, and at a methodological level, keynote speaker Tetsuro Matsuzawa stressed the combination of field experiment — building specific device in the wild to study wild populations of apes manipulating tools (Biro *et al*. 2003) — with participant observation relying primarily on the bound between the ape mother and her child (Matsuzawa *et al.* 2006). All in all, the conference highlighted the strong expertise of various Japanese research centers in animal studies.

A fourth methodological trend was a latent reflection on the scientific paradigms relied on to study the evolution of language. In addition to de Boer's suggestions on successful modeling, Roberts & Winters addressed the development of nomothetic approaches in contrast with idiographic ones. While the latter deal with singular cases, the former draw on large sets of data — spanning over large linguistic, cultural, physical, and other domains — and seek law-like patterns behind 'surface' correlations (Roberts & Winters 2012). Nomothetic approaches have been the subject of recent publicized studies and hot debates among scholars working on the origins and current diversity of modern languages (e.g., Lupyan & Dale 2010, Atkinson 2011, Bybee 2011, Dunn *et al*.

2011). Since the Evolang conferences rather focus on the emergence and development of the faculty of language, contributions relying on this methodology remained limited. However, as large datasets in various fields have ever been more and more available and manipulable, there are reasons to believe that such contributions could become influential in future venues. Nonetheless, Roberts & Winters warned against the pitfalls of this line of work, where poor quality of data (e.g., in terms of sampling), spurious correlations and lack of alternative hypotheses may all lead to wrong conclusions (for further details, see www.replicatedtypo.com). Statistical problems linked to the non-independence of the statistical units of a study — whether due to the historical relatedness of languages or their spatial distribution with possible geographic diffusions — prove to be especially difficult (Jaeger *et al*. 2011), as also noted by Russell Gray during his keynote lecture regarding his work on linguistic Bayesian phylogenies (Gray *et al*. 2009). Integrating different approaches — nomothetic, idiographic, constructive — is seen as the best way forward to compensate the weak explanatory power of the first approach — correlation does not imply causation —, the limited range of the second and the potential circularity of the last.

The final point we want to make regards brain imagery techniques applied to activities related to communication and language evolution. EEG (encephalography) or fMRI (functional magnetic resonance imaging) are of course ubiquitous in today's neuroscience, but original studies are gradually appearing which focus on the evolution of language. Takashi Hashimoto thus mentioned studies where simultaneous EEG recording took place in two subjects playing a coordination game (Hashimoto 2012), allowing to observe the neural activity at various stages of the formation of a symbolic communication system. Russell Gray also referred to Stouts and collaborators' experiments where the brain activities of the tool-makers were recorded through PET (positron emission tomography) during sessions of tool-making. This allows detecting significant changes in activated areas for different prehistoric lithic industries (e.g., Oldowan and Acheulean), and possible overlap with language circuits (Stout *et al*. 2008, Stout & Chaminade 2012). Finally, whole-brain fMRI recordings in Zebra finches of neuronal correlates of song learning were presented, showing evolving activations during the course of the sensitive period in primary and secondary auditory areas (van der Kant & van der Linden 2012, Moorman *et al*. 2012).

Given these methodological remarks, we can now turn to the contents of the contributions reported at Evolang9, trying to frame various lines of evidence and disciplines.

3. Designing Language Structures: Disentangling Biology, Culture, Cognition and Learning

During Evolang9, Hajime Yamauchi usefully reframed the famous ban against publications on the origins of language by the Société Linguistique de Paris in its cultural and political context (Yamauchi *et al*. 2012). As in the 1860's, the evolution of the contributions to the Evolang series reflects the dominant forces and structures of the scientific domain.

David Premack's famous quote, "Human language is an embarrassment for

evolutionary theory" (Premack 1985: 281–282), has been used as a subtitle for some of the past Evolang conferences. Generally speaking, these meetings have attempted at providing an answer by disentangling the influences of various frames to which language may belong, including (i) biology (with the genetic substrate of language), (ii) culture (with language existing in a socially constructed community of interacting speakers), (iii) cognition (with language building on and coexisting in the human mind with other cognitive abilities), and (iv) learning (with language being repeatedly learnt and transmitted between generations of speakers).

Such frames are only partially separable from each other, and one may advocate for natural selection as the primary force that drove language evolution, stating that all further effects may ultimately be traced back to genes and their evolution.

Several periods of discussions during Evolang9 actually focused on the role played by natural selection in the emergence of language, with clear evidence that more than twenty years after Pinker & Bloom's (1990) seminal paper on the question, some scholars still opposed to its primacy. Keynote speaker Massimo Piattelli-Palmarini particularly challenged the standard evolutionary perspective, defending instead an evo-devo (evolutionary developmental biology) perspective with minor gene rearrangements and shifts in gene regulation leading to major morphological changes, hence understating the driving role of function for such changes as long as survival and reproduction are preserved. The specific analogy with the eye of the rhopalia jellyfish (Gerhart & Kirschner 1997, Coates 2003) was cited as a complex structure without function by Piattelli-Palmarini, although the question was raised by the discussants of how it could have spread to the entire population without functional advantage — see also Mackie (1999) for further arguments about the functionality of the cubozoan ocelli or 'eyes'.

Irrespective of the actual weight of standard selection, several contributions reminded of the complexity of the phenomena at hand. Yasuhiro Suzuki and colleagues introduced the intricacies of the evolution of herbivore-induced plant volatiles, and how interwoven evolutions of species led to complex dynamics with possible increase or decrease in biodiversity (Shiojiri *et al*. 2010, Suzuki *et al*. 2012). Keynote speaker Simon Fisher furthermore detailed the complexity behind the role of the FOXP2 gene, arguing against the reductionist view of the 'gene for oral language' and stressing the complex set of genetic interactions in which FOXP2 fulfills its functions (Fisher & Scharff 2009, Fisher 2012). Fisher also highlighted some recent advances in neurogenetics, and how this discipline might help in future to decipher the convoluted relationship between the cognitive function of language and its genetic basis.

The subtlety of natural selection beyond the key ideas of genetic variability and selection was particularly addressed during Evolang9 through the notions of masking and unmasking of selective pressure in relation to the process of niche construction. Interestingly, these phenomena were referred to by scientists from various fields, covering modeling and animal studies.

During his concluding lecture, Terrence Deacon gave a clear example outside the linguistic sphere: While many animals synthesize ascorbic acid (vitamin C), anthropoid primates lack this capacity and only possess a non-functional

version of the crucial gene involved in the chemical mechanism. According to Deacon, the primates' fruit diet, rich in vitamin C, explains this evolution: Because this vitamin was readily available 'exogenously' for these animals, the selective pressure on the gene involved in endogenous synthesis relaxed — it was masked — until it lost its function. This in turn bounded primates to their diet, playing a role in the construction of their specific ecological niche. Functions related to living in this niche — especially being efficient in acquiring food rich in vitamin C — hence became under stronger selective pressure. In other words, the selective pressure on such functions was unmasked in the process (Deacon 2003, Wiles *et al.* 2005). Deacon insisted that the whole process was cyclical, with adaptations for niche-maintaining leading to novel functional synergies. He also applied this evolutionary pattern to language, stating that the construction of a symbolic linguistic niche resulted in unmasking specific selective pressures on the human brain while at the same time masking previous ones, hence allowing brain structures to evolve in functionality (Deacon 2012).

Other speakers presented test cases for this framework. The evolution of Bengalese finches (BFs) in Japan with respect to white-rumped munias (WRMs) was especially enlightening. WRMs are wild birds found in tropical Asia and in some parts of Japan; a strain was isolated 250 years ago and domesticated, resulting in today's BFs. Studies devoted to the features of the vocal cultures of both strains, with two colonies recorded over several generations in sound-proof boxes, showed that WRMs kept the colony founders' song through generation while BFs displayed rapid divergence (Takahasi & Okanoya 2010, Takahasi *et al*. 2012). These observations could be explained by a stronger innate bias in WRMs toward specific songs, which in turn is related to the previous notions of masking and relaxed selective pressure: WRMs in the wild are under strong selective pressures to produce songs that will attract conspecifics, while this pressure was relaxed/masked in the domesticated strain. In such studies, evaluating the similarities between birdsongs, or their overall complexity and diversity, can be done with simple or more refined techniques. Katahira *et al*. (2012) relied on hidden Markov models to study the high-order context dependencies in Bengalese finch songs, showing that a first-order model was enough to predict the songs. We can also report here on Sasahara *et al*.'s (2012) approach, which consisted in applying network construction and analysis techniques to the transitions observed between different phrases along song sequences of the species California Thrasher. It appeared that the structural properties of the bird's 'syntax' allowed both familiarity at the local level of the song sequences and novelty at the global level; both aspects were judged useful by the authors, with the first one to establish a singer's identity and the second one to let birds develop virtuosity in their singing.

Another test case came from the modeling efforts attempting at assessing the weights of biology, culture, and learning in the emergence of linguistic structures. A Bayesian iterated learning model of cultural transmission coupled with a mechanism of biological evolution showed that weak genetic biases could be quickly unmasked and stabilized by cultural transmission in a population of speakers, yet never turn into strong biases because of a masking by iterated learning (Kirby *et al*. 2007, Thompson *et al*. 2012). These simulations stand against the postulate that linguistic universals are due to strong innate biases — a 'uni-

versal grammar' (UG) (Chomsky 1965). Instead, they suggest that such universals can rather be explained by weak biases and a coordination of biology and culture regardless of their different evolutionary rates.

Another key concept that was repetitively addressed during Evolang9 was the double articulation of language, with meaningful units (morphemes) built from meaningless units (phonemes) and then articulated in larger structures (sentences and discourses). In his keynote talk, Simon Kirby denoted the first articulation of the duality of patterning, combinatoriality, and the second, compositionality.

The emergence of compositionality was investigated by Kirby and colleagues with a lab experiment involving learning an artificial language — strings of syllables paired with structured graphic meanings. Subjects could get tested on their learning, with their answers then used to teach naive learners, much in the fashion of iterated learning in computer models (e.g., Kirby *et al.* 2008). Different conditions led to different results. Isolated subjects learning a system and transmitting it to the next generation — i.e. vertical transmission but no horizontal transmission —, with an additional and external mechanism to avoid ambiguity, led to the emergence of a compositional communication system. While not preventing ambiguity restricted compositionality to develop, replacing ambiguity avoidance by horizontal transmission — having two subjects for each generation, communicating with one another on the various meanings — restored the previous result. Finally, when vertical transmission was removed and only horizontal transmission took place, compositionality was only limited. These various results showed that a combination of both naïve learners and communication was needed to achieve compositionality. In addition, a fourth study, where structures were learned and exchanged without corresponding meanings, further showed that semantics was not needed for the emergence of repeated subsequences in the strings of syllables.

In order to address the emergence of combinatoriality, getting away from existing languages was needed. Tessa Verhoef and colleagues have addressed this issue by relying on slide whistles used by subjects to produce sounds, the properties of which could be analyzed in terms of combinations, repetitions, etc. Their results suggested that phonemic coding not rely on pressure from large number of signals — an argument behind the hypothesis that an initial holistic proto-language could have evolved as the number of exchanged meanings increased with time. Rather, starting from random sequences of whistles, iterated learning gradually led to whistled elements being reused according to combinatorial constraints (Verhoef *et al*. 2011, 2012).

Combinatoriality, as described by Kirby, was also addressed in a contribution regarding the alarm calls of Campbell's monkeys (Barceló-Coblijn & Gomila 2012). Contrary to popular vervet monkeys' holistic alarm calls (Seyfarth *et al*. 1980), Campbell's monkeys' six calls displayed an internal structure, with the adding of a final –oo resulting in a different meaning ('krak' relates to leopards, while 'krakoo' can be used for almost any disturbance) (Ouattara *et al.* 2009). What looks a priori here as affixation points to the morphology found in human language. However, Barceló-Coblijn & Gomila insisted that the components of the alarm calls not share all the features of human morphemes. On the one hand,

the final –oo does not possess a meaning of its own and the call resulting from the concatenation of, say 'krak' and 'oo', does not have a meaning transparently related to the meanings of its parts. On the other hand, the authors stressed that morphemes are more than minimal units of meanings, and are at the crossing of two processes. The first process is lexicalization, by which concepts are turned into lexical units respecting the 'edge features' of morphemes. These features describe the semantic and syntactic compositional properties of morphemes, and lead to a hierarchical structure of lower and higher meaningful units. The second process is externalization, by which lexical units get a phonological structure. Campbell's monkeys' alarm calls were then defined as pleremes — meaningful signals made of meaningless particles —, relating only to the second process of encoding and compressing information into an external signal.

Barceló-Coblijn & Gomila were not the only participants to remind the audience of the very specific nature of linguistic symbols. Piattelli-Palmarini also mentioned properties of words that made them more than other symbols: aspectual reference, headedness, internal structure, and the previously mentioned edge features.

In the context of Evolang9, the previous considerations on lexicalization and combinatorial properties could be connected more generally to the cognitive context of language evolution. James Hurford commented on *Merge*, which can be said to extend the previous notion of lexicalization and lie at the center of the Minimalist Program inside generative grammar (Chomsky 1993, 1995). Whether this cognitive capacity came before or after externalization is at stake: Externalization enables communication with others, while merge may not only enhance it but also participate in the development of complex private thoughts. Which came first is hard to know, since, as demonstrated by Hurford, a double dissociation exists between having complex private thoughts and possessing a complex communication system. However, biolinguist Cedric Boeckx took side and advocated for communication not playing a role in the initial development of linguistic cognitive abilities (although it later became relevant with cultural transmission). The merging operation was listed along with the edge property and cyclic transfer, or phase, as the three minimally specified syntactic components needed for a plausible UG. Boeckx further introduced the notion of a global neuronal workspace (GNW) to provide a frame in which bridges could be built across previously disconnected cognitive modules; a language of thought, with lexicalization and then merging of concepts, allowed meanings of various natures to integrate (Boeckx 2012). This approach explicitly echoed Fodor's language of thought (Fodor 1975), but was also reminiscent of Fauconnier & Turner's (1998, 2002) scope blending, or Mithen's (1996) cognitive fluidity. The GNW was furthermore rooted in the brain structure and evolution. First, neurons with long-distance connections were seen as central in cross-modules exchange. Second, modern humans' brains evolved to be more globular than our ancestors' (Neubauer & Hublin 2011, Gunz *et al*. 2012), thus leading to easier communication between on average spatially closer areas. No matter whether it derived from constraints linked to locomotion, bite force, cognition, and so on, according to Boeckx, the evolution of the brain shape provided easier cross-modularity.

Other contributions detailed the evolution of language in the brain and

alongside other cognitive abilities. Some talks focused on non-linguistic capacities in animals, like Kazuo Fujita's search for meta-cognition (Fujita 2012), or Moore's (2012) and Froese *et al*.'s (2012) studies of primates' depth of analysis of others' actions, whether or not in the context of communication. As usual, co-evolution enjoyed popularity, with various proposals. Invited speaker Tao Gong attempted at simulating the co-evolution of language acquisition and joint attention (Gong & Shuai 2012), while Michael Arbib (2012) and Russell Gray put forward the now classical relationship between language, gesture, and tool use. The results of the previously mentioned PET recordings of tool-makers were particularly stressed by Gray: The manufacture of late Acheulean tools, but not of older Oldowan or even of early Acheulean tools, resulted in increased activation in areas of (i) the parietofrontal praxis circuits in both hemispheres and (ii) the right hemisphere homologue of Broca's area. The hierarchical complexity of the organization of actions in the later tools correlates with the syntactic features — among others recursion — of modern language.

Tetsuro Matsuzawa gave an example illustrating the idea that abilities may not always get reinforced in a co-evolutionary fashion: His trade-off theory of memory and representation indeed articulates the acquisition of language and the strong decrease in eidetic imagery in humans, with the backup of experiments demonstrating the highly efficient eidetic memory of chimpanzees (Inoue & Matsuzawa 2007).

Finally, the social and cultural frame of language was considered through the prism of psychology, as well as of linguistics, animal studies or models.

At the core level of interactions, Matsuzawa insisted on the significant consequences of the differences in mother-child bonding between primates and humans. While baby primates are clinging to their mothers during the first months of their lives, early physical separations in humans allow face-to-face communication, vocal exchange, and early object manipulation. Cries in human babies are absent in primates, where the young by themselves move to reach their mothers' breasts.

At a larger scale, models tend to focus on the co-evolution of social and linguistic conventions. Models have evolved from homogeneous populations to structured yet static communities (e.g., Nettle 1999), before the introduction of more dynamical ties between agents (e.g., Gong & Wang 2005, Gong 2010). Bachwerk & Vogel (2012) presented a model with social ties continuously updated based on the success of previous interactions. Using a control parameter defining how cautious/impulsive the agents were to establish friendship (that is, reinforcing their tie with another agent) upon successive communication, the authors concluded that a high social update rate (making friends quickly and also forgetting older friends faster) paralleled sociological observations, and was very likely in early hominids, despite raising questions regarding the possibility to then build systems of conventions at a large scale.

In addition to building friendship and cooperation, the role of conflicts and competition between individuals was also considered in the emergence of language. The possibility of cooperative behavior under natural selection at the individual level has long been questioned (e.g., Axelrod & Hamilton 1981), and simulations like the previous one often leave this problem aside, although it

applies to the emergence of language as a specific form of cooperation based on exchanging information. Jacob Foster elaborated on recent works on the evolution of human cooperation, emphasizing intergroup competition as a factor favoring intra-group cooperation (Boyd & Richerson 2009). He considered language in this context as a catalyst for other intra-group cooperative behaviors and an accelerator of cultural differentiation (Foster 2012).

These different studies all show that carefully consideration of social structure is necessary, both to remind of the inter- and intra-group relationships that prevailed during hominid prehistory, and to account for the specific social distributions observed today, like scale-free or small-world networks, or quantitative observations like Dunbar's (2010) number of 'relationships'.

The socio-cultural environment of our hominid ancestors was finally addressed by a few contributors, although one may consider that as in previous Evolang conferences, this line of research did not prove as present as it perhaps should be: Indeed, theories and models about languages in animals and modern humans always run the risk of diverging from the actual course of prehistory. Archeological and paleo-anthropological data are safeguards against attractive but ultimately artificial evolutionary scenarios, but they also suffer from the complex chains of inferences needed to go from often scarce material remains to behaviors and collective thinking. This was apparent in Cuthbertson & McCrohon's (2012) re-reading of evidence on sea-crossings, leading them, contrary to others (Davidson & Noble 1992, Morwood & Cogill-Koez 2007), to deny the need of a sophisticated language to account for this behavior. In a similar fashion, Johansson (2012) reviewed the evidence for Neanderthal's language, building on data which lead to a variety of interpretations — likely depending on the intuitions of the scholars making use of them. A recurrent problem therefore lies in the integration of such data with other analyses of language evolution.

4. Future Research on the Evolution of Language

What conclusion may be drawn from the previous sections in terms of future research on the evolution of language, and can suggestions be made regarding potentially fruitful explorations?

First, the experimental trend on communication/coordination games is likely to develop in the coming years and strengthen itself as a fruitful paradigm. Just as computer simulations gradually shifted from the emergence of 'simple' linguistic conventions (holistic words, vowels, word orders) to more refined linguistic constructions (say, the expression of space; Spranger & Steels 2012), we may expect future games to focus on more specific linguistic domains (Steels 2012). They will then touch more closely on the grammatical devices used in modern languages and how such devices may have emerged in the past, thus connecting to similar attempts by 'traditional' linguists (e.g., Carstairs-McCarthy 1999, 2010; Heine & Kuteva 2007). However, one may wonder if they will not meet the same difficulties as some current models: as games grow in complexity, deciphering and presenting the emerging processes at hand become difficult. As one describes a formerly unknown language, providing a synchronic description of its linguistic processes can prove daunting; adding the additional layer of com-

plexity that creates diachrony and emergence often brings more issues than solves problems.

Recording 'online' brain activities as people engage in communicative activities seems another exciting avenue for research. With the simultaneous recording of several subjects, correlating synchronization at the psychological, linguistic, and neuronal levels becomes possible, which in a way opens the door to the idea of "neuro-pragmatics".

Integrating replicative archaeology and brain imagery, analyzing neural patterns of activities such as tool-making at the light of language-related brain areas also appear attractive. Tool-making and the related, precise control of motor actions are appealing in regard of the fine motor control needed for speech, but what other activities could be further studied? The Symbolic Revolution around 50,000 years before present, as observed by archaeologists in Europe and independently of its exact causes in the broader context of *Homo sapiens* emergence in Africa (Conard 2010, d'Errico & Stringer 2011), suggests looking at the making of more artistic and symbolic objects like anthropo-morphic or zoomorphic sculptures, for example, the ivory lion-man of Stadel-Höhle im Hohlenstein or the Venus of Hohle Fels (Conard 2009), or music instruments like flutes (Higham *et al*. 2012). What are the psychological and neurophysiological differences between making a tool and making a piece of art? Does an additional amount of imagination and creativity get reflected in the brain activations continuously or intermittently during the making process of the latter? Do we observe a clear distinction as between Oldowan and Acheulean, or a continuum going from purely 'functional' tools — that is, whose only goal is, say, to scrap meat, but not to carry symbolic meanings — to tools with symbolic markings to 'non-functional' objects like figurative sculptures?

Focusing on the neural aspects of the evolution of language also suggests addressing more closely the neurophysiology of language production and perception. Indeed, the neural bases of our communication system not only cover high-level cognitive functions, but also lower-level sensory and motor abilities that are essential and sometimes unique to our species. The neurophysiology of the emergence of *speech* has been addressed by some scholars (e.g., Kay *et al*. 1998, MacNeilage 1998, DeGusta *et al*. 1999, McLarnon 1999, Davis & MacNeilage 2004), though their focus has been mostly on the production. Although the issue was rather left aside during Evolang9, Shuai & Gong (2012) addressed the perceptual side by shedding some light on categorical perception, the functional lateralization of which was considered in the broader framework of language evolution (Wilkins & Wakefield 1995, Gannon *et al*. 1998, Cantalupo & Hopkins 2001, Botha 2003).

Departing from the preceding topics, another option for future research lies in semiotic approaches to early forms of symbolism (Coupé 2012). This line of thinking has been partially explored by palaeo-anthropologists (e.g., Henshilwood & Dubreuil 2009, Rossano 2010), but the investigations are often restricted to the surface of semiotic science — like Peirce's notions of icon, index, and symbol — and could make a better use of the typologies of signs established by semioticians (e.g., Peirce 1998, Farias & Queiroz 2003). Just as some speakers insisted on the special semiotic status of linguistic units with respect to others

symbols, one could question the specificities or archaeological artifacts as signs, or investigate whether the semiotic specificities of linguistic units also apply to them.

Finally, given the emphasis on the complexity of the relationship between the genotype and the phenotype, one may look for more realistic models of biological evolution in simulations integrating biology, culture, and learning. Many results on strong or weak innate biases behind today's linguistic universals are based on rather simple — if not sometimes simplistic — models of genetic regulation. One may therefore ask whether significantly different outputs could be obtained with designs involving gene networks rather than more independent genetic units.

As a conclusion, it appears that research on the evolution of language successfully follows an integrative path when it comes to the methods and fields involved. Concepts previously designed for the sole field of modeling — like iterated learning — have met the experimental field with success. Replicative archaeology, which previously helped understand our ancestors' past behaviors (including language) has now been benefiting from brain imagery techniques. Animal studies start to apply these techniques too, as well as network analysis. Theoretical notions of the Minimalist Program are now said to find their roots in the past evolution of brain shapes. To us, this is a strong sign of the vitality of the field, whose actors already plan to meet at Evolang10 in Vienna in 2014.

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A Statistical Investigation into the Cross-Linguistic Distribution of Mass and Count Nouns: Morphosyntactic and Semantic Perspectives

Ritwik Kulkarni, Susan Rothstein & Alessandro Treves

We collected a database of how 1,434 nouns are used with respect to the mass/count distinction in six languages; additional informants characterized the semantics of the underlying concepts. Results indicate only weak correlations between semantics and syntactic usage. In five out of the six languages, roughly half the nouns in the database are used as pure count nouns in all respects; the other half differ from pure counts over distinct syntactic properties, with fewer nouns differing on more properties, and typically very few at the pure mass end of the spectrum. Such a graded distribution is similar across languages, but syntactic classes do not map onto each other, nor do they reflect, beyond weak correlations, semantic attributes of the concepts. Considerable variability is seen even among speakers of the same language. These findings are in line with the hypothesis that much of the mass/count syntax emerges from language- and even speaker-specific grammaticalization.

Keywords: cross-linguistic variability; language universals; mass/count syntax; multi-dimensional scaling; mutual information

1. Introduction

The mass/count distinction between nouns, in various languages, has been discussed in the linguistic literature since Jespersen (1924), and has received considerable attention in particular in the last 35 years (see the bibliography in Bale & Barner 2011). This distinction between mass and count nouns is a grammatical difference, which is reflected in the syntactic usage of the nouns in a natural language, if it makes the distinction at all (as has been often noted, not all

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language do; in the Chinese language family, for example, all nouns are mass). For example, in English, mass nouns are associated with quantifiers like *little* and *much* and require a measure classifier (*kilos*, *boxes*) when used with numerals; on the other hand, count nouns are associated with determiners like *a(n)*, quantifiers like *many*/*few* or *each*, and can be used with numerals without a measure classifier.

These syntactic properties are intuitively correlated with semantic properties. Typical count nouns denote sets of individual entities, as in *girl*, *horse*, *pen*, while typical mass nouns denote 'substances' or 'stuff', for example, *mud*, *sand*, and *water*. It has often been noted that the correlation is not absolute, and that there are mass nouns which intuitively denote sets of individuals (e.g., *furniture*, *cutlery*, *footwear*). Nonetheless, the correlation seems non-arbitrary and there has been much discussion of this correlation in the linguistics literatures as well as in the psycholinguistics literature (e.g., Soja *et al*. 1991, Prasada *et al*. 2002, Barner & Snedeker 2005, Bale & Barner 2009) and in the philosophical literature (e.g., Pelletier 2011 and references cited therein).

Within the semantics literature, a seminal attempt to ground the syntactic distinction semantically is Link (1983). Link proposed that mass nouns are associated with homogeneity and cumulativity, while count nouns are associated with atomicity. Homogeneity, cumulativity, and atomicity are properties which can be associated with matter or with predicates. An object is atomic when it has a distinguishable smallest element which cannot be further divided without compromising the very nature of the object, and an atomic predicate denotes a set of atomic elements. Thus *boy* is an atomic predicate, since we can easily identify atomic boys, parts of which do not count as boys. Homogeneity is a property by which, when parts of an object are separated, each individual part holds the entire identity of the original object, and a homogeneous predicate is one which denotes entities (or quantities of matter) of this kind. For example, any part of something which is water is water, thus *water* is a homogeneous predicate. Cumulativity is the property that a predicate has if two distinct entities in its denotation can be combined together to make a single entity in the denotation of the same predicate. For example, if A is water and B is water, then A and B together are water. Cumulativity and homogeneity can be seen as different perspectives on the same phenomenon, though linguistic research has shown that the difference between them is important in certain contexts (see e.g., Landman & Rothstein 2012). However, for our purposes, we can ignore these differences. The generalization emerging from Link (1983) is that mass nouns are non-atomic and exhibit properties of being homogeneous and cumulative, whereas count nouns are atomic.

Link's proposal has been hugely influential, giving a representation to the intuition that the syntactic expression of the mass/count distinction correlates with a real semantic or ontological contrast. Expressions of this intuition are widespread. Thus Koptjevskaya-Tamm (2004) writes about the mass/count distinction: "In semantics, the difference is between denoting (or referring to) discrete entities with a well-defined shape and precise limits vs. homogeneous undifferentiated stuff without any certain shape or precise limits" (p. 1067).

Despite this ingrained intuition, it has been generally recognized that it is not possible to postulate a simple projection of the homogeneous/atomic or

undifferentiated/discrete distinction onto mass/count syntax (seem e.g., some recent references such as Gillon 1992, Chierchia 1998, 2010, Barner & Snedeker 2005, Nicolas 2010, Rothstein 2010, Landman 2010, as well as Koptjevskaya-Tamm 2004). There are various pieces of evidence which show this. In the first place, there are mass nouns which denote sets of atomic entities, such as *furniture* and *kitchenware*, and some of these have synonyms in the count domain as in the English pairs *change/coin*(*s*), *footwear/shoe*(*s*), *carpeting/carpet*(*s*) which denote roughly the same entities. Conversely, there are also count nouns such as *fence* and *wall* which show properties of homogeneity (Rothstein 2010). Secondly, nouns stems may have both a count and mass realization in a single language, with the choice depending on context. In some cases, both count and mass usage are equally acceptable, as with *stone* and *brick* and *hair* in English. In other cases, one of the uses is considered non-normative, for example, when a count noun like *dog* is used as a mass noun in *After the accident there was dog all over the road*. Thirdly, items which are comparable in terms of lexical content do not have stable expressions cross-linguistically as either mass or count. The much cited examples is *furniture*, which is mass in English but count in French (*meuble/s*), while in Dutch and Hebrew, the comparable lexical item has both a mass and a count realization (Hebrew: count *rehit/im* vs. mass *rihut*, Dutch: count *meuble/s* vs. mass *meubiliar*).

The received wisdom therefore oscillates between these two perspectives, with much recent research trying to mediate between them, both capturing the basic generalization, while accounting for the variations both cross-linguistically and within a single language. Chierchia (2010) suggests that the mass/count distinction is based on whether or not the noun is envisaged to have a set of stable atoms. Rothstein (2010) argues that semantic atomicity is context dependent. Pires de Oliveira & Rothstein (2011) argue that the mass/count alternation is a reflection of whether the noun relates to its denotata as a set of entities to be counted or as a set of quantities to be measured.

However, in the midst of all this discussion, certain basic facts remain unclear. In particular, how great is the cross-linguistic variation in mass/count syntax? Clear evidence that the syntactic mass/count distinction is not a projection of a semantic or ontological distinction has stayed at the level of the anecdotal, with discussion focusing on a few well-known and well-worn examples (see, e.g., Chierchia 1998 and Pelletier 2010 for reviews). As a consequence, most discussions of the basis of the mass/count distinction have been based on some explicit and some tacit assumptions, which have not been verified empirically. In particular, it is often assumed that the mass/count distinction is essentially binary, that is, that a noun is classified as mass or as count or as ambiguous. (This is explicit in accounts which assume that nouns are labeled as mass or count in the lexicon, and implicit in accounts such as Borer (2005) which assume that noun roots are not classified lexically but naturally appear in either a count or a mass syntactic context.) Another, related, common assumption is that in a language with a mass/count distinction, most nouns are either mass or count, with the syntax reflecting the homogeneous/atomic distinction, and that cross-linguistic variation occurs in a lexically defined 'grey area' in the middle, which includes nouns which are not easily classifiable. But crucially, discussion of the facts of the

matter has not gone far beyond the anecdotal. The semantics literature has discussed in great depth the syntactic properties of nouns like *furniture* and comparing it syntactically and semantically with its cross-linguistic counterparts, but despite very few more in-depth, but still narrow, studies (e.g., Wierzbicka 1988), we have little sense of how representative nouns like this actually are.

An answer to the question to what degree there is cross-linguistic variation in the expression of the mass/count distinction is essential to the discussion of its cognitive and semantic basis. If there is ultimately little cross-linguistic variation, then we are entitled to hypothesize that there may be some general strong correlation between properties of the denotata (e.g., as atomicity and homogeneity) and the grammatical distinction. In this case, the grammatical mass/count distinction may have a sound cognitive/perceptual foundation, and its semantic interpretation would reflect this. The task of linguistics would then be to characterise precisely the semantic basis of the grammatical distinction, to identifying 'exceptional' areas where the correlation does not hold and/or where cross-linguistic variation naturally appears, and to try and explain why these occur. This is an approach which has been exploited especially with respect to 'furniture nouns' which has been identified as 'super-ordinates' (Markman 1985) or functional artifacts (Grimm & Levin 2011). On the other hand, if cross-linguistic variation is wide, then the basis for assuming that there is a correlation between cognitive/perceptual features and the grammatical distinction is considerably weakened. Then questions that linguistics should be asking will depend directly on the nature of the patterns, or lack of them, that an analysis of the cross-linguistic facts of the matter reveals. The lack of any quantitive data on the extent of cross-linguistic variation is thus highly problematic.

With the goal of remedying this lack of data and contributing to understanding the cognitive aspects of mass/count syntax and the relation between grammatical, semantic, and cognitive differentiations in this domain, we have conducted a statistical cross-linguistic empirical study based on a quantitative approach, and also a corpus study on the Browns section of the CHILDES database (MacWhinney 1995). We hope with this to be able to begin to answer several basic questions: To what extent is the mass/count distinction a straightforward reflection of the semantic properties of nouns? Is the variability across languages in any degree predictable, or is the grammatical division into mass and count arbitrary? Furthermore, is the division into mass and count absolute, or are some nouns 'more count' or 'more mass' than others? Do differences in the semantic explanations essentially arise due to the multi-dimensional nature of the semantic (as well as the syntactic) space? And if so, can the multi-dimensional aspect provide useful insights in the acquisition of mass/count syntax in humans?

Our study aims to go some way to providing empirically substantiated answers to these questions. We carried out a relatively large scale analysis of the mass/count classification of nouns cross linguistically. Count nouns are usually distinguished from mass nouns by a number of different syntactic properties, for example, co-occurrence with numerical expressions, co-occurrence with distributive quantifiers like *each*, and so on, but the specific tests vary from language to language. We focused on several issues:

(i) To what extent can mass/count syntax be predicted in language A on the basis of knowledge of language B?
(ii) To what extent is mass/count syntax a binary division (i.e. if a noun classifies as count on one test, what are the odds that it will classify as count on all tests)?
(iii) To what extent can mass/count syntax be predicted on the basis of real-world semantic properties?

2. Methods

2.1. Data Collection

2.1.1. Noun List

Binary syntactic usage tables were compiled for a list of 1,434 common nouns in English, which included 650 abstract and 784 concrete nouns. The list was derived from a longer list of 1,500 very frequent English nouns, originally extracted from the CELEX database (see http://www.ldc.upenn.edu/Catalog/CatalogEntry.jsp?catalogId=LDC96L14) for a different project, integrated with about 150 additional nouns often used in linguistics to study the mass/count domain, after translating the nouns into the five other languages included in our study, and eliminating over 200 nouns for which either the identification of the common semantic concept, or the syntactic classification in at least one language, as described below, were unclear or problematic. At the translation stage, each noun/concept was provided with a sample usage sentence, to disambiguate its potentially divergent meanings; thus trying to ensure that each language had the same semantic concept translated, for the same context, into a corresponding noun.

2.1.2. Usage Tables

A set of yes/no questions was then prepared, in each language, to probe the usage of the nouns in the mass/count domain. The questions asked whether a noun from the list could be associated with a particular morphological or syntactic marker relevant in distinguishing mass/count properties. Some questions were designed to give positive properties of count nouns (e.g., can N be directly modified by a numeral?) and some to give positive properties of mass nouns (e.g., can the noun appear in the singular with measure expressions?). Since the mass/count distinction is marked by different syntactic properties cross-linguistically, the questions were dependent on the particular morpho-syntactic expressions of mass/count contrast in each language. For example, in English we asked whether a noun could appear with the indefinite determiner *a(n)* but this was obviously an inappropriate question to ask in Hebrew where there is a null indefinite determiner. The questions in English are shown in Table 1 below.

The questions were answered by native speakers of each of the languages in our study. Thus each noun was associated, for each informant, with a string of binary digits, 1 indicating yes and 0 indicating no, reporting how that particular

noun is used (or predominantly used) in the mass/count domain, by that informant. Such usage tables (a tiny portion of an English usage table is shown as Table 3 below) were compiled by Armenian, English, Hebrew, Hindi, Italian, and Marathi informants (at present, we have complete data for 16 informants; Armenian: AN, AR, GR, GY, RF; Italian: LE, FR, GS, RS, BG; Marathi: SN, TJ, SK; English: PN; Hebrew: HB; Hindi: MN). Although the choice of languages was ultimately determined by the available informants, the languages studied represent a spread across language families. The five Indo-European languages come from distinct branches: Germanic (English), Romance (Italian), Northern Indo-Aryan (Hindi), southern Indo-Aryan (Marathi), and Armenian, which constitutes a branch of its own. Hebrew comes from a distinct phylum, the Semitic family.

No.	Syntactic Questions
1.	Can the noun be used in bare form?
2.	Can the noun be used with *a/an*?
3.	Can the noun be pluralized (in a morphological distinct form)?
4.	Can it be used with numerals?
5.	Can the noun be used with *every/each*?
6.	Can the noun be used with *many/few*?
7.	Can the noun be used with *much/little*?
8.	Can the noun be used with *not much*?
9.	Can the noun be used with *a lot of*?
10.	Can the noun be used with a numeral modifier + plural on kind?
11.	Does the noun appear in the singular with a classifier or measure phrase?

Table 1: List of questions used in English to compile the usage table.

The questions probe whether a particular noun is associated with certain typical syntactic markers, important in English for the mass/count distinction. Similar questions were used for other languages, formulated according to the morphosyntactic properties of the languages in question. These are listed in tables A1–A5 in the Appendix.

2.1.3. Semantic Table

A similar table was prepared by five informants (KM, RI, SL, SU, and TJ, four native Marathi and one Hindi speaker) using the English database to describe the properties of the denotations of the nouns in the list. These questions probed aspects of the denotations which were plausibly related to the more general semantic properties of atomicity, homogeneity and cumulativity discussed above. The questions asked (also supplied with an example to each, to clarify the meaning) are shown in Table 2. The questions were purposely formulated in informal terms, since we were interested in the correlation between mass/count syntax and what is often taken as the 'intuitively obvious' basis for the distinction. We will somewhat loosely refer to these as 'semantic questions'.

No.	Semantic Questions
1.	Is it Alive irrespective of context?
2.	It is an Abstract Noun?
3.	Does it have a single Unit to represent itself ?
4.	Does it have a definite Boundary, visually or temporally?
5.	Does it have a stable Stationary shape (only if concrete)?
6.	Can it Flow freely (only if concrete)?
7.	Does it take the shape of a Container (only if concrete)?
8.	Can it be Mixed together indistinguishably (only if concrete)?
9.	Is the identity Degraded when a single unit is Divided (only if concrete)?
10.	Can it have an easily defined Temporal Unit (only if abstract)?
11.	Is it an Emotion /Mental process (only if abstract)?
12.	Can it have an easily defined Conceptual Unit (only if abstract)?

Table 2: Questions used to probe the semantic properties of the nouns.

The questions are based on the properties of atomicity, homogeneity and cumulativity, if nouns are concrete. For abstract nouns, the semantics is based on how easy it is to define a unit of the concept. The questions were asked without elaboration, with only a reference example; in the case of question 8, for example, applicable to concrete nouns: Can it be mixed together indistinguishably? [e.g., butter *as opposed to* man*].*

Both syntactic and semantic tables were then processed through the analysis described below.

2.2. Analysis

Nouns in the syntactic usage table of a particular informant were clustered together according to the binary string associated with them. In this way, nouns which have the exact same binary string are grouped together, reflecting the fact that their mass/count syntactic behavior is (considered by that informant to be) the same. Thus each group formed in the usage table is identified with a unique binary string. Informants for each language of course group the nouns according to their own syntactic rules, hence the clusters formed in different languages inform us about mass/count phenomenology in that language. The same grouping procedure can be applied to the semantic table, generating 'semantic classes' (relative to the main features putatively underlying mass/count syntax across languages). The resulting distributions of nouns/concepts in syntactic or semantic classes were analyzed, with the measures described below, for both syntactic and semantic tables.

Noun	Context	1	2	3	4	5	6	7	8	9	10	11
ability	Ability is more desirable than wealth.	1	1	1	1	1	1	1	1	1	0	0
accident	The crash was an accident, not intentional.	0	1	1	1	1	1	0	0	0	0	0
acid	Acid stains clothes.	1	1	1	1	1	1	1	1	1	1	1
act	The flood was an act of nature.	0	1	1	1	1	1	0	0	0	0	0
act of crime	Murder is always an act of crime.	0	1	1	1	1	1	0	0	0	0	0
activity	A favorite activity was spitting cherry stones.	1	1	1	1	1	1	1	1	1	1	0
actor	Any good actor can play Tarzan.	0	1	1	1	1	1	0	0	0	0	0

Table 3: A small section of the usage table for English as filled by a native informant. Numbers in the top row refer to the syntactic questions in Table 1.

2.2.1. Hamming Distance Scale

The data in the usage tables is in principle high-dimensional, containing distinct contributions from each of several syntactic markers. It is possible, however, that much of the relevant mass/count syntax might be organized along one main dimension. We consider the hypothesis that this most important dimension may be defined as the 'distance' from a pure count string, where nouns at different distances might be associated with characteristic combinations of syntactic markers (see Fig. 1 below).

To probe this potential organizing dimension, the high dimensional data is collapsed onto a single dimension. This is obtained by calculating the Hamming distance, or fraction of discordant elements, of each noun (i.e. of each syntactic group) from a bit string representing a pure count noun. A pure count string is one which has 'yes' answers for all count questions and 'no' answers for all mass questions. Hence a noun that has distance 0 from a pure count string is a proper count noun, whereas a noun with all its bits flipped with respect to a pure count string is a mass noun, and has a normalized distance of 1 from the pure count string. Such a noun has answers 'no' to all count questions and 'yes' to the mass questions. By plotting the distribution of nouns on this dimension we expect to be able to visualize the main mass/count structure, to relate easily with a linguistic interpretation. This measure does not strictly reflect the categorical nature of groups defined by a unique syntactic string, in the sense that all nouns with a syntactic string differing at 3 bits from the pure count string are clustered together, irrespective of which are the 3 syntactic markers for each noun. This allows for a coarser but perhaps more intuitive and linguistically more transparent comparison between languages than the mutual information measure discussed below, which is a fine-grained comparison between languages, taking into account all the existing dimensions.

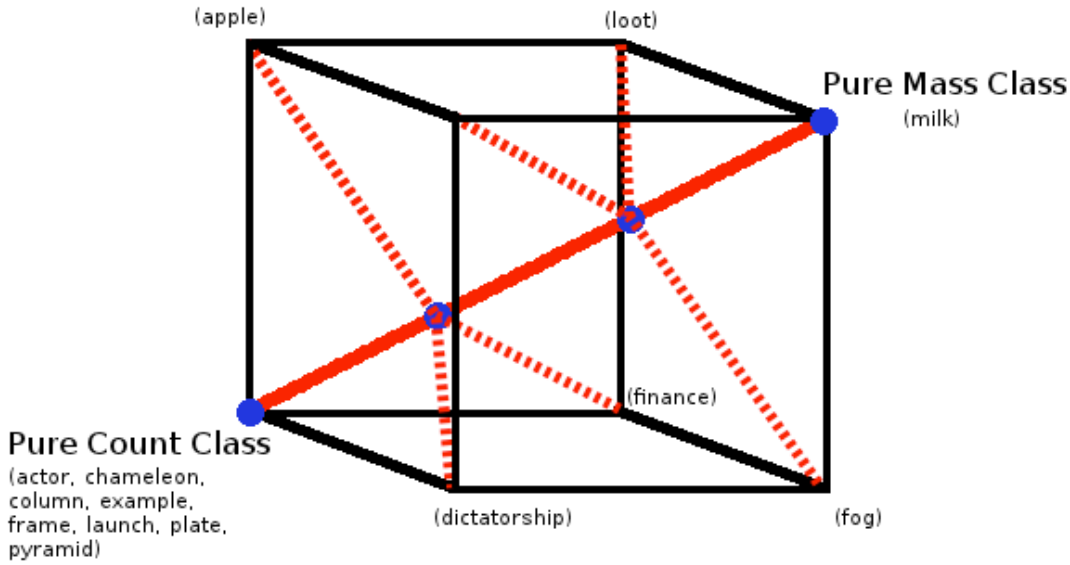

Figure 1: Schematic representation of the Hamming distance scale.

Nouns are located in an N-dimensional space (here only three dimensions are represented) and the Hamming Distance scale projects these points onto the mass/count dimension (red diagonal), going from the bit string of pure count to that of pure mass.

Agreement between two languages is estimated as a variance measure, $\langle x^2 \rangle + \langle y^2 \rangle - 2\langle xy \rangle$ which is simply a sum of squares of the difference between the Hamming distances x and y of a noun from the pure count class, as found in the two languages concerned. This measure has a strict upper bound of 1, if Hamming distances are expressed as fractions of discordant bits, which is attained when each noun is either pure count in one language and pure mass in the other, or vice versa; clearly a rather implausible occurrence. A more natural reference value, although not strictly speaking an upper bound, can be estimated by calculating the variance measure between the Hamming distances in a language and those of randomly shuffled nouns in another language, $\langle x^2 \rangle + \langle y^2 \rangle - 2\langle x \rangle \langle y \rangle$. The random shuffling simulates the case of a total absence of any relation between the position of the nouns along the main mass/count dimension in the two languages, while respecting the distribution of Hamming distances in each. Thus by comparing the actual value with the reference value, we can get an understanding of how the languages match each other in broadly classifying nouns on the main mass/count dimension. Each language however has different number of questions analyzing its mass/count structure and hence the Hamming distance space for a language is populated only at intervals of $1/N^{th}$ of a bit, where N is the number of questions in a language. To minimize the effect of different intervals we estimate a true minimum of variance between

languages (which in an ideal case is 0) by calculating the variance between two languages when all the nouns are ordered in the same way in their position on the Hamming distance scale. We adjust the raw variance by simply subtracting the minimum variance for that pair, and then normalize it by dividing it by the (adjusted) effective maximum value as mentioned above.

2.2.2. Clustering and Information Measures

Information theory provides us with useful tools to quantify aspects of the clustering observed in the data. The entropy of a variable, which can take a certain set of values, quantifies the uncertainty in predicting the value it can take in terms of its possible values and their probabilities. A variable which always takes a single value is perfectly predictable and has an entropy of 0 bits. A binary variable has an entropy of 1 bit when it has 50% probability to take either value, e.g. 1 or 0. We can apply this measure to the grouping structure formed around the mass/count distinction in the languages we study. In our case, the variable *G* is which group any given noun or concept has been associated to in a particular table, taking values 1,…,*i*,…,*n*, where *n* is the total number of groups observed in that table. The probability $p(i)$ is determined for our purposes as the relative frequency of nouns/concepts assigned to group *i*. The entropy of the table is then calculated as:

$$H(G) = -\sum_{i=1}^{n} p(i)\log_2 p(i)$$

H(G) informs us about the overall syntactic variability expressed (by an informant) in a language, and can be regarded as the logarithm of an equivalent number of significant syntactic classes.

To make cross lingual comparisons, we quantify the extent to which the groups formed by informants in one language overlap with the groups formed by those in another. This amounts to defining equivalence classes, whereby two nouns are grouped together if and only if they are members of the same syntactic usage group in the two languages. For example, if the nouns *water* and *wine* are a part of the same group in language X and also fall in one group in language Y, whatever the syntactic usage questions that define groups in the two languages, they are members of the same equivalence class. For analyzing syntactic-semantic relations, language Y is replaced by the semantic table. To give a limiting case, if two languages were to behave exactly the same in classifying nouns in the mass/count domain, the equivalence classes would coincide with the groups formed in the individual languages, reflecting the exact match between groups produced by language X and Y. At the other extreme, if two languages were to share no commonality, there would be no relation whatsoever between the groups in the two languages, and membership in a group in one language would not be informative about membership in the other language.

The mass/count similarity between X and Y can be quantified by the mutual information *I(X;Y)*, a measure that quantifies the mutual dependence of two variables. If two variables share no common information then the mutual information between them is 0, which is the lower bound for *I*, whereas the upper bound on mutual information is the lower between the entropies of the two

variables (the shared information between two variables cannot be more than the total information content in one variable, i.e. its entropy). Mutual information is calculated using the joint entropy of the two variables in question, which in our case is the entropy of the groups, by the relation

$$I(X;Y) = H(X) + H(Y) - H(X,Y)$$

which can be written also

$$I(X;Y) = \sum p(i,j) \log_2 [p(i,j) / p(i)p(j)]$$

and where $H(X,Y)$ is the joint entropy of the two variables, at least equal to the higher of the two individual entropies. In the limit case in which the syntactic groups are identical, $H(X)=H(Y)=H(X,Y)=I(X;Y)$, whereas in the opposite limit case, in which there is no relation whatsoever between the groups each table, $p(i,j)=p(i)p(j)$, expressing independent assignments, and then $H(X,Y)=H(X)+H(Y)$, so that $I(X;Y)=0$.

Mutual information measures suffer from a bias due to limited sampling (Panzeri & Treves 1996) related to the number of equivalence classes actually occupied compared to the total possible ($2^{Nq1} \times 2^{Nq2}$) classes, where Nq1 and Nq2 are the number of questions for the two languages in the pair. The correction to mutual information is estimated by calculating the mutual information between the pairs of languages when the nouns for one pair are randomly shuffled, thus simulating the lack of correlation between the two languages, and then averaging the value over 50 such shuffles. The correction is then subtracted from the raw value calculated for a pair.

2.2.3. Artificial Syntactic String Generation

To test the importance of the mass/count dimension and its link with semantics, an artificial syntactic usage table was also generated, wherein the 'yes/no' decision to a syntactic question was decided by a stochastic algorithm based on the position of the noun on the main semantic mass/count dimension. This algorithm generates a 0 or 1 for each of a string of N_l 'pseudo-syntactic' questions, one string per language, where N_l is the number of syntactic questions in that language. To do so, it uses two reference points, namely the syntactic pure count string for that language and the position of the noun concept along the semantic mass/count dimension, which is taken to be a language universal. The latter is quantified by the Hamming distance from the pure count semantic string, i.e. by the fraction d=D/N of semantic features that differ, for that concept, from those of the pure count. Each bit of the artificial string is then assigned, one by one, for a given noun, the value the bit has in the pure count string with probability (1–d), and the other value with probability d.

Syntactic questions, for this purpose, are empty of content, and simply refer to distinct bits of a pseudo-syntactic usage string. Such bits are determined, for a particular language, by the specific configuration of the pure count string for that language. If the noun is semantically close to a pure count then the probability to generate a syntactic pure count, or something close to it, is higher. The Hamming

distances of the artificial strings from the pure count string have a certain distribution (a convolution with exponentials of the semantic Hamming distance distribution) which resembles that of the real syntactic strings, in most cases (except for Marathi, see below); while the position of each noun along the artificial syntactic mass/count dimension is strongly correlated with the position of the noun along the semantic mass/count dimension. The variance measure between the pseudo-usage table of any language and the semantics table provides us with a lower reference value for the variance itself, in contrast to the upper reference value obtained by random shuffling of the nouns. We are then able to better gauge the significance of the mass/count dimension and the importance of semantics with respect to the mass/count syntax. Also, the mutual information between natural usage tables and semantics can be compared to the mutual information between the pseudo-usage table and semantics, to allow a better estimate of what is the contribution of sheer semantics to the mass count syntax (by providing what for the mutual information scale is a more realistic upper value, see Fig. 12 below). The entropy for a particular language depends also on the number of questions used to investigate the mass/count syntax. By looking at the entropies of the artificial syntax we can see how the entropy measure scales with the number of questions.

2.3. Corpus Study of the Mass/Count Distinction in English

Brown's section of the CHILDES corpus was also used, in an additional component of the study, to obtain mass/count information about nouns occurring in a natural English language corpus. For this purpose all nouns were collected, in the adult-produced sentences of the corpus, which co-occurred with a set of predefined mass/count markers. The co-occurrence frequency of a noun and the set of mass/count markers was recorded and normalized to the total occurrence frequency of the noun. Thus, for each noun, there was a set of numbers which indicated the statistical distribution of syntactic markers for that noun. The markers that were used to measure co-occurrence frequency were *a(n)*, *every/each*, pluralization, *many*, *much*, *some* + sing. N, and *a lot of* + sing. N. This study contains a total of 1,506,629 word tokens and 27,304 word types.

The usage table obtained from the CHILDES corpus was analyzed with multi-dimensional scaling, and the distribution of the nouns on the mass count dimension. Multi-dimensional scaling projects high dimensional data on a lower dimensional space while preserving the inter-data-point distance, allowing to visually identify structural information in the data. By analyzing the distribution and clusters in the projected space one can gain information about statistically important dimensions and markers. Moreover, the data from the CHILDES corpus was analyzed in terms of distribution of distances from the pure count class and of entropy measures, after binarizing the table indicating the frequency of each marker. Thus, for example, if a noun was found at least once in plural form, this was taken as evidence that it could be pluralized; if found at least once with *a* or *an*, that it could take the indefinite article, and so on. In this way, the same analyses could be applied as for our database.

3. Results

3.1. Individual Syntactic Rules and Semantic Attributes Do Not Match

The starting point of our analysis is the observation that, at least in five out of the six languages we considered, roughly half the nouns in the sample can be easily classified as pure count nouns. The exact numbers in each language are Armenian: 1058, English: 693, Hebrew: 757, Hindi: 994, Italian: 863, and Marathi: 255. For example, in both Italian and English nouns like *act, animal, box, country* (as the territory of a nation), *house, meeting, person, shop, tribe, wave*; and *accident, cell* (as in biology), *loan, option, pile, question, rug, saint, survey, zoo* were classified as count in all respects by our informants. In Marathi, while the first 10 examples were also classified as count, the second 10 tested positive on all count properties except one, usually the property of having a morphologically distinct plural form. Marathi appears to stand out from the group in other ways, as reported below. For all other languages, clearly the focus has to be on the remaining proportion of non-pure-count nouns.

Among the informant responses, we observed cases of nouns that were regarded as pure count in English but cannot be normally used with numerals in Italian (*back, forum, grin*), or vice versa that test as pure counts in Italian but cannot be normally used with numerals in English (such as *behavior* or *disgrace*). Interestingly, when considering only usage with numerals and with distributive *each/every* (*ogni* in Italian), our informants classified as 'count' in English nouns the translation of which failed both tests in Italian: *love, noon, youth* have count usages in English, but not in Italian. The converse is also found: there are count nouns in Italian that, translated into English, failed both the numerals test and the test "can be used with *each/every*": *advice, blame, literature, trust, wood*. There were cases where the impression of one of the authors was that his or her judgment might differ from the informants, or the informants disagreed among themselves. Since we are interested in this study in an overall quantitative analysis of cross-linguistic usage judgments, we did not subject these differences of judgments to in-depth linguistic analysis, but entered the judgments of the majority. We note that that there were a significant number of such cases. Overall, there were only 116 nouns that were classified as pure count in all six languages, and still only 392 when excluding Marathi. We thus proceeded to a quantitative analysis, without further questioning the responses by the informants on a noun-by-noun basis.

For a quantitative analysis, we first assessed whether, in any of the languages in the database, a particular syntactic usage rule can be taken to reflect in a straightforward manner a particular semantic attribute of the noun. While in many cases the yes/no answer to a syntactic question turns out to be significantly or highly significantly correlated with a specific semantic attribute, we found no cases where the correspondence could be described as expressing a 'rule', even a rule with a few exceptions. To present quantitative results, we focused on cases where the semantic-syntactic correspondence was higher. The notion of high correspondence is somewhat arbitrary, because for example, one may contrast a case where among 10% of nouns with a particular semantic attribute, 90% admit a

certain syntactic construct, with another case where those proportions are 30% and 70%. In our sample, the first 'quasi-rule' appears stricter, but it applies to only 129 nouns in the sample, whereas the second one, while laxer, applies to 301 nouns. For consistency with later analyses, we focus on relative (normalized) mutual information as a measure of correspondence, while reporting also the number of nouns for which syntax matches semantics. The relative mutual information measure ranges from 0 to 1 and it quantifies the degree to which the variability in the syntax, across nouns, reproduces that in the semantic attributes, both of which are quantified by entropy measures.

Language	++	+–	–+	—	H(Lang)	H(Sem)	MI(S,L)	Norm MI
Armenian	24	31	686	43	0.451	0.366	0.080	0.218
Italian	26	29	662	67	0.536	0.366	0.053	0.145
Marathi	25	30	559	170	0.819	0.366	0.020	0.054
English	29	26	668	61	0.503	0.366	0.046	0.126
Hebrew	29	26	682	47	0.447	0.366	0.055	0.150
Hindi	28	27	686	43	0.434	0.366	0.062	0.170

Table 4: A case of relatively high correspondence between a semantic attribute and a syntactic rule. Semantic question 8, applied only to 784 concrete nouns, asked whether the noun denotes an entity (or individual quantity) that can be mixed with itself without changing properties. (This somewhat loosely phrased question makes reference to the homogeneity and cumulativity properties discussed in section 1, since it can be interpreted either as asking whether proper parts can be permuted without changing the nature of the object, or whether instantiations can be collected under the same description.) The syntactic question considered was whether the noun can be used with numerals, and it was present in all languages. The largest group of concrete nouns, in the –+ class, denote objects that are not homogeneous, and the nouns can be used with numerals. The relative proportion of nouns in each of the four classes, however, yield meager normalized information values, indicating that individual attributes are insufficient to inform correct usage of specific rules, even in this 'best case' example.

Table 4 shows that most concrete nouns in our database (729/784) denote entities that, according to our informants, cannot be 'mixed' while retaining their properties as instantiations of the noun. Most of these nouns can be counted in the sense that they can be preceded with numerals, across languages (with a somewhat less disproportionate bias in Marathi). Nevertheless, among the nouns for which the answer to question 8 was positive, i.e. that displayed properties of either cumulativity or homogeneity, roughly half can be used with numerals, again across languages, yielding rather low values of mutual information between semantics and syntax, as quantified in the last column of the table. Normalized MI values are much closer to zero than to one.

Even though the correspondence with the particular semantic attribute of cumulativity is low, the results above suggest that there might be a high degree of correspondence among the syntactic usage with numerals across languages, at least when excluding Marathi. After all, across languages it is roughly half the nouns denoting entities which intuitively are cumulative, which can be used with numerals, and half which cannot. Is it roughly the same half?

Language pair	++	+–	–+	—	H1	H2	I(1:2)	Norm. MI
Arm–Ita	662	48	26	48	0.451	0.536	0.124	0.275
Arm–Mar	560	150	24	50	0.451	0.819	0.059	0.131
Arm–Eng	666	44	31	43	0.451	0.503	0.106	0.235
Arm–Heb	675	35	36	38	0.451	0.447	0.095	0.212
Arm–Hin	683	27	31	43	0.451	0.434	0.129	0.297
Ita–Mar	548	140	36	60	0.536	0.819	0.062	0.115
Ita–Eng	654	34	43	53	0.536	0.503	0.131	0.261
Ita–Heb	652	36	59	37	0.536	0.447	0.068	0.152
Ita–Hin	661	27	53	43	0.536	0.434	0.102	0.235
Mar–Eng	547	37	150	50	0.819	0.503	0.041	0.082
Mar–Heb	553	31	158	42	0.819	0.447	0.034	0.076
Mar–Hin	556	28	158	42	0.819	0.434	0.037	0.086
Eng–Heb	669	28	42	45	0.503	0.447	0.119	0.266
Eng–Hin	675	22	39	48	0.503	0.434	0.144	0.331
Heb–Hin	675	36	39	34	0.447	0.434	0.078	0.181

Table 5: The correspondence between languages is not higher.

In the same case of relatively high correspondence between a semantic attribute and a syntactic rule, entropy and mutual information between languages yield the relatively low normalized MI values listed in the fifth column, which indicate that a broadly applicable syntactic question ("Can the noun be used preceded by a numeral?") selects different subsets of nouns across different languages.

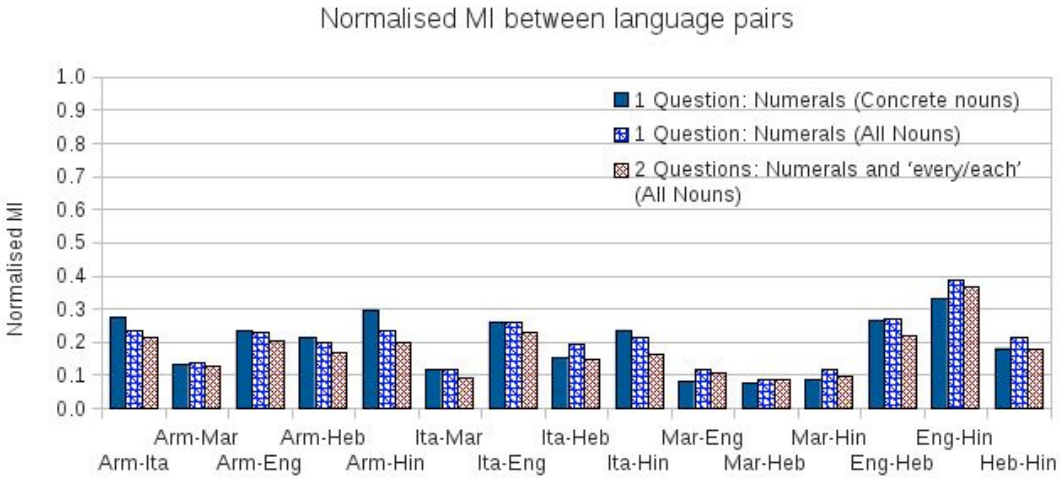

Figure 2: Agreement across languages remains low, however it is measured.

The solid bars show the normalized mutual information between pairs of languages for a single question, on usage with numerals, for concrete nouns only. The stippled bars are for the same measure over all nouns in the database, both concrete and abstract. The patterned bars are for pairs of questions, on both the use of numerals and that of distributive quantifiers such as each/every in English (see text).

Table 5 and Figure 2 show that the naïve expectation is not met by the data. The syntactic correspondence in the usability with numerals is weak across languages, even irrespective of any semantic attribute it may originate from. The congruence (number of concrete nouns in the same syntactic class when translated across languages) appears relatively high, because most nouns can be used with numerals anyway, but, properly quantified in terms of normalized mutual information, the degree of correspondence even excluding the special case of Marathi is roughly in the 15–30% range, with English and Hindi reaching a peak value of 33%. When considering all nouns in the database, including abstract nouns, the degree of correspondence does not change much (stippled bars in Fig. 2). Again excluding the special case of Marathi, it falls roughly in the 20–27% range, with English and Hindi reaching a peak value of 39%.

One may ask whether the low MI values with semantics, in Table 4 above, may be due to the lack of exact match between the semantic attribute considered and the specific syntactic rule. Similarly, one may ask whether the weak correspondence in the pattern of usage with numerals may also be due to the fact that numerals might point in different directions, so to speak, in the syntactic space of each distinct language, for example, atomicity vs. non-homogeneity. To approach these issues, we have begun by considering pairs of attributes, and pairs of syntactic rules. The degree of correspondence of each language with semantics does not change much, and in fact it tends to slightly decrease. For example, when asking whether the object denoted by the noun can flow freely, and also whether it is cumulative, and on the other hand whether the noun can be used with numerals and whether it can be used with distributive quantifiers like 'each' in English, we find that the normalized MI decreases with respect to the above analysis with one attribute and one rule, in all cases except for Marathi (data not shown). The decrease is entirely due to the increase in the entropy that appears in the denominator of the normalization (see Methods). In terms on non-normalized mutual information, instead, adding dimensions reveals perforce more variability.

Similarly, the match between languages, independently of semantic attributes, does not increase when considering two syntactic rules instead of one. Table 6 and Figure 2 report the data, this time for concrete and abstract nouns together, when considering the two syntactic rules above.

Table 6 shows that normalized mutual information values are low, all below 0.23 except for the English–Hindi match, even though 'congruence' values appear high. Congruence is the sum of the number of nouns that are used in the same way in both languages, with respect to the two syntactic constructs considered. Except for pairs including Marathi, between 70–81% of nouns are congruent across pairs. Yet mutual information is low because many of the congruent nouns are simply pure count nouns in either language, accepting both numerals and distributives, and their permanence in the largest class is not very informative about mass/count syntax in the other classes. As considering two questions rather than one does not affect results, it is interesting to ask what happens when considering all available questions together. We first focus on the main mass count dimension.

Language pair	H1	H2	I(1:2)	Norm. MI	Congruency
Arm–Ita	0.862	1.129	0.186	0.215	1119
Arm–Mar	0.862	1.427	0.109	0.127	825
Arm–Eng	0.862	0.872	0.176	0.204	1154
Arm–Heb	0.862	0.940	0.143	0.166	1152
Arm–Hin	0.862	1.242	0.172	0.200	1046
Ita–Mar	1.129	1.427	0.106	0.094	849
Ita–Eng	1.129	0.872	0.199	0.228	1132
Ita–Heb	1.129	0.940	0.141	0.150	1081
Ita–Hin	1.129	1.242	0.182	0.161	1011
Mar–Eng	1.427	0.872	0.094	0.108	882
Mar–Heb	1.427	0.940	0.082	0.087	826
Mar–Hin	1.427	1.242	0.122	0.098	812
Eng–Heb	0.872	0.940	0.191	0.219	1157
Eng–Hin	0.872	1.242	0.320	0.367	1099
Heb–Hin	0.940	1.242	0.169	0.179	1037

Table 6: Congruency and mutual information between languages.

The correspondence between languages is not higher when considering pairs of rules at a time. Here we considered whether a noun can be used with numerals, and whether it can be used with a distributive quantifier such as each/every *in English.*

3.2. Hamming Distance

Plotting the data on the main mass/count dimension (Fig. 3) as the distance from the pure count string shows that a very high proportion of the nouns are at a distance zero from the pure count class (groups are labeled from 1 to N+1 at an increasing Hamming distance of a single bit, where N is the number of questions and group 1 represents pure count nouns). Overall there is an exponential-like decreasing trend in the group frequencies, as we go further from the pure count, for all languages but Marathi. Since this measure does not distinguish between different classes that are at the same distance from the pure count class but vary in the questions that define them, we use different colors in the bars to show the proportions of particular classes at that specific distance from the pure count class. The number of questions, N, is 9 for Armenian, 8 for Italian, 5 for Marathi, 11 for English, 9 for Hebrew, and 5 for Hindi. Since there are N+1 possible groups, we see that for Italian the 9th group is empty, whereas for Hebrew the last two groups are empty.

Distributions in Figure 3 seem to reflect the nature of the nouns as brought out by the questions used to investigate them. In the case of Marathi, the distribution is seen to have two groups of high frequency, at the distance of 1 bit from the pure count class and mass class, respectively. In each of these high frequency groups there is one class that accounts for most of the nouns. The class making up most of 5th group differs from the pure mass class in answering 'no' to the question regarding use of measure classifiers. Upon closer inspection, we find that, out of the 411 nouns that form the largest class in the 5th group, 332 are

abstract nouns, hence answering 'no' to the measure classifier question. The question that differentiates, instead, the largest class in the 2nd group from the pure count class is 'Pluralization with morphological change', to which for nouns in the largest class the answer is 'no'. Figure 4 shows the same distribution, only for Marathi, but restricted to the 650 abstract and to the 784 concrete nouns, respectively. Notice the changes in the frequencies of the 5th group for both concrete and abstract nouns, as compared to Figure 3. For other languages, the distributions restricted to concrete and to abstract nouns look similar to the overall distribution, with a quasi-exponential downward trend (not shown).

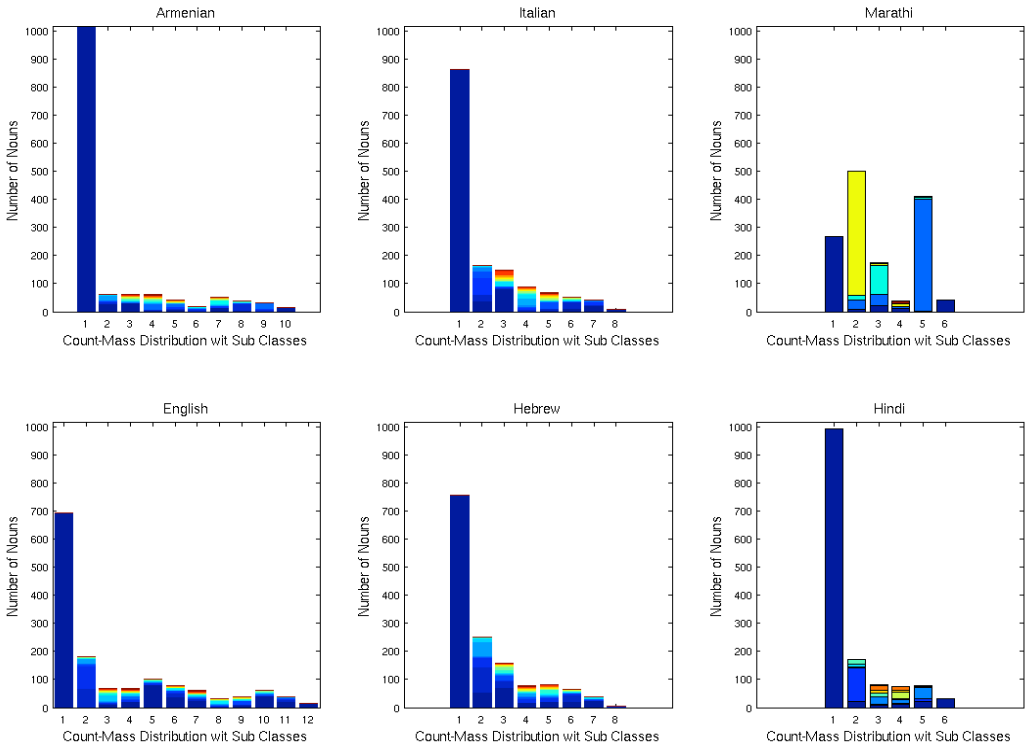

Figure 3: Distribution of nouns along the mass/count dimension.

Each histogram reports the frequency of nouns in the database, for a particular language, at increasing distances from pure count usage (1) and towards pure mass usage (N+1), where N is the number of syntactic question for the language. Colors in the bars indicate the proportion of nouns in each of the syntactic classes occurring at the same Hamming distance from the pure count.

In summary, the distribution of mass/count syntactic properties is undoubtedly graded rather than binary, as might have been intuitively expected. Most common nouns are strictly count in nature, in five of the six languages considered, with mass features increasingly rarer as they approach the pure mass ideal. Marathi differs from the other languages, and it remains of be examined whether it is representative of several other natural languages not considered in this study.

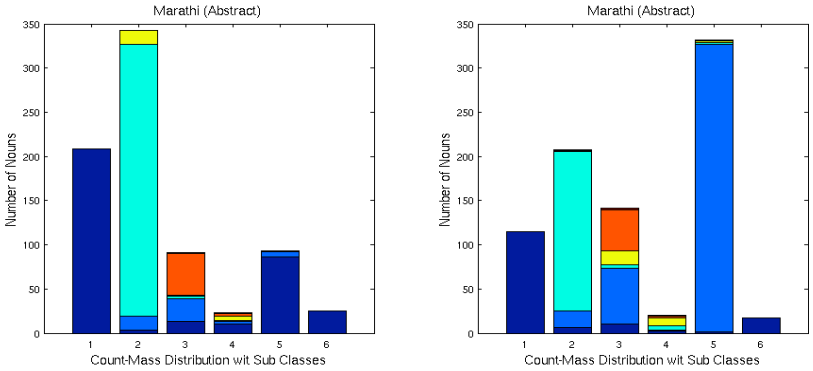

Figure 4: The distribution for Marathi, restricted to concrete and to abstract nouns.

As the 5th group in the histogram in Figure 3 (top right) includes mostly abstract nouns, it dominates the abstract noun distribution (right), while it is considerably reduced for concrete nouns (left).

How distributed are the semantic features characterizing these same nouns? Figure 5 shows that, for concrete nouns, semantic features define a monotonically decreasing distribution from the pure count class, roughly similar to that observed for syntactic usage features in five out of six languages. *Prima facie*, this might suggest that concrete semantics might be the common source that influences the global structure of the mass/count classification, at least for concrete nouns. For abstract nouns, the two semantic questions considered leave most nouns in the 'ambiguous group' — in particular, in the class which includes abstract nouns without an easily definable conceptual unit or a temporal unit. Since the semantics of abstract nouns does not have as clear a definition as concrete nouns, it may not have a strong independent influence on the mass/count syntax of abstract nouns.

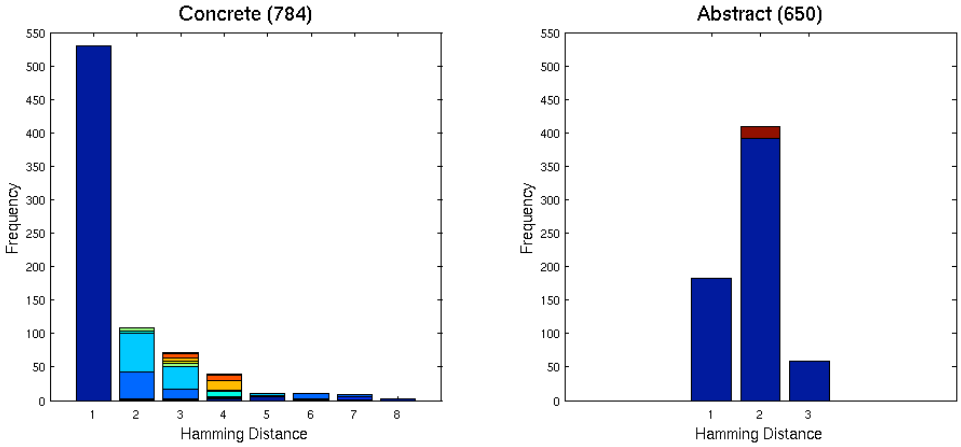

Figure 5: Distribution of nouns on the main mass/count dimension for semantics.

Given the different applicable semantic questions, it is shown separately for concrete (left) and abstract nouns (right). Concrete nouns show an exponential like shape similar to most of those in Figure 3, whereas most abstract nouns are in the ambiguous group.

If semantics serves as the common source of the mass/count syntax for concrete nouns, we expect not only the distributions to look similar overall, but also to include individual nouns at similar positions along the main mass/count dimension, both when comparing semantics with syntax for each of the 'well-behaved' languages, and when comparing the syntax of two such languages. This can be assessed through our variance measure, which quantifies the overall difference between such positions.

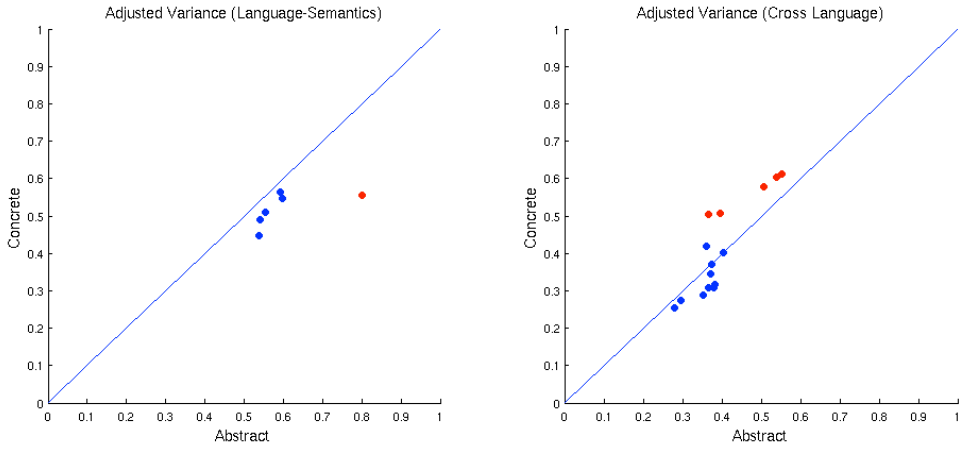

Figure 6: Scatter plots of variance values along the main mass/count dimension.

The adjusted and normalized variance (see Methods) in the position of individual nouns is shown between semantics and syntax (left) and between syntax in pairs of languages (right). In each plot, the adjusted and normalized variance for concrete nouns is on the y-axis and for abstract nouns on the x-axis. Pairs that include Marathi are indicated in red.

This expectation is only weakly borne out by the data. Figure 6 shows our relative variance measure, which is normalized to range between zero (when individual nouns are identically ordered in terms of their distance from the pure count class, either in both of two languages or between semantics and the syntax of one language) and one (when the relative orders are completely unrelated to each other). For concrete nouns, Figure 6 (left) shows that relative variance from semantics hovers around 0.5, halfway to complete lack of any relationship. For abstract nouns, variance values from semantics are somewhat higher. Marathi is an outlier, with yet higher variance from semantics, for abstract nouns. It appears therefore that the relationship to the underlying semantics is not very strong, even when considered solely along the main mass/count dimension.

Between languages, the adjusted variance is less than 40% of the adjusted maximum for all the pairs except for those including Marathi, both for concrete and abstract nouns. Marathi is an outlier, with higher variances from semantics (for concrete nouns) and from other languages. Marathi has higher variance since it does not have the exponential-like distribution as the rest of the languages. The variance values calculated over the entire database tend to be, for each pair of languages, close to the average between the values calculated over concrete and over abstract nouns, separately (not shown). The fact that between languages

variance values are relatively low, relative to those from semantics, may indicate that there is an overall agreement across languages in classifying the nouns in the mass/count domain. This is shown only a gross level, however, along the main mass/count dimension, since here we do not take into account the fine grained differences between the classes at the same distance from the pure count class.

3.3. Mutual Information along the Main Mass/Count Dimension

The results from the analysis of the variance can be verified by considering an alternative measure of the correspondence in the classification, the mutual information. Along the main mass/count dimension, the mutual information can be calculated, e.g. between two languages, by grouping nouns at each Hamming distance from the pure count nouns, rather than each syntactically defined class. The mutual information (Table 7) ranges between zero (when there is no correspondence whatsoever in the groupings) and the minimum of the two entropy values, where entropy is calculated also by putting together all nouns in a group, i.e., at the same Hamming distance from pure count nouns.

Language	**Entropy**
*Armenian	1.63
*Italian	1.96
*Marathi	2.15
English	2.66
Hebrew	2.11
Hindi	1.54
*Semantics	(2.01) 1.58 (C) 1.24 (A)

Table 7: Language–entropy relations

*Entropy values along the main mass/count dimension in the six languages, and for semantics. The * sign indicates an 'average' over five informants (three for Marathi), taken by assigning to each question and each noun the yes/no answer chosen by the majority. For semantics, the overall value (in parenthesis) has little significance, because concrete nouns are assigned to eight distinct groups and abstract to only three, and combining them distributes the abstract nouns into the two extreme concrete groups and one central group.*

Figure 7 confirms, on the different quantitative scale of mutual information measures, the results obtained with the analysis of variance. The normalized mutual information with semantics is quite low, and lower for abstract than for concrete nouns, corresponding to higher variance values. It is at its lowest, 0.016, for abstract nouns in Marathi, which had the highest variance values. Between languages, mutual information is somewhat higher, and not markedly different between abstract and concrete nouns.

To better appreciate the significance of the relatively high variance values we measured, and of the relatively low MI values, we contrasted the values obtained between different languages with those obtained 'within' languages, i.e. measuring the correspondence between different informants of the same language. These data are available for five informants each for Armenian and Italian, and three for Marathi, and also five for the semantics classification. They give thus rise to 10 informant pairs in three cases and three pairs for Marathi.

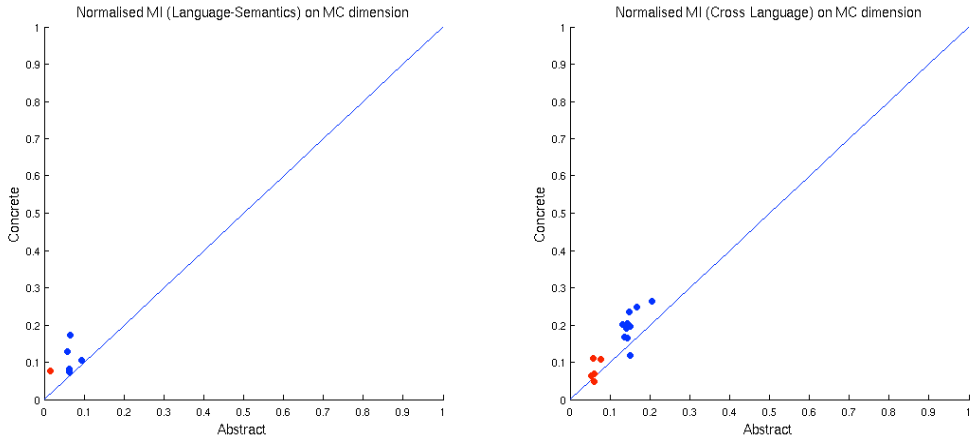

Figure 7: Scatter plots of mutual information values along the main mass/count dimension.

The normalized mutual information (see Methods) between the groups of individual nouns is shown between semantics and syntax (left) and between the syntax of pairs of languages (right). In each plot, the normalized mutual information for concrete nouns is on the y-axis and for abstract nouns on the x-axis. Pairs that include Marathi are indicated in red.

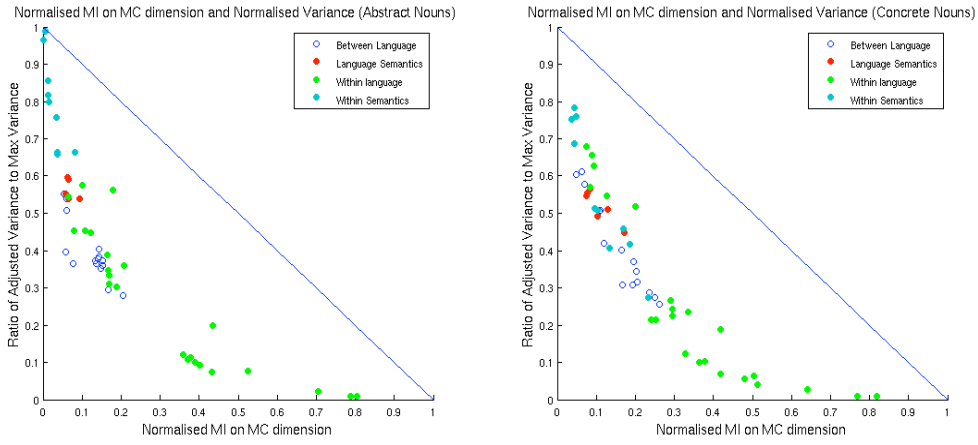

Figure 8: Scatter plots comparing variance with mutual information values.

The normalized mutual information (along the main mass/count dimension) is shown on the x-axis with the corresponding normalized variance value on the y-axis, for abstract nouns (left) on and for concrete nouns (right). Different colors denote data points between the syntax of pairs of languages (empty circles), between the semantics and syntax (red), within language (green) for 10 Armenian, 10 Italian, and three Marathi data points, and within different semantics informants (10 light blue data points).

Figure 8 shows, first of all, that the MI measure and the Variance measure are broadly equivalent. Their relation is (very roughly) Var ~ $(1–MI)^4$. This occurs despite the different nature of the two measures: the mutual information is not sensitive to distance along the mass/count dimension, only to group membership, whereas variance has limited sensitivity to small differences in the exact classification of each noun, as long as its position on the mass/count dimension does not vary too much. Variance turns out to be a more informative measure with our data, which better span its 0–1 range, but mutual information can be easily generalized beyond the main mass/count dimension.

Second, the within language data show mostly more agreement (higher MI and lower Var) than the between language data. Exceptions are due to one Armenian informant (yielding four data points) and one Marathi informant (yielding two more data points) that differ sensibly in their syntactic judgment from the rest. The 'average' data for both Armenian and Marathi, however, due to the majority rule effectively disregards their peculiarities. Thus both measures overall indicate more agreement between informants of the same language than between languages, although this is very far from a clear cut all-or-none difference. Confronted with the requirement to answer yes or no to a set of binary questions, speakers of the same language vary substantially in their responses.

Third, the informants who contributed the semantic classification show the least agreement, particularly for abstract nouns. Even though there were just two questions to answer for abstract nouns, the responses to those two questions are effectively random, with the variance between informants close to its random reference value (in one case exceeding it), and the mutual information close to zero. This suggests that while the semantic properties that should inform the mass/count syntactic usage are already not that salient and self-evident for concrete nouns, they are completely irrelevant for abstract nouns.

3.4. Mutual Information across the Complete Syntactic Classification

Mutual information is however higher when all the dimensions are considered (Fig. 11), even in relative terms, i.e. when taking into account that the entropy values are higher for the full classification (Table 8). Entropy values, as discussed in the Methods section, inform us about the logarithm of the equivalent number of significant classes found in the data. Table 8 shows that the entropies of the languages are in the range of 2–4 bits, which indicates the presence of something equivalent to 2^2–2^4 equi-populated classes of nouns (from slightly above 4 for Hindi to just below 16 for English). In a hypothetical case where there were just two significant classes of mass and count the entropy would have been in the range of 1 bit, in fact even less if the count class were, as it turns out to be in most cases, much more populated. This provides a quantitative estimate of the variability that exists in the mass/count classification, which is much higher than may have been intuitively expected.

Language	Entropy
*Armenian	2.29
*Italian	3.02
*Marathi	2.71
English	3.92
Hebrew	3.40
Hindi	2.12
*Semantics	(3.72) 2.94 (C) 2.34 (A)

Table 8: Entropy values for the full classification in the 6 languages, and for semantics.
*The * sign indicates an 'average' over five informants (three Marathi), taken by assigning to each question and each noun the yes/no answer chosen by the majority.*

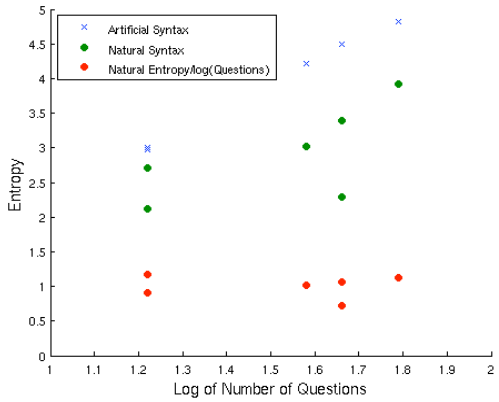

Figure 9: The entropy scales up with the number of questions.
Both when calculated for natural syntax and for the artificial syntactic strings used as controls, entropy values turn out to be roughly proportional to the logarithm of number of questions, hence to yield almost the same value, around 1, when divided by that number.

It is important to note that the entropy and mutual information values obtained with our procedure are influenced by the number of questions used for each language. The scale of the entropy of the 'artificial syntax' depends solely on the number of questions, and we can see from Figure 9 how also the entropy values for natural syntax are strongly correlated with the logarithm of number of questions. Dividing the entropy of natural syntax (Table 8) by the logarithm of the number of question all the entropy values get together at around the 1 bit mark.

The limited agreement that there is, is somewhat stronger for concrete than for abstract nouns except for the 10 within Italian pairs. Figure 10 indicates that this holds within languages, between languages, and much more so when including semantics. As noted above in the case of measures restricted to the main mass/count dimension, the semantic classification of abstract nouns is so

arbitrary that agreement among the five informants that filled the questionnaire is extremely low, and the correspondence of their majority response with any natural syntax is also low.

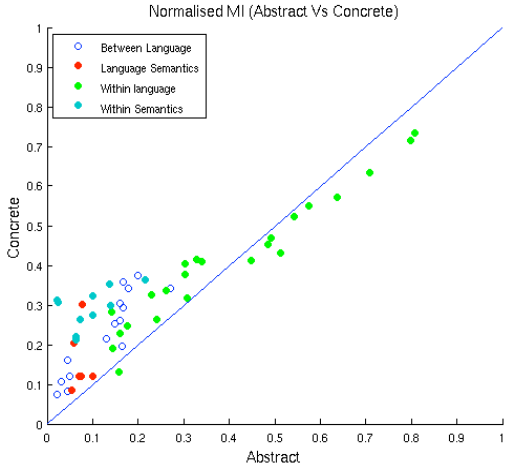

Figure 10: Scatter plots of mutual information values for abstract and concrete nouns.

The normalized mutual information is shown for abstract nouns on the x-axis with the corresponding value for concrete nouns on the y-axis. Different colors denote data points between the syntax of pairs of languages (empty circles), between the semantics and syntax (red), within language (green) for 10 Armenian, 10 Italian, and three Marathi data points, and within different semantics informants (10 light blue data points).

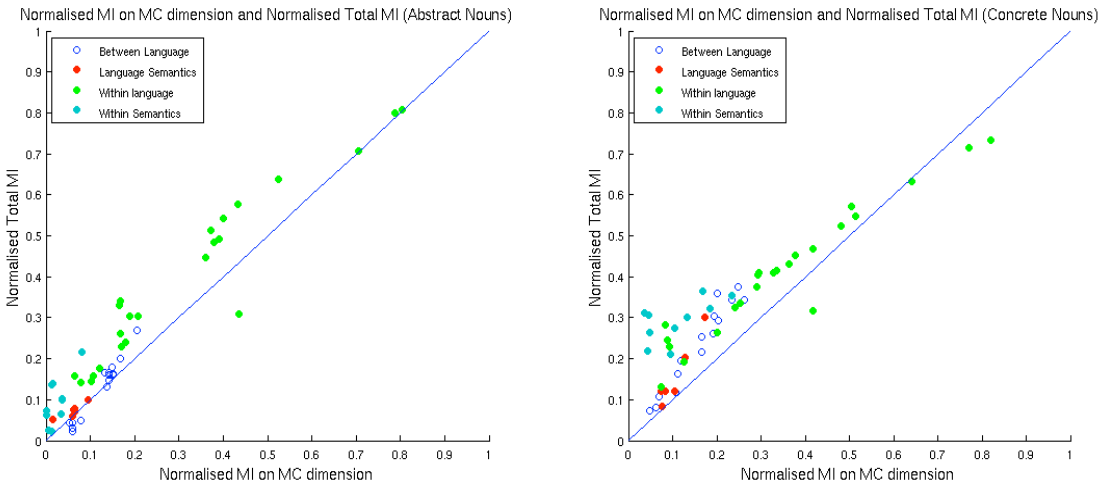

Figure 11: Scatter plots comparing mutual information on the MC dimension with total mutual information values.

The normalized mutual information along MC dimension is shown on the x-axis with the corresponding normalized mutual information including all dimensions on the y-axis, for abstract nouns (left) on and for concrete nouns (right). Different colors denote data points between the syntax of pairs of languages (empty circles), between the semantics and syntax (red), within language (green) for 10 Armenian, 10 Italian, and three Marathi data points, and within different semantics informants (10 light blue data points).

From Figure 11 we see that relative mutual information, when all the dimensions are taken together, is only slightly higher than when just the main mass/count dimension is considered, telling us that most of the variability is present along the main MC dimension. Again, abstract nouns show a larger variability between and within languages, and this difference is particularly strong within semantics. The source of the variability is most likely to be the degrees of freedom left in the syntactic or semantic classification task, applied to the abstract nouns. Even though the nouns and their meanings were disambiguated with a reference sentence, informants were still free to frame the sentences while deciding whether a particular marker can be used with a particular noun. Hence part of the variability may come as a result of the somewhat arbitrary determination of the exact meaning used by different informants when adapting their abstract cognitive categories to the classification of nouns, or of individual differences in the manipulation of context (Raymond *et al*. 2011).

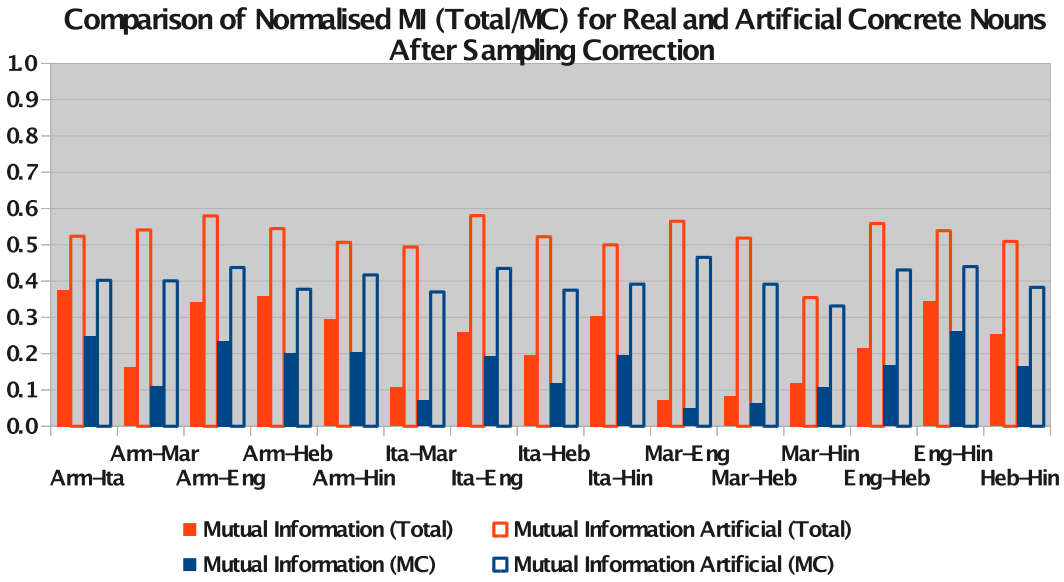

Figure 12: Mutual information between language pairs vs. artificially generated control values.

Normalized mutual information between language pairs (red solid) are in the 0.33–0.52 range, except for pairs including Marathi, for which they are around 0.2. These values can be contrasted with the higher values obtained by generating a pseudo usage table, based solely on semantic properties (red empty), as explained in Methods. A similar comparison is shown for the normalized mutual information but only on the MC dimension (blue-solid for real and blue empty for artificial).

Figure 12 tells us that there while relative values are higher than when computed only along the main mass/count dimension (Fig. 7), still there is little agreement across languages even on a finer scale, as the MI values are mostly less than half of the lower of the two entropies. Mutual information is a strict measure, wherein a single bit difference will put a noun in a different equivalence class and lower the mutual information. In contrast, however, artificial syntactic strings produced from the semantic ones, with the stochastic procedure

outlined in Methods, share around 50% mutual information, relative to the lower of their entropy values. Artificial syntactic strings also 'suffer' from a sensitivity to single fluctuating bits, hence the contrast between their 50% agreement and the 20–30% (roughly) agreement of the real syntax tells us that real agreement is genuinely low, and it is not all due to using a bizarre measure. The low mutual information of the natural syntax suggests that there is considerable syntactic variability along different dimensions of the syntactic domain, although most of the variability is already in the main mass/count dimension, since when restricted along that dimension agreement is even lower (Fig. 11–12).

3.5. CHILDES Corpus Study

With a method analogous to the Hamming distance measures, we analyze the Brown's section of the CHILDES corpus (only for the adult sentences) on the main mass/count dimension. We simply count the frequency of occurrence of a noun with mass markers out of the total occurrence of the noun in the corpus. There are 1551 nouns in this study out of which 522 nouns (151 are abstract and 371 concrete) are in common with the nouns used for the analyses above. Figure 13 plots the distribution of all the nouns on this main mass/count dimension. In a similar trend to Figure 3, we see the nouns to be distributed all across the spectrum from count to mass, with an overall decreasing trend in frequency going from count to mass (except for the pure mass class). Nouns with pure count usage are very many compared to the rest of the groups. We do find, however, a higher number of pure mass nouns in the corpus, as compared to the English syntactic data obtained from an informant.

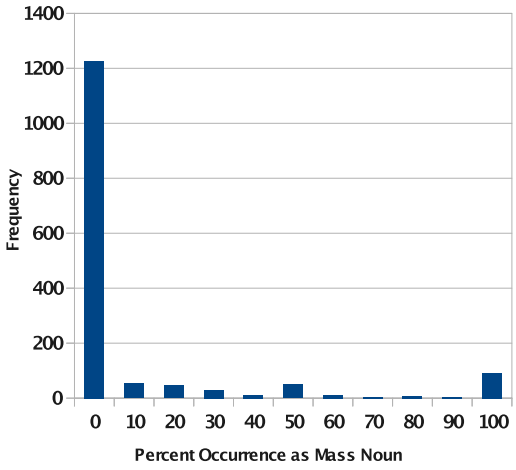

Figure 13: Distribution of nouns from the CHILDES corpus on the main mass/count dimension. Count occurrences of nouns are very frequent as compared to mass occurrences, with nouns lying along the entire spectrum.

A multi-dimensional analysis of the corpus data brings forward four markers as salient, two count ('*a(n)*' and Pluralization) and two mass markers

(bareness and '*some* + singular noun'). Nouns mostly lie along the vertices connecting these four markers. Figure 14 shows the most significant dimensions in terms of the co-occurrence frequencies found in the corpus, for example, along the edge connecting the vertices '*a(n)*' and 'pluralization', close to the '*a(n)*' vertex there are nouns that occur almost always with '*a(n)*' but seldom in plural form, in the corpus, while close to the 'pluralization' vertex there are nouns with the opposite occurrence, with the rest of the nouns occurring in between these two extremes. All nouns along this edge are in any case classified as pure count nouns, in the first bin of Figure 13. The density of nouns along the count edge is much higher than along the 'mass edge' (defined by the properties of appearing in bare form, at one vertex, and appearing with 'some' + singular noun at the other vertex). These four markers have the highest variance in their frequency of occurrence across the nouns in the corpus (Table 9).

bare	***a/an***	***every/each***	***many***	**pluralization**	***much***	***some***	***a lot of***
0.0485	0.1556	0.0044	0.0015	0.1177	0.0034	0.0275	0.0010

Table 9: Variance of the markers in the CHILDES corpus.

The variance of the markers we used to classify nouns in the Brown's section of the CHILDES corpus was calculated across its 1551 nouns, and the four markers with highest variance were used, a posteriori, to characterize the three most significant dimensions of mass/count variability, as independently generated by multi-dimensional scaling.

Finally, we contrast mass/count entropy values extracted from the corpus from those measured from the informant responses. To obtain entropy estimates from the CHILDES corpus, which can be used for the comparison, we first binarize the corpus co-occurrence frequency table, such that if a marker was found at least once with a noun, it was assigned the value of 1, and 0 otherwise. With this method, the total entropy of the corpus data was calculated to be 3.75 bits, as compared to the English informant entropy, which is 3.92. Since 522 nouns (151 abstract and 371 concrete) are common to the corpus and informant usage tables, we calculated the entropy on the MC dimension for them, too.

Informant entropy for concrete nouns on MC dimension	2.16
Corpus entropy for concrete nouns on the MC dimension	1.37
Informant entropy for abstract nouns on MC dimension	2.46
Corpus entropy for abstract nouns on the MC dimension	1.35

Table 10: The entropy values for nouns in both the database and the CHILDES corpus.

The entropy of the corpus on the MC dimension is lower than that of informants, perhaps due to the restricted contexts in which sentences can occur in a corpus, as opposed to the freedom of choice to the informants. The normalized

mutual information, including all dimensions, between binarized corpus and informant data after sampling correction is 0.051 for concrete nouns and 0.001 for abstract nouns.

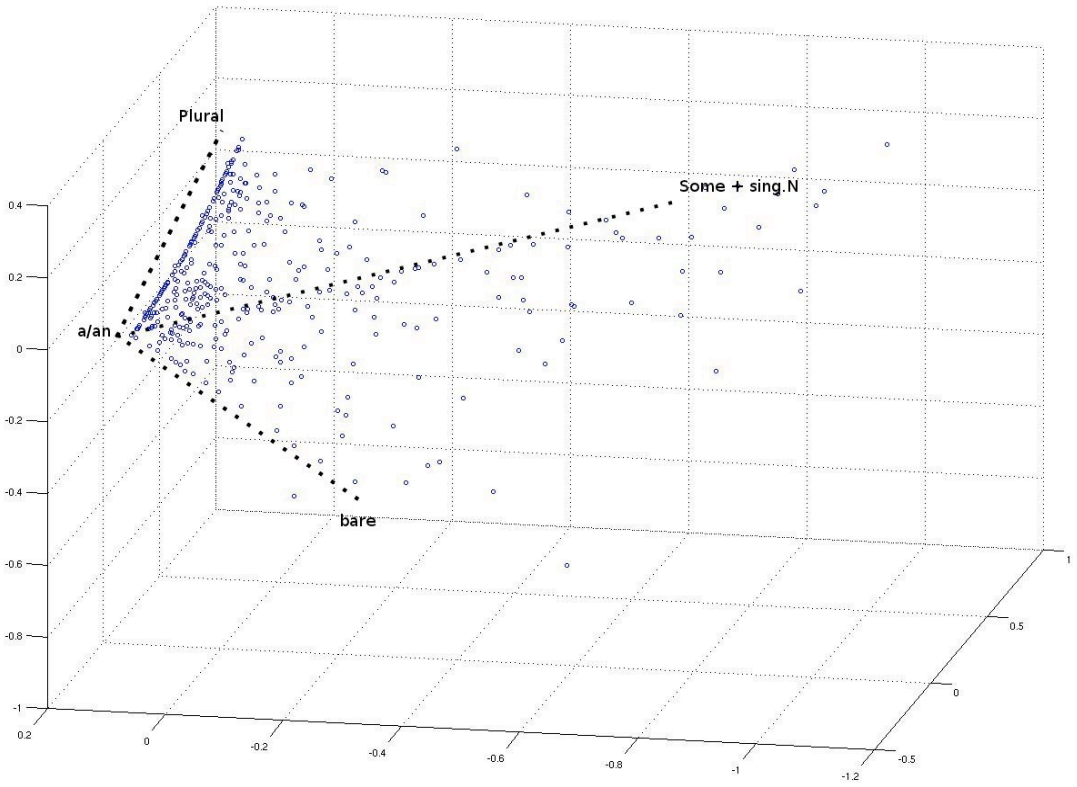

Figure 14: Visualization of the nouns in the Brown's section of CHILDES corpus in three dimensions, from multi-dimensional scaling.

4. Discussion

This is to our knowledge the first wide scale examination of cross-linguistic variation in the expression of the mass count distinction, which attempts to investigate the question of the degree to which the distinction is driven by perceptual-semantic attributes. Previous discussions in terms of data have stayed more or less at the level of the anecdotal. Our major contributions to the discussion are to show that the relation between such universal perceptual-semantic attributes and syntactic usage in specific languages is very weak; as is the relation between languages: There is a core group of count nouns where semantic atomicity corresponds directly with count syntax, but beyond this there is indeed widespread cross-linguistic variation in whether or not a concept is expressed via count syntax. In our sample of 1,434 nouns, in the five languages excluding Marathi, approximately 50% were what we would call 'robustly count', however only 392 were robustly count cross linguistically. We have little to say about core mass nouns, of which there were few or none in our sample.

This might conceivably be because of the way in which we chose our data base, rather than because of the inherently lower number of mass nouns in the languages. We leave it to other studies to identify a significant core group of mass nouns, cross-linguistically.

We have made a number of observations which are relevant to the discussion of the mass/count distinction:

I. Semantic or 'real world' attributes do not lead in a straightforward manner to individual syntactic rules in the mass/count domain, hence we have to probe a potential mapping, for any given natural language, between semantic attributes and a constellation of multiple syntactic rules. The obvious alternation i.e. atomic vs. homogeneous does not predict mass vs. count morphosyntax. This provides solid statistical support for the theoretical discussion in Gillon (1992), Chierchia (1998), Rothstein (2010), and many others.

II. When probing this domain with multiple syntactic usage alternatives, the distribution of 1,434 frequently occurring nouns in six natural languages is typically very far from binary. The largest single class of nouns in five of the six languages was the pure count prototype, i.e. the nouns classed 'count' by all syntactic probes. The rest are distributed in a graded fashion, with fewer and fewer nouns having more usage properties opposite to those of pure count nouns. Out of the 1,434 nouns, on average 873 were 'pure' count in a single language, range [693–1058], when excluding Marathi (where the figure was 255), but only 392 were 'pure count' in all other five ('typical') languages.

III. Outside of the pure count nouns, the correspondence between languages is weak, even when considering a single matching usage marker in each of the five non-exceptional 'typical' languages in the sample. In other words, learning what is a pure count noun, in any of these five languages, gave no significant clues as to the content of the pure count class in any of the other languages, beyond the 392 nouns which were pure count in all languages.

IV. Marathi differs from the other 'typical' languages in having a substantial fraction of nouns close to a pure mass prototype, particularly among abstract nouns, and a distribution closer to bimodal.

V. The semantic attributes that may be at the origin of the syntactic usage properties are distributed similarly, across concrete nouns, to the typical syntactic distribution, with most concrete nouns having 'count-like' attributes, and gradually decreasing proportions showing progressively more mass-like attributes.

VI. Despite the overall similarity between distributions, of semantic attributes and of syntactic usage properties (in all languages tested except Marathi) the correspondence in position along the main mass/count dimensions between semantics and syntax is very weak, even for concrete nouns. Quantitatively, in terms of variance it is midway between fully matching and random, and in terms of mutual information it is close to random. The dif-

ferent range reflects the non-linearity of the MI measure, but both measures point at the weakness of the observed correlation.

VII. Similarly, the correspondence between languages is weak, whatever measure is used.

VIII. Taking into account the detailed attributes and syntactic rules, rather than only the main mass/count dimension, the correspondence remains weak.

IX. There is considerable variability also among informants of the same language; part of which may be due to the testing paradigm.

X. A similar distribution along the main mass/count dimension can be gauged from 1,551 nouns extracted from the adult section of the English-language CHILDES database, after a different analysis, namely in terms of graded rather than binary syntactic usage frequencies. The three main dimensions of syntactic variability of nouns in the CHILDES database describe an asymmetrically loaded pyramid: most nouns are countable, and simply vary in their plurality at each instance; many fewer nouns span the other two dimensions, characterized by an increasing frequency of use in bare form, and of use with *some*+singular form, both mass-like attributes.

The results that we have reported have been purely statistical, that is to say, we have reported numbers with no discussion of any of the 1,434 items that make up of data base (where an item is a token from a particular language plus its particular feature values). An analysis of patterns within the data base is obviously the next stage in a linguistic analysis. This analysis will involve investigating whether there are recognizable patterns within the variation which are open to interpretation, whether there are lexical classes of nouns which function as classes cross linguistically, and if so how to characterize them. For example, *advice, information,* and *evidence* are strongly count in Hebrew and Italian, and mass in English. Do they behave as a class in other languages too? However, the results that we have so far already have theoretical implications relevant for continued research into the semantics and grammatical aspects of the mass/count distinction, and we conclude by specifying three of them.

First, we have provided solid empirical evidence that count syntax is not a direct reflection of atomicity in the denotation. Our initial aim to quantify the correlation (which we had presumed strong) between non-homogeneous nouns and count syntax could not reach beyond a core group of 392 nouns which pattern as pure count in all languages checked, excluding Marathi. This indicates a weak correspondence between perceptual/semantic and grammatical or morphosyntactic properties. Note that the 392 cross-linguistically count nouns included approximately 27% abstract nouns (284 concrete and 108 abstract), thus it is not even possible to argue that count syntax correlates directly with concrete atomic entities. Beyond this group of 392 nouns, the low level of mutual information between any two languages indicates language-specific grammaticalization of the distinction. This means that it is no longer possible to assume a general correlation between atomicity and count, and homogeneity and non-count. A preliminary examination of the 284 items in the pure count group which

are concrete rather than abstract indicates a high number of [+animate] nouns, in particular individuals of a certain profession (*scientist, nurse, preacher, slave, spectator*), nouns denoting buildings with a particular function (*library, bank, apothecary*) and nouns denoting artifacts that individuals stand in an one-to-one relation with (*wallet, watch, handkerchief*). All these predicates are atomic in an absolute sense, since they come in inherently individuable units, but they also frequently occur in contexts in which a particular instantiation of the predicate is perceptually salient. Thus it is plausible to posit that atomicity may be a necessary condition of a non-abstract noun being robustly count cross-linguistically. However, beyond this there are no straightforward generalizations.

The fact that these 284 nouns constitute between a third and a half of the robustly count nouns, in any particular language, indicate that beyond this weak generalization the grammaticalization of the correlation between atomicity and count differs from language to language. Furthermore, there are 108 abstract nouns in the pure count group, where the criteria for atomicity are by definition not well defined (since 'atomicity' is usually taken to express non-overlapping properties of matter). One can at this stage hypothesize potential criteria, for example, individuation via events: *nightmare, appointment*, and *crash* are all robustly count and non-concrete and atomic instantiations can be potentially be individuated via temporally located events. But this requires a notion of event individuation, itself problematic (see, e.g., Parsons 1990) and even then, leaves open the question of which event-types are 'inherently atomic' and which not. The conclusion for the linguist is that exploration of the basis of the mass/count distinction must be language particular, and will involve semantic features far beyond the homogeneous/atomic distinction.

We can draw a second theoretical implication from our results. We have seen that, for each language (again excluding Marathi), the approximately 50% of nouns which are not purely or robustly count in almost all cases cannot be characterized as 'pure mass'. These nouns are located at varying distances from the pure count class, depending on how many non-count features they have. This could be taken as support for the view that mass/count syntax is imposed on a neutral root, that it is appropriate to talk of mass or count 'usage', and that essentially, noun roots are flexible and can appear in either context. This is the view taken in Borer (2005), who claims that 'being a count noun' is an exo-skeletal phenomenon, the result of count syntax being imposed on a neutral syntactic root. Our data, however, show that approximately 50% of the nouns in each language do show a consistent count pattern, and furthermore, as stressed above, beyond the first 392 nouns, the choice of which nouns are used consistently as counts is specified within a language (subject to some idiolectal variation) and not across languages. This suggests that count syntax is a lexical specification, and that beyond a core group, it is specified independently for each language.

A third point is that our data reveals cross-linguistically (again excluding Marathi) a large group of pure count nouns, and no comparable group of mass nouns. This may be taken to support the widely accepted view (e.g., Chierchia 1998, Borer 2005, Rothstein 2010) that mass syntax is the default case, and that count nouns are derived from mass nouns via some form of operation, which results in their sharing common properties. The degree to which Marathi differs

from the other languages studied also forces us to realize that languages with a mass/count contrast may differ quite radically in how they implement it, and that the division of languages into those which have a count/mass distinction and those which do not tells us little about typological variation.

The overall conclusion is that the questions that linguists have been asking should be reformulated: Instead of looking for a general semantic characterization of the mass/count distinction which will explain the grammatical distribution cross-linguistically, linguists should be looking for language-specific patterns or generalizations, indicating that in a particular language, certain lexical classes are or are not grammaticalized as count. (For example, a cursory examination of the data indicates that Marathi is very restricted in allowing count syntax for abstract nouns.) If there are cross-linguistic generalizations, we might expect for them to have an implicational structure in the sense of Greenberg (1963), i.e. we could look for patterns of the form: If lexical class C_1 is pure count, then lexical class C_2 is also pure count. But it is an open question whether we would find them at any significant level. We should avoid classifying nouns as 'count', 'mass' or 'flexible'. In particular, our data show that non-robustly count nouns are flexible in different ways and to different degrees. What these ways and degrees are is still to be investigated.

If there is a general characterization of the mass/count distinction, then it probably is in terms of how the denotations of count (or mass) predicates are represented in the language, rather than in terms of any real-world feature. For example, Rothstein (2010) suggests that count nouns denote entities which are indexed for the context in which they count as atomic. This leaves place for particular languages to rank features which contribute to contextual salience, or to give them different weights, which might then influence patterns in classifying nouns as count. Features which weigh heavily in their contribution to count syntax in all languages would result in the set of pure count nouns cross-linguistically. In any case, the set of robust count nouns and the lack of a set of robust mass nouns indicate that we are more likely to find a general semantic characterization of count nouns than of mass nouns.

At a deeper epistemological level, not only is mass count syntax largely left undetermined by semantic attributes, it is also mistaken to regard it as a binary or quasi-binary structure. The distribution of syntactic usage properties is very far from bimodal in five out of the six languages tested, in fact it has nothing to do with bimodality. One is led to think of this grammaticalization as a graded self-organization process, operating within languages and to some extent within individual speakers, and driven only to a limited extent by universal attributes, and plausibly governed or at least constrained by language specific principles. However, at this stage we cannot tell to what degree the grammaticalization is governed, beyond the universal semantic or perceptual principles that we have attempted to quantify, by language-specific principles of different nature, such as cultural factors, historical accidents, individual language acquisition history, even context dependence within individual speakers. What is already clear, however, is that a domain of grammar, that to the non-specialist may seem rather straightforward, in fact opens new vistas on the character of what are improperly called language 'rules'.

Appendix

The following tables are the equivalent of Table 1, for languages other than English.

No.	Syntactic Questions
1.	Can the noun be used with 'a(n)'? (անորոշ գոյական +'մի' հոդ)
2.	Number distinction: Can the noun be used with plural form? (հոգնակի թիվ)
3.	Can it be used in combination with numerals? (համադրում թվականների հետ)
4.	In combination with classifiers or measure phrases that manipulate number? (համադրում դասակարգիչների հետ)
5.	Can the noun be used with 'every'/'each'? (Ամեն/յուրաքանչյուր)
6.	Can it be used with '(a) little'? (մի քիչ)
7.	Can it be used with '(a) few'? (մի քանի)
8.	In combination of 'many' + plural form of noun? (շատ + գոյականի հոգնակի թիվ)
9.	In combination of 'much' + singular form of noun? (շատ + գոյականի եզակի թիվ)

Table A1: List of questions used in Armenian to compile the usage table.

No.	Syntactic Questions
1.	Can the noun appear in the singular?
2.	Can the basic form appear with *af* (as in *af yeled lo 'ana*, 'not a single boy answered')?
3.	Is there a plural form?
4.	Can the plural form of the noun appear with a number?
5.	Can the singular form of the noun appear after *kol* 'every'?
6.	Can the singular form appear with *kzat*, *me'at*, *harbe* ('a little, a little, a lot')?
7.	Can the noun appear with *tipa* (literally 'a drop')?
8.	Can the noun appear with a classifier?
9.	Is it possible to say 10 + the singular form of the noun?

Table A2: List of questions used in Hebrew to compile the usage table.

No.	Syntactic Questions
1.	Can it be used with 'many'/'few'?
2.	Can it be pluralized?
3.	Can it be used with 'every'?
4.	Can it be used with numerals?
5.	Can it be used 'with a lot of'?

Table A3: List of questions used in Hindi to compile the usage table.

No.	Syntactic Questions
1.	Can the noun be in singular form with the indefinite article (*un*/*o*/*a*)?
2.	Can it appear (suitably pluralized) with a numeral (*due*, *tre*)?
3.	Can the noun appear with at least one singular indeterminate quantifier (*molto*/*molta*/*un po' di*)? Note: *non molto* should not be considered.
4.	Can the singular form be preceded by indefinite quantifier *qualche*?
5.	Can the singular form be preceded by exact quantifiers (*chili di*, *litri di*)?
6.	Can the singular form be preceded by *non molto* ('not much')?
7.	Can it have a plural form with a definite article (*i*, *gli*, *le*)?
8.	Can the plural form be preceded by exact quantifiers (*chili di*, *litri di*)?

Table A4: List of questions used in Italian.

No.	Syntactic Questions
1.	Can it appear with a numeral?
2.	Can it be used in combination with an exact quantifier (kilo, liter)?
3.	Can it be used with the article *ek* ('a')?
4.	Can it be pluralized?
5.	Does the morphology change when pluralized?

Table A5: List of questions used in Marathi.

Note: The questions were posed to the informants in their respective languages, not in the English translation.

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Syntactic Theory and the Evolution of Syntax

Brady Clark

Contemporary work on the evolution of syntax can be roughly divided into two perspectives. The incremental view claims that the evolution of syntax involved multiple stages between the non-combinatorial communication system of our last common ancestor with chimpanzees and modern human syntax. The saltational view claims that syntax was the result of a single evolutionary development. What is the relationship between syntactic theory and these two perspectives? Jackendoff (2010) argues that "[y]our theory of language evolution depends on your theory of language". For example, he claims that most work within the Minimalist Program (Chomsky 1995) is forced to the saltational view. In this paper it is argued that there is not a dependency relation between theories of syntax and theories of syntactic evolution. The parallel architecture (Jackendoff 2002) is consistent with a saltational theory of syntactic evolution. The architecture assumed in most minimalist work is compatible with an incremental theory.

Keywords: evolution; language; syntax; theory

1. Introduction

There are roughly two contemporary views on the evolution of syntax, the incremental view and the saltational view. The incremental view claims that the evolution of syntax involved multiple intermediate stages between the communication system of our last common ancestor with chimpanzees and full-blown modern human syntax. The saltational view claims that syntax was the result of just a single evolutionary development.

What is the relationship between contemporary theories of syntax and these two perspectives on the evolution of syntax? Jackendoff (2010) argues that there is a dependency between theories of language and theories of language evolution: "Your theory of language evolution depends on your theory of language". Building on earlier work (Jackendoff 1999, 2002, 2007a, 2007b; Culicover & Jackendoff 2005), he describes two types of architecture for the human language faculty, *syntactocentric* architectures (e.g., most work within the Minimalist

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Program; for background, Chomsky 1995) and the *parallel* architecture (Jackendoff 2002). According to Jackendoff, syntactocentric architectures are associated with the claim that syntax is the sole source of combinatoriality in the human language faculty, whereas the parallel architecture claims that there are independent combinatorial systems for, at least, phonology, syntax, and conceptual (semantic) structure. Jackendoff has argued in several publications (Jackendoff 2002, 2007a, 2010, 2011) that the parallel architecture lends itself to an incremental view of language evolution, whereas the syntactocentric approach is forced to the saltational view.

The focus of this article is the evolution of syntax, and, in particular, the relationship between syntactic theory and the two views on the evolution of syntax described above. I argue that there is not a dependency between syntactic theory and views on syntactic evolution. The parallel architecture is compatible with a saltational view on syntactic evolution that involves just one evolutionary development. Syntactocentric architectures consistent with minimalist assumptions are in harmony with an incremental view on the evolution of syntax. In section 2, I discuss the parallel architecture. In section 3, I turn to minimalism. Section 4 is the conclusion.

2. Parallelism

2.1. The incremental view of syntactic evolution

Gradualism was a feature of Darwin's perspective on natural selection (Sober 2011: 19; see also Fitch 2011: 3). On his view, natural selection is an incremental stepwise process in which the steps are small and numerous (Sober 2011: 19). Gradualism has characterized a great deal of work on language evolution. For example, Pinker & Bloom (1990: 713) assume that "language is a complex system of many parts, each tailored to mapping a characteristic kind of semantic or pragmatic function onto a characteristic kind of symbol sequence". For them, the building blocks of grammars include major lexical categories, major phrasal categories, phrase structure rules, rules of linear order, mechanisms of complementation and control, *wh*-movement, etc. They argue that this complex system arose via a series of small steps.

How do we tell an instance of incremental evolution apart from a saltational one (see section 3 for further discussion of the saltational view)? This may not always be possible in practice. The result of a single genetic macromutation may be difficult to distinguish from the result of an incremental process comprising numerous steps. However, many researchers would argue that an incremental account is preferable to a saltational account, even in situations where the available data does not distinguish between the two: "[A]lthough the outcome of a slow additive series of steps can be indistinguishable after the fact from a macromutation causing an immediate and radical change, the latter is evolutionarily highly unlikely" (McMahon & McMahon 2013: 195).

On the incremental view, the evolution of syntax involved a sequence of innovations, where the "sentence-forming word-combining powers of humans started small, and evolved to be more extensive" (Hurford 2012: 587). Each of

these innovations increased the expressive power of the system making it a target for natural selection (Progovac 2010a: 248, Jackendoff 2011). To illustrate, the following sequence of steps for the incremental evolution of syntax is adapted from Johansson (2005). Each step in the sequence is a functional communication system.[1]

(1) *Incremental view of syntactic evolution:*

1. one-symbol (i.e. phonology + meaning, but no combinatoriality)
2. two-symbol
3. hierarchical phrase structure
4. recursive hierarchical phrase structure
5. full modern human syntax (i.e. recursive hierarchical phrase structure plus functional categories and inflection)

(adapted from Johansson 2005: sect. 11.4)

In the remainder of this section, I discuss stages 1–5 in detail and then turn to evidence that has been used to support the incremental view.

At stage 1, early hominins produced monopropositional, non-combinatorial single-symbol utterances (Givón 1979, cited in Jackendoff 2002: 239 and Hurford 2011: 605). (The term *hominin* refers to "[a]ll species, living or extinct, on the 'human' side of the evolutionary tree after our last common ancestor with chimpanzees divided into the two lineages that would produce modern humans and modern chimpanzees" (Coyne 2009: 248).) The symbols themselves were monomorphemic at this stage and could be used in a non-situation-specific fashion to designate objects and events outside the sensory range of the sender and the receiver(s). As Hurford (2012: 587) puts it, "all linguistic knowledge [at this stage] was in terms of specific lexical items … there was no knowledge of constructions other than words".

At stage 2, a group of hominins produced monopropositional, multisymbol utterances. These utterances involved string-concatenation of symbols: [A B], [A B C], [A B C D], [A B C D E], etc. The sequences uttered were just a few symbols in length ("say, a maximum of three to five", Bickerton 1998: 347). Semantic and pragmatic factors such as Agent First and Focus Last (Jackendoff 1999, 2002, 2011) constrained the relative order of these symbols.[2]

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[1] Note that each step is characterized in terms of the structures that early humans had and used in communicative interactions, rather than in terms of the computational mechanisms that generated those structures. Lobina (2011a, 2011b; see also Lobina & García-Albea 2009) argues that we need to be careful to distinguish between the expressions that languages manifest and the mechanisms that operate over those expressions. This is especially important when discussing the role of recursion in the evolution of language: "Non-recursive mechanisms are in fact capable of processing recursive structures" (Lobina 2011b). Even if recursive syntactic structure emerged at a particular stage in the evolution of language, it does not follow that recursion is a property of the computational system underlying language at that stage (and vice versa).

[2] Casielles & Progovac (2010) argue that the Agent First constraint should be reconsidered in light of verb–subject structures from languages such as Spanish. They argue that verb–subject structures are better and simpler candidates than subject–verb structures for primary structures in the evolution of syntax.

These sequences were not associated with any features of modern human syntax such as hierarchical phrase structure. Only prosody serves as an indicator that the symbols in these sequences comprise a single unit (Progovac 2009b). Stage 2, in which words were strung together with no syntactic organization whatsoever, is sometimes referred to as *proto-language* (Bickerton 1990; Hurford 2012: 638). Symbol sequences at this stage are comparable in complexity to the multi-symbol non-hierarchical sequences uttered by Kanzi, a language-trained bonobo, using a combination of lexigrams on a keyboard (Savage-Rumbaugh & Lewin 1994, Savage-Rumbaugh *et al.* 1998).

Stage 3 introduced hierarchical phrase structure: [A B], [C [A B]], [D [C [A B]]], [E [D [C [A B]]]], etc.[3] This breakthrough might have involved "the grouping of words into headed units [such as a noun phrase—BC], and the application of structural rules [such as passivization and raising—BC] to headed units as a whole, rather than to individual words" (Johansson 2005: 234–235, expanding upon the proposal in Jackendoff 1999).[4]

Linear order within the syntactic units was not constrained by autonomous syntactic principles, but rather by semantic and pragmatic factors like those discussed above for stage 2. There were no recursive structures at this stage. (A recursive structure is a structure characterized by self-similar syntactic embedding (Tomalin 2011: 306). For example, in the structure [$_{NP1}$ the linguist who created [$_{NP2}$ the Na'vi language]], a noun phrase, NP2, is embedded within a phrase of the same type, the noun phrase NP1.)

Kinsella (2009: 121) observes that recursive structure is often confused with or subsumed under the notion of hierarchical phrase structure. As she points out, "recursion [= recursive structure] is not directly entailed by such hierarchical structuring; a structure can be hierarchical without being recursive. Recursion [= recursive structure] only arises when the specific phrases that are embedded inside each other are of the same type". The communicative behavior of certain non-human animals such as some birdsong arguably involves complex hierarchical structure (e.g., nightingale song; see Todt & Hultsch 1998, Hurford 2012: 57–62, and Berwick *et al*. 2012 for discussion). There is no evidence, however, that this hierarchically structured non-human animal communicative behavior encompasses recursive structures.

Stage 4 was the breakthrough into recursive syntactic structure.[5] Stage 5 is

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[3] Hurford (2012) does not treat the transition from stage 2 to stage 3 as a single step, but, instead, as a "continuous growth toward syntactic organization" (p. 607). At first, symbols are strung together with no clear boundaries. Then, strings of symbols are divided up into chunks — sentence-sized units — with clear boundaries. He proposes that the initial part of the two-word stage was governed by the principle 'Say first what is most urgent to convey, then what is next uppermost in your mind, and so on'. Berwick (2012) critiques Hurford's evolutionary story.

[4] Hornstein (2009: 114) similarly speculates that a crucial development in the evolution of syntax was the emergence of endocentric labeling.

[5] Lobina (2011b: 21, drawing on Moro 2008) argues that, at the appropriate level of abstraction, structural recursion is a property of any type of syntactic structure: "[E]very syntactic phrase (NPs, VPs, etc.) accords to the same geometry, an asymmetric structure of the following kind: [Specifier [Head–Complement]]". For example, a clause (a CP) is a complex [Specifier [Head–Complement]] structure containing structurally equivalent [Specifier [Head–Complement]] structures.

modern human syntax including functional categories and inflections. According to some incremental accounts such as Pinker & Bloom (1990; see also Pinker 2003), the mechanism driving the transition from one stage to the next was natural selection for communication in a knowledge-using, socially interdependent lifestyle. It is possible that some of these transitions (e.g., the transition from stage 4 to 5) involved processes of language change, rather than biological evolution (see below for discussion).[6]

Two primary sources of evidence for the incremental view of the evolution of syntax are language acquisition and language creation (see Hurford 2012: 590-595). Both processes have been claimed to involve the gradual development of syntactic structure. For example, Aronoff *et al*. (2008) discuss the linguistic organization of Al-Sayyid Bedouin Sign Language, a language that arose about 70 years ago in a small, insular community with a high incidence of deafness. Al-Sayyid Bedouin Sign Language displays the existence of certain syntactic properties (such as recursive hierarchical phrase structure), but not others (e.g., overt syntactic markers such as complementizers). They argue that "the existence of certain syntactic mechanisms and the lack of others [in Al-Sayyid Bedouin Sign Language] suggest that language does not appear all at once, but rather develops incrementally. Even syntax is not an 'indecomposable bloc'; instead it builds up over time" (Aronoff *et al*. 2008: 149).

Certain linguistic constructions ('fossils'; Jackendoff 2002: 236) have also been used to bolster the incremental view that syntactic evolution involved presyntactic but combinatorial stages (Jackendoff 1999, 2002; Progovac 2009a, 2010b). These constructions can be found in modern human grammars but are argued to be simpler than canonical syntactic constructions, yet display clear continuity with them. They are claimed to be traces of earlier stages in the evolutionary development of syntax. Examples are root small clauses (*Him worry?*), verb–noun exocentric compounds (*scare-crow*, *pick-pocket*), and paratactic combinations of small clauses (*nothing ventured, nothing gained*) (Progovac 2009a, 2010b). These constructions have been hypothesized to be structurally similar to those uttered at a stage in the evolution of human syntax in which two elements could be loosely combined, with prosody indicating that they form a single utterance (the two-word stage discussed above).

2.2. Parallelism and the incremental view

Jackendoff (2010) holds that the parallel architecture for the human language faculty lends itself to the view that the human language faculty evolved incrementally, each stage "adding an increment to the system's communicative efficiency and flexibility" (Jackendoff 2010: 71). In the remainder of this section, I discuss the parallel architecture that Jackendoff has presented in various publications, focusing on the syntactic component of the architecture. I argue that the architecture is compatible with both incremental and saltational views on the evolution of syntax.

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[6] Arbib (2012) argues that the transition from proto-language to language was a matter of cultural rather than biological evolution.

In the parallel architecture, the human language faculty has the tripartite organization illustrated in Figure 1 (from Jackendoff 2011: 609). Phonology, syntax, and conceptual structure are independent parallel systems connected by interfaces. Interface principles authorize correlations between structures in the three parallel systems. For example, interface principles ensure that "a syntactic head (such as a verb, noun, adjective, or preposition) corresponds to a semantic function and that the syntactic arguments of the head (subject, object, etc.) correspond to the arguments of the semantic function" (Jackendoff 2007a: 49). These interface principles are the Head Rule and the Argument/Modifier Rule. The Head Rule (Culicover & Jackendoff 2005: 163) says that a semantic function F in conceptual structure canonically maps to the head H of a syntactic phrase HP (for example, the head N of an NP) in syntactic structure. The Argument/Modifier Rule (Culicover & Jackendoff 2005: 163) states that the syntactic arguments and modifiers of the head H of a syntactic phrase HP canonically map to arguments and modifiers of a corresponding semantic function F in conceptual structure. A linguistic expression "is well-formed if it has well-formed structures in all components, related in well-formed fashion by all relevant interface [principles]. The theory is named the *parallel architecture* on the basis of this characteristic" (Jackendoff 2007b: 358).

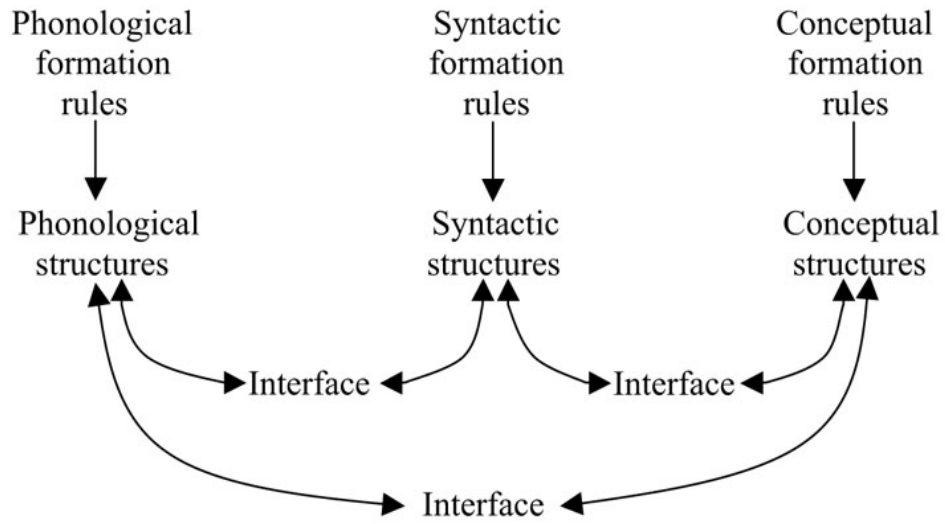

Figure 1: The tripartite parallel architecture (Jackendoff 2011)

Jackendoff (2011) motivates the parallel architecture by arguing that it better integrates with what is known about brain computation and other aspects of human cognition than other conceptions of the language faculty, particularly syntactocentric architectures such as that assumed by much work within the Minimalist Program (see section 3).[7] Further, Jackendoff claims that the parallel

[7] In their review of Jackendoff (2002), Phillips & Lau (2004) discuss limitations of Jackendoff's arguments on behalf of the parallel architecture and, in particular, dispute his claim that the

architecture makes available a much more plausible approach to the evolution of language than syntactocentric approaches (Jackendoff 1999, 2002, 2007a, 2007b, 2010, 2011).

In order to understand the relationship between the parallel architecture and the evolution of syntax, we need to be clear about the role of syntax in the parallel architecture. Culicover & Jackendoff (2005) describe the syntactic component of the parallel architecture in detail (see Merchant 2009, forthcoming for a critical discussion of Culicover & Jackendoff's non-structural approach to ellipsis). They defend the following hypothesis (Culicover & Jackendoff 2005: 5):

(2) *Simpler Syntax Hypothesis*
The most explanatory syntactic theory is one that imputes the minimum structure necessary to mediate between phonology and meaning.

For Culicover & Jackendoff, syntactic knowledge consists of syntactic features and a body of principles that place constraints on possible syntactic structures. Formally, these principles are pieces of syntactic structure stored in memory. The components in (3) are attributed to human syntactic knowledge:

(3) a. Syntactic features like category (NP, S, etc.), tense, number, and count.
b. Principles of constituency that place constraints on possible hierarchical structures. For example, "a phrasal node typically has a unique lexical node as its head; all its other dependents are either phrasal or minor categories" (i.e. $\{_{XP} \ldots (X) \ldots\}$; see Culicover & Jackendoff 2005: 110).
c. Principles of linear order that place constraints on possible arrangements of constituents. For example, $[_{VP} \{ V \ldots$ (the setting of the head parameter for the English VP) (Jackendoff 2007: 59).

Principles of the syntax–conceptual structure interface also play an important role in Culicover & Jackendoff's (2005) theory. These principles license connections from parts of syntactic structure to parts of phonological and conceptual structure. The Head Rule and the Argument/Modifier Rule (discussed above) are interface principles of this sort. Figure 2 (adapted from Jackendoff 2007a: 50) gives (part of) the syntactic and conceptual structure of the NP *the cats*. The numerical subscripts indicate links between different parts of syntactic and conceptual structure. Interface principles such as the Head Rule license those links.[8]

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parallel architecture approach is superior to syntactocentric approaches on psycholinguistic grounds. See Jackendoff (2007b: 359–366, 382–387) for a reply to Phillips & Lau's review.

[8] In addition to syntax, phonology, and conceptual structure, Culicover & Jackendoff (2005: chap. 6) propose a separate layer of the human language faculty — the Grammatical Function Tier (GF-tier) — that constrains the realization of direct NP arguments. This layer of grammar mediates between conceptual structure and syntactic structure. The basic idea is that the semantic arguments that are to be expressed as direct NPs (grammatical functions such as subject and object, as well as certain oblique arguments) are correlated with positions in the GF-tier ($[_{Clause}$ GF (> GF (> GF))]). These positions are then associated with particular positions in syntactic structure. Culicover & Jackendoff (2005: 232) propose that the GF-tier emerged late in the evolution of language:

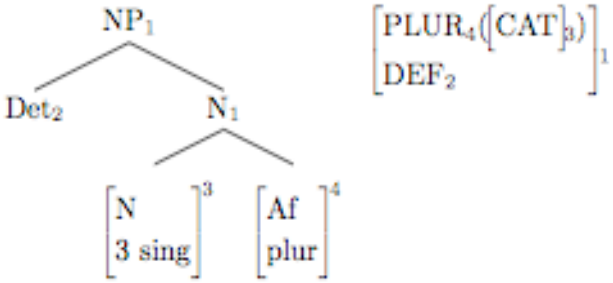

Figure 2: Syntactic and conceptual structure of the cats *(Jackendoff 2007)*

Syntax plays a central role in the parallel architecture: "What distinguishes true language from just collections of uttered words is that the semantic relations among the words are conveyed by syntactic and morphological structure" (Jackendoff 2007a: 63). Syntax "is the solution to a basic design problem: semantic relations are recursive and multidimensional but have to be expressed in a linear string … [syntax] is a sophisticated accounting system for marking semantic relations so that they may be conveyed phonologically" (p. 64).

I argue below that, for Jackendoff, the key innovation in the evolution of syntax was the emergence of principles of constituency (i.e. hierarchical phrase structure), principles that impose constraints on possible hierarchical structures. What about other aspects of syntactic knowledge such as syntactic features (e.g., syntactic categories) and principles of linear order? As I discuss below, on Jackendoff's assumptions about the architecture of grammar, principles of linear order might be better understood as principles constraining the syntax–phonology interface rather than as autonomous syntactic principles. Hence, the emergence of principles of linear order might not have been a stage in the evolution of syntax, strictly speaking, but rather a byproduct of the interaction between the syntactic and phonological components of the human language faculty. Further, the evolution of certain aspects of syntax such as functional categories might be better accounted for in terms of processes of language change such as grammaticalization, rather than as a consequence of biological evolution.

> Given that its properties depend heavily on the particular properties of the syntax-semantic interface, we do not see how it could possibly have been adapted from some other cognitive capacity. We conclude that the GF-tier is part of the narrow faculty of language in Chomsky's sense, and that the ability to infer a GF-tier from primary language data must be innate. We speculate further […] that the opportunities offered by the GF-tier for enhanced communication are what made it adaptive in the course of the evolution of the human language capacity. Given that the GF-tier logically depends on so much of the rest of the system being already in place, we would be inclined to see it as a relatively recent stage in the evolution of language.

The GF-tier plays an important role in Culicover & Jackendoff's formulation of argument structure and certain constructions such as the passive construction and the raising construction. While recognizing the central role that the GF-tier and principles of the syntax-semantic interface play in Culicover & Jackendoff's theory, I restrict my attention primarily to their claims concerning the syntactic component of the human language faculty and how they relate to different perspectives on the evolution of syntax.

In various publications, Jackendoff (1999, 2002, 2007a, 2007b, 2011) discusses how language might have evolved gradually. Figure 3 is from Jackendoff (1999; see also Jackendoff 2002: 238 and 2007b: 393) and is a hypothesis about how the entire human language faculty (including, but not limited, to syntax) might have evolved, given parallel architecture assumptions.

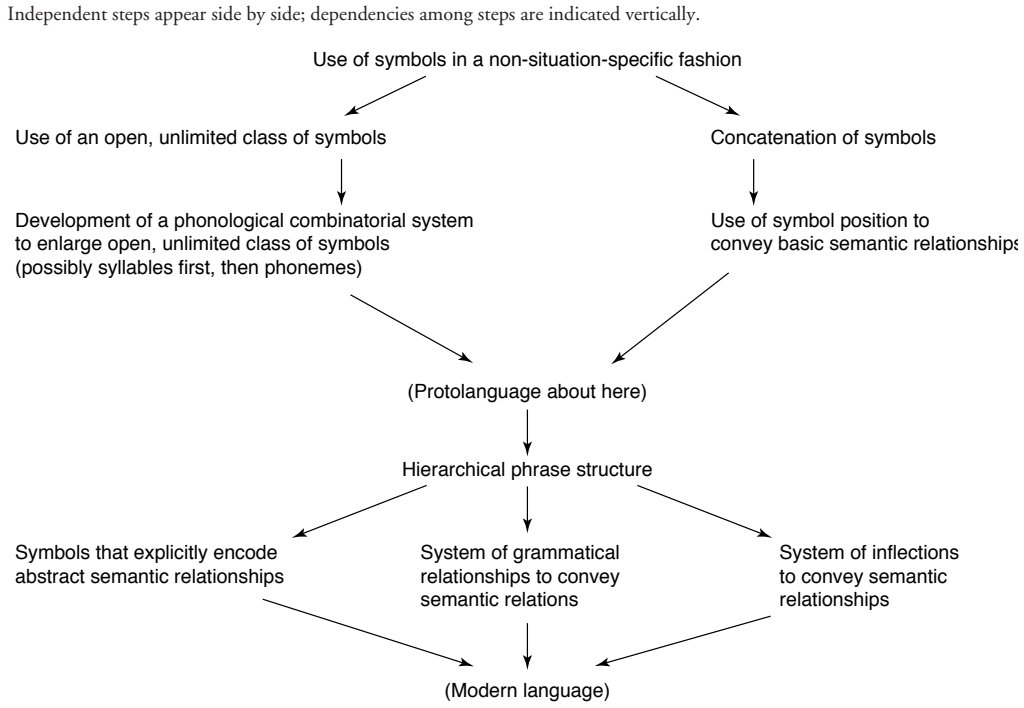

Figure 3: Summary of incremental evolutionary steps (Jackendoff 1999)

Each step in Figure 3 is communicatively adaptive. The model in Figure 3 can be roughly divided into four stages (see Jackendoff 2007a: 74):

(4) *Incremental view of language evolution:*

1. symbolic use of simple vocalizations, without grammatical organization
2. regimentation of vocalization along the lines of phonological structure
3. concatenation of symbols into larger utterances
4. syntax emerges, making more complex semantic relations among the words of an utterance more precisely mappable to linear order in phonology

(Jackendoff 1999)

Stages 1 and 2 comprise the one-word, no grammatical organization stage discussed above. Stage 3 is the two-word stage (sequences of symbols, but no hierarchical organization). The transition to Stage 4 is the transition to modern human syntax.

Figure 3 encompasses all of language evolution. What about the evolution of syntax in particular? As Jackendoff (2007b: 393) observes that "[t]he most significant cut" is between proto-language and hierarchical phrase structure (see Jackendoff 2002: 252 for discussion of this stage). In Figure 3 there are no intermediate stages between proto-language and hierarchical phrase structure. Other aspects of syntax (such as functional categories and inflection) emerged subsequent to the development of hierarchical phrase structure.

Culicover & Jackendoff (2005: 230–231) flesh out the parallel architecture approach to the incremental evolution of syntax, where "each successive layer adds further precision and flexibility to the system offered by the layers above it" (p. 231):

(5) *Incremental evolution of syntax:*

1. unstructured collection of symbols (*proto-language*) pieces of the same constituent adjacent to each other rather than scattered throughout the utterance (*principles of constituency*, *principles of the syntax conceptual structure interface*)
2. certain pieces of the same constituent are always in a predictable order (*principles of linear order*)
3. fixed order for direct NP arguments (*principles of the GF-tier*; see fn. 8)
4. flexibility of NP argument position to meet demands such as processing and information structure (*principles of the GF-tier*; see fn. 8)

(adapted from Culicover & Jackendoff 2005)

At stage 1, conceptual structure has been carved up into symbols. (This process is often referred to as *lexicalization*.) The communicative system at this stage is what Bickerton (1990) considers proto-language. Symbols can be concatenated by string-concatenation to build larger utterances, but without headed hierarchical phrase structure. This stage is characterized by semantically-based principles of word order such as Agent First and Focus Last (see Jackendoff 2002: 247–249).

Next, at stage 2, we have the emergence of hierarchical phrase structure (principles of constituency) such as "an XP has an X somewhere within it" ($\{_{XP} \ldots (X) \ldots\}$). Interface principles correlate embedding in conceptual structure to embedding in syntactic structure. For example, the Head Rule requires that semantic functions canonically map to the heads of syntactic phrases (Culicover & Jackendoff 2005: 162–163).

Stages 3–5 involve further refinements of hierarchical phrase structure. At stage 3, the linear order of heads, arguments, and modifiers is imposed by principles such as "NPs precede PPs within VP" (principles of linear order). The next two stages concern the GF-tier, the aspect of grammar that constrains the realization of direct NP arguments (see fn. 8). At stage 4, the linear order of NP arguments of verbs is determined by a subsystem of grammatical functions that is sensitive to (among other things) the thematic hierarchy (Actor/Agent > Patient/Undergoer/Beneficiary > non-Patient theme > other; see Culicover & Jackendoff 2005: 185). At stage 5, the subsystem of grammatical functions is further manipulated by particular constructions like raising and passive.

As noted above, each stage in this model is assumed to be an innovation that increases the expressive power of the system. How important are the details of human evolutionary history to this approach to syntax and its evolution? At various points, Jackendoff suggests that evolutionary considerations might play a (limited) role in constraining claims about the architecture of the human language faculty: "*Evolutionary considerations do lead us to seek a theory that minimizes demands on the genome* — but not at the expense of a rigorous account of the modern language faculty" (Jackendoff 2011: 589, emphasis added).

However, Jackendoff makes very little attempt to relate his model of language evolution to hominin pre-history. For example, he does not discuss how the different stages of his model relate to estimates about when the language faculty emerged during human evolution. As discussed in section 3 below, many researchers (both linguists and non-linguists) assume, not uncontroversially, that the syntactic component of the human language faculty evolved within the last 200,000 years.[9] If we assume that syntax evolved recently (in evolutionary time), then, it has been argued (see section 3), we should prefer models that minimize what had to evolve biologically in syntactic evolution.[10]

In fact, in Jackendoff's model (Figure 3) just a single evolutionary step gives rise to hierarchical phrase structure, a key feature of modern human syntax. The communication system prior to hierarchical phrase structure is proto-language, which involves string-concatenation. There are no intermediate stages between proto-language and hierarchical phrase structure. What evolutionary change gave rise to this feature of syntax (hierarchical phrase structure)?

Exaptation is the end-product of an evolutionary change in function where an adaptive trait was coopted to serve a new function (Gould & Vrba 1982; Fitch 2011). For example, the organs that evolved into bird and insect wings started out as temperature regulators and were exapted for a completely different function (flight). As a further example of exaptation, "the wings of alcids (birds in the auk family) may be considered exaptations for swimming: these birds 'fly' underwater as well as in the air" (Futuyma 2009: 294). Exaptations can be further modified by natural selection (e.g., the modification of penguin wings into flippers for efficient underwater locomotion; Futuyma 2009: 294).

Exaptation plays an important role in Jackendoff's view of the evolution of hierarchical phrase structure. For him, there is a close relationship between hierarchical structure in syntax and hierarchical structure in conceptual structure (i.e. thought): "recursive conceptual structure accounts for why recursive syntax is useful, namely for EXPRESSING recursive thoughts" (Jackendoff 2010: 608). On his view, the precursor mechanism for hierarchical structure in human syntax is hierarchical structure in conceptual structure. Hierarchical structure in conceptual structure was coopted to serve a new function in syntax: the expression

[9] Boeckx (2011: 45): "[E]veryone seems to grant that the FL emerged in the species very recently (within the last 200,000 years, according to most informed estimates)".

[10] There is ample empirical evidence for rapid evolutionary change in which significant changes occur in just tens of generations, maybe even faster (see Számadó & Szathmáry 2012 for discussion and references). That being the case there is no reason, given the evidence that is currently available, to rule out the possibility that human language syntax was the product of multiple evolutionary innovations within the last 200,000 years. We just don't know.

of conceptual structure. Jackendoff (2011: 616) argues that combinatoriality in conceptual structure is evolutionarily ancient, shared by both humans and non-human primates. In Culicover & Jackendoff's model, stage 2 involved the emergence of principles of constituency such as "an XP has an X somewhere within it" constraining headed hierarchical structures. These principles are skeletal pieces of hierarchical phrase structure stored in memory that can be unified with the symbols from stage 1 of their model. Outside of conceptual structure, the headed hierarchical structures recruited by syntax can be found in syllabic structure and musical structure (Jackendoff 2007a).

As discussed above, interface principles such as the Head Rule and the Argument/Modifier Rule (Jackendoff 2002; Culicover & Jackendoff 2005) constrain the association of conceptual structure with syntactic structure ("a syntactic head […] corresponds to a semantic function and […] the syntactic arguments of the head […] correspond to the arguments of the semantic function"; Jackendoff 2007a: 49). These interface principles are the result of the association of two components of the human language faculty, conceptual structure and syntactic structure, where the emergence of syntactic structure involved the recruitment of embedding structures originally used in conceptual structure for syntactic structure.

Jackendoff's single-step view of the emergence of hierarchical phrase structure can be contrasted with more strictly incremental views such as that presented in Hurford (2012: 607–608) (see fn. 3) in which the evolution of hierarchically structured syntax involved continuous growth toward syntactic organization. At the initial stage, words are simply strung together with no clear boundaries. At later stages, strings of words are chopped into chunks (first, sentence-sized units with clear boundaries and, then, later, smaller sub-sentential units).

What about the remaining steps of the evolution of syntax? On Jackendoff's view, the emergence of hierarchical phrase structure was just one stage among several in the evolution of syntax, each perhaps a product of a separate biological change. Aspects of syntax that developed subsequent to hierarchical phrase structure were principles of linear order, syntactic features (like tense, number, count, and category), systems of inflections to convey semantic relationships, symbols that explicitly encode semantic relationships, and a system of grammatical relations to convey semantic relations (the GF-tier; see fn. 8).

It is possible that the development of these other aspects of syntax did not involve the biological evolution of syntax per se, but rather were the outcome of interfacing separate components of the human language faculty or processes of language change. Jackendoff (2007b; see also Jackendoff 2011: 616) discusses how much of syntactic evolution can be attributed to sources other than separate genetic mutations. He speculates, for example, that symbol concatenation and the use of linear order to express semantic relations might have been cultural inventions, rather than the consequence of genetic mutations. Hierarchical phrase structure, grammatical categories, inflectional morphology, and grammatical functions (subject, object, and indirect objects) are hypothesized by Jackendoff to have a genetic basis.

Stage 3 of Culicover & Jackendoff's incremental model — the emergence of principles of linear order (e.g., $[_{VP}$ { *V* …, the setting of the head parameter for the

English VP) — is a stage in the evolution of the syntax–phonology interface. According to some contemporary work on syntax, the linearization of syntactic structure is an interface requirement imposed by the cognitive systems that we use to hear and speak language (see Hornstein *et al.* 2005: 219 for discussion). For example, Yang (1999) proposes a PF interface condition for the linearization of terminal nodes and argues that cross-linguistic variation in linear order is instantiated at the level of morphophonology, not syntax, strictly speaking. On this view, there are no autonomous syntax principles or operations in the syntactic component of the human language faculty that make reference to linear order or directionality.[11] This view is not incompatible with the parallel architecture, where principles of linear order could be understood as principles of the syntax–phonology interface constraining the alignment of syntactic structures with phonological ones.[12]

What about the other aspects of syntax discussed by Jackendoff such as syntactic categories and inflection? Can these be explained by processes other than the biological evolution of syntax? Jackendoff (2007b: 394) proposes that the vocabulary for relational concepts such as spatial relations and time might have been a cultural invention rather than requiring a genetic change.

That leaves the evolution of grammatical categories, inflectional morphology, and grammatical relations. Heine & Kuteva (2007) discuss the role of grammaticalization in the evolution of grammar after the emergence of the earliest human language(s). Grammaticalization is a process in language change involving the development of grammatical forms from lexical ones, and even more grammatical forms from grammatical ones. For example, the English modal *will* marking future (*I will go tomorrow*) was grammaticalized from the Old English main verb *willan* ('want to'). Grammaticalization theory is an account of the development and structure of functional categories. Heine & Kuteva use grammaticalization theory to explain the gradual emergence of syntactic categories and inflectional material such as case and agreement markers. They propose that all syntactic categories arose through the process of grammaticalization with the noun-verb distinction as the starting point. Hurford (2003, 2012) similarly discusses the role of grammaticalization in the evolution of word classes and morphological inflections after the biological evolution of hierarchical phrase structure.

In sum, the parallel architecture is compatible with the saltational view that syntax is the product of one evolutionary innovation.[13] This innovation was the

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[11] Some recent work conducted within the minimalist program (e.g., Chomsky 2012) speculates that linearization is a property of externalization, the mapping of the structured expressions generated by the syntactic component of the language faculty to the cognitive systems that humans use for sound and/or gesture (the sensorimotor interface). I discuss externalization in section 3.

[12] The notion that the principles constraining word order are of a different type than those that constrain hierarchical phrase structure has ample precedent in the literature. Curry (1961) distinguished between the *tectogrammatics* and *phenogrammatics* of language. Tectogrammatics concerns the underlying structure of language (i.e. the steps by which a sentence or subsentential unit is built up from its parts); phenogrammatics concerns the form of language (i.e. how linguistic elements are combined, the order in which they are combined, etc.). See Dowty (1996) and Muskens (2010) for discussion.

[13] However, the parallel architecture is arguably **in**compatible with a saltational view of the

recruitment for syntax of hierarchical phrase structure from elsewhere in cognition (conceptual structure). Later stages in the development of modern human syntax, such as the evolution of syntactic categories, might have been the outcome of interfacing separate components of the human language faculty or language change processes such as grammaticalization.

3. Minimalism

3.1. The saltational view of syntactic evolution

A saltation is a discontinuous mutational change in one or more traits, typically of great magnitude (Futuyma 2009). Gould (1980: 127) once suggested a saltational origin for vertebrate jaws:

> I envisage a potential saltational origin for the essential features of key adaptations. Why may we not imagine that gill arch bones of an ancestral agnathan moved forward in one step to surround the mouth and form proto-jaws?

According to most evolutionary biologists, saltations play a minor role in evolution ("my own betting money goes on a minor and infrequent role"; Gould 2002: 1146).

On the saltational view of the evolution of syntax, the emergence of syntax was at once and abrupt (see Kinsella 2009: 13–14 for further discussion). "[A] single evolutionary development can account for all the major mechanisms of syntax" (Bickerton 1998: 341). This development involved just a single genetic mutation. The mutation had a large effect, producing most, perhaps all, of the properties of human language syntax in one fell swoop. For example, Berwick (1997, 1998, 2011; Berwick & Chomsky 2011) has argued that the appearance of a recursive combinatorial operation (*Merge*; see below) accounts for many of the design features of syntax. These design features include:

(6) *Design features of syntax*

a. digital infinity and recursive generative capacity (i.e. the familiar 'infinite use of finite means')

b. displacement (e.g., *This student, I want to solve the problem*, where *this student* appears at the front of the sentence instead of after the verb *want*)

c. locality constraints (e.g., *who* cannot be interpreted as the subject of *solve* in *Who do you wonder Bill thinks solved the problem*)

d. restricted grammatical relations (e.g., no analog to 'object-of' like 'subject-object-of', where the subject and object of a sentence must agree)

(from Berwick 2011: 69–70)

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evolution of the human language faculty as a whole. Jackendoff (2002) assumes that phonology, conceptual structure, and the phonology–conceptual structure interface existed prior to syntax.

As discussed in section 2, the saltational view is typically associated with the assumption that syntax evolved very recently (maybe even only 50,000 years ago). Given this assumption, it is argued that the saltational view is more plausible than the incremental view. The incremental view posits multiple stages in the biological evolution of syntax. This would demand a longer evolutionary time period than the historical record suggests (but see fn. 10).

3.2. Minimalism and the saltational view

The saltational view of the evolution of syntax is most strongly associated with generative work on syntax, particularly recent work in the Minimalist Program (henceforth, *minimalism*). In this section, I discuss the relationship between minimalism and the evolution of syntax. I argue that minimalism is compatible with both saltational and incremental views on the evolution of syntax. More generally, my goal in this section is to establish that even accounts (such as Berwick's) that are typically characterized as saltational are, in fact, committed to there being several stages in the evolution of syntax.

Minimalism (Chomsky 1995; Marantz 1995; Belletti & Rizzi 2002; Hornstein *et al.* 2005; Boeckx 2006) grew out of the success of the Principles and Parameters approach to syntax. It explores the idea that the basic operations of the human language faculty are simple and few in number, and that the attested complexities of natural language are a byproduct of the interactions of simple subsystems (Hornstein 2009). Some recent work in minimalism proposes that syntactic knowledge involves only two components and that the interaction of these two components can explain all of the apparent complexity in modern human syntax (Boeckx 2011). The two components are:

(7) a. words[14] (understood as bundles of features)
b. a single, simple recursive operation, Merge, that glues together words and word complexes, thus forming larger units

(Boeckx 2011: 50)

Merge is a grouping operation which combines two syntactic objects α and β to form a labeled set {L, {α, β}}, where L is the label of the syntactic object resulting from Merge (Chomsky 1995, 2008; see also Boeckx 2006: 78, 2011: 52).[15] Merge has (at least) the properties in (4) (adapted from Longa *et al.* 2011: 599):

[14] In contrast to the parallel architecture. A key feature of the parallel architecture is that the basic units can be both words and multi-word phrases (Jackendoff 2002, 2010).

[15] Hornstein (2009) and others have argued that the operation Merge should be distinguished from the operation of labeling. On this view, Merge is simple concatenation: It takes a pair of simple syntactic objects (atoms) and combines them. Labeling identifies one of the two inputs to Merge as the label of the resulting concatenate.

(8) a. binarity (i.e. Merge combines exactly two elements)[16]
b. (asymmetric) labeling (i.e. the label of the syntactic object resulting from Merge is identical to one of its constituents)[17]
c. structural preservation (i.e. labels are not tampered-with and the basic structure of the tree remains unchanged over the course of a derivation)[18]
d. unboundedness (see discussion below)
e. flexibility (see discussion below)

Merge has the property of recursion. It is an operation that can take the output of a previous application of the operation as part of the input for the next application. The output of a system including Merge is, in principle, unbounded (property (8d)) because Merge has the property of recursion.[19]

The claim that Merge has the property of flexibility (8e) amounts to the (controversial) claim that "phrase structure-building and movement are special instances of the same basic operation — Merge (External and Internal Merge, respectively) — there is no fundamental distinction between movement and phrase structure construction" (Drummond & Hornstein 2011: 247). Each new application of Merge either draws from the lexicon (External Merge) or from within the expression constructed by Merge (Internal Merge, i.e. movement) (Chomsky 2004; Berwick & Chomsky 2011: 31).[20]

In minimalism, a sequence of applications of Merge constitutes a sentence derivation. Figure 4 gives a conceptual overview. Figure 5 gives a more detailed derivation of the sentence *the guy drank the wine*. The derivation involves multiple applications of Merge to words (understood as bundles of features) and word complexes, resulting in the syntactic structure for the string *the guy drank the wine*. This syntactic structure is passed off to the cognitive systems that humans use for sound and meaning. Figures 4 and 5 are from Berwick (1997).

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16 Yang (2009) and Chametzky (2000: 124–130) discuss the requirement that Merge always applies to exactly two syntactic elements, resulting in uniformly binary branching structures. Culicover & Jackendoff (2005: chap. 4) argue that the uniform binary branching assumption is deeply flawed.

17 Chametzky (2000: 127–128) discusses the status of labels in minimalism and argues that they should not be included as parts of syntactic objects.

18 Phillips (2003) challenges this claim. He argues that syntactic structures are built incrementally and that an incremental derivation can destroy certain constituents that existed at earlier stages in the derivation.

19 Progovac (2010b) suggests that specialized functional categories underwrite recursion. For example, the presence of complementizers might have enabled clausal embedding. We should keep in mind here the distinction between computational mechanisms like Merge and the structures that those mechanisms operate over. Progovac is suggesting that recursive structures become possible only with the development of certain functional categories. Merge, a recursive operation, was already in place before the development of those functional categories.

20 It has not been demonstrated formally that Internal Merge (movement) and External Merge are reducible to a single operation. Rather, External Merge and Internal Merge have been shown to be related. Internal Merge is sometimes analyzed as being decomposable into an operation Copy and the operation (External) Merge (Nunes 1995, Hornstein 2001). Hunter (2011) analyzes External Merge as decomposable into an operation Insert and the operation Internal Merge.

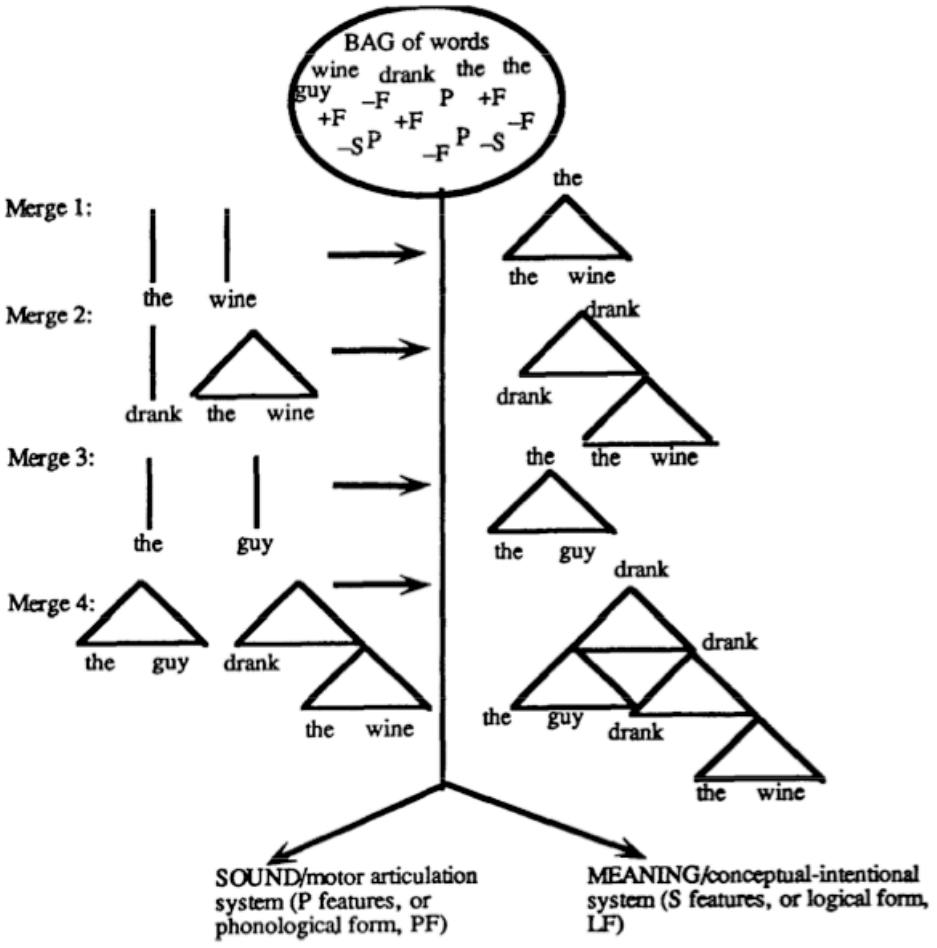

Figure 4: Sequence of Merge operations (Berwick 1997)

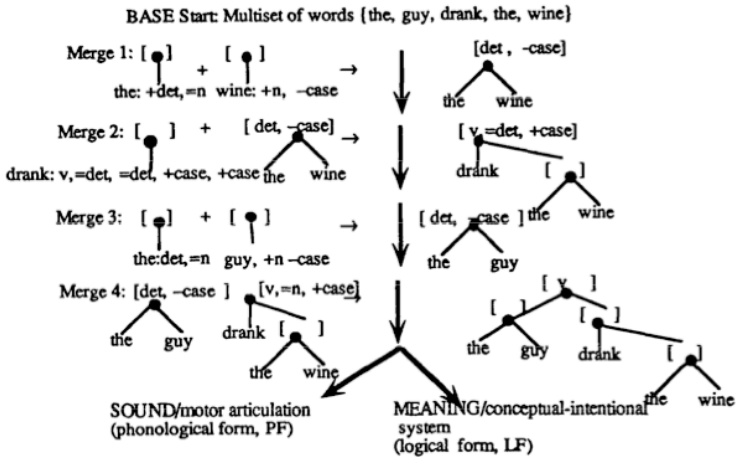

Figure 5: Sentence derivation in the Minimalist Program (Berwick 1997)

Berwick and others have claimed that there is no room for proto-syntax (stages between proto-language — a system involving string-concatenation — and modern human syntax) in minimalist approaches to the evolution of syntax. For example, Berwick (2011: 99) asserts that there is no possibility of an intermediate system between single-symbol utterances (or multi-symbol sequences involving string-concatenation) and full natural language syntax given minimalist assumptions: "[O]ne either has Merge in all its generative glory, or one has effectively no combinatorial syntax at all, but rather whatever one sees in the case of agrammatic aphasics: alternative cognitive strategies for assigning thematic roles to word strings", where combinatorial syntax is a syntax involving hierarchical structure, not simply the concatenation of symbols.[21] Berwick & Chomsky (2011: 31) continue: "[T]here is no room in this picture [i.e. the picture that language involves just a single recursive operation Merge] for any precursors to language— say a language-like system with only short sentences" (i.e. a system that outputs hierarchical structures that are bounded in size).

Kinsella (2009: 65–66, 87, 91–95, 160) argues that minimalism only permits a saltational account and is, consequently, evolutionarily improbable (on the assumption that an incremental account of the evolution of syntax is more plausible than a saltational one): "The simple minimalist system is furthermore evolutionarily improbable by virtue of permitting only a saltational account: the usual gradual adaptive processes of evolution leading to greater complexity than minimalism admits" (p. 160).

Some work in the Minimalist Program goes further, arguing that there is no conceptual reason for the postulation of proto-language. For example, Piattelli-Palmarini (2010) argues that there was no non-compositional proto-language involving string-concatenation of words because there could not be any words (defined as mergeable sound-meaning pairs) without syntax: "Words are fully syntactic entities and it's illusory to pretend that we can strip them of all syntactic valence to reconstruct an aboriginal non-compositional proto-language made of words only, without syntax" (p. 160).[22]

Minimalism is, in fact, compatible with both the incremental view and the saltational view of the evolution of syntax. First, I discuss the saltational view and minimalism. Next, I discuss how the incremental view can be reconciled with minimalism.

Arguments for the saltational view of the evolution of syntax have appeared in various forms in the minimalist literature (see, for example, Berwick 1997, 1998, 2011; Berwick & Chomsky 2011). The version of this argument that I present here is adapted from Hornstein (2009: 4–5; see also Hornstein & Boeckx 2009: 82). It has the following structure:

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[21] Berwick's view is similar to that of Bickerton (1990, 1998), who proposes that modern human syntax emerged in a single step from a proto-language involving string-concatenation.

[22] Anticipating Piattelli-Palmarini's point about there being no room for proto-language under minimalist assumptions, Jackendoff (2007a: 74) states that a single word stage (involving the symbolic use of a single vocalization, without grammatical organization) in the evolution of language is "logically impossible in the syntactocentric theory, since even single-word utterances have to arise from syntactic structure".

(9) *Argument for the saltational view of the evolution of syntax:*

1. Natural language grammars have several properties:[23]
   - they are recursive (sentences and phrases are unbounded in size and made up of elements that can recur repeatedly);
   - they generate phrases with a particular kind of hierarchical organization;
   - they display non-local dependencies which are subject to both hierarchical and locality restrictions.
2. These properties follow from the basic organization of the faculty of language.
3. The faculty of language arose in humans within the last 200,000 years, perhaps as recently as 50,000 years ago.[24]
4. This is very rapid in evolutionary terms ("the blink of an evolutionary eye"; Hornstein 2009: 5).[25]
5. The faculty of language is the product of (at most) one (or two) evolutionary innovations, which when combined with the cognitive resources available before the changes that led to language, delivers the faculty of language.

This argument has been heavily criticized in the language evolution literature (see, for example, Kinsella 2009; Jackendoff 2010: 68–70; Hurford 2012: 585–595). As noted above, some researchers criticize the conclusion in part 5 of this argument on plausibility grounds (i.e. incremental accounts of the evolution of syntax are more plausible than saltational ones). For example, Hurford (2012: 587) writes: "From an evolutionary point of view it is sensible to hypothesize that humans have progressively evolved greater combinatorial powers. This is more plausible than a tale of an evolutionary jump, such as Berwick envisages, to the infinite products of 'Merge in all its generative glory'".

Other researchers criticize the empirical support for part 3 of the argument above. Hornstein's (2009) claim about the timing of language evolution — i.e. that the faculty of language arose in humans within the last 200,000 years — is informed by the discussion in Diamond (1992; see Hornstein & Boeckx 2009: 82). Bickerton (1990) also proposes that language evolved as recent as 50,000 years ago. A defense of the claim that the faculty of language arose fairly recently can be found in Boeckx (2012). Boeckx claims that "the language faculty arose in *Homo sapiens*, and fairly recently, i.e. within the last 200,000 years" (p. 494). Evidence comes from cultural artifacts in the archaeological record; e.g., the emergence of new multicomponent tools. As Boeckx (2012: 495) puts it, expressing what I think is a view shared by many people working on the evolution of lang-

[23] See Berwick (1997, 1998, 2011) and above for a similar list.

[24] As Noam Chomsky (p.c.) puts it: "All we know with any confidence about evolution of the language capacity (languages, of course, don't evolve) is that it hasn't changed for about 50K years, and that about 50–100K years before that (plus or minus, it doesn't matter) there is no evidence that it existed. That sets some significant conditions on a serious approach to evolution of language".

[25] But see fn. 10.

uage, "it is hard to imagine the emergence of these artifacts and signs of modern human behavior in the absence of the language faculty".

But the archaeological findings (e.g., personal ornaments or tools that are comprised of more than one component) that have been used to support claims about the evolutionary timing of the emergence of syntax are consistent with a theory of the evolution of syntax in which proto-language or full-blown human syntax was in place long before the cultural artifacts were made. It has not been demonstrated that syntax (or language generally) is either a necessary or a sufficient condition for the construction of the cultural artifacts in the archaeological record. Botha (2009, 2012) shows that inferences drawn about language evolution from archaeological findings (e.g., the shell beads excavated at Blombos Cave; Henshilwood *et al*. 2004) are not well-founded.

Is minimalism compatible with the incremental view on the evolution of syntax? Yes. The evolution of syntax must have involved at least two steps on minimalist assumptions. Progovac (2009a, 2010b) presents an incremental approach to the evolution of syntax, while adopting the basic insights of minimalism and its predecessors. She argues for the following stages in the evolution of syntax:

(10) *Incremental view of syntactic evolution on minimalist assumptions :*

1. *parataxis* (non-hierarchical; only prosody serves as an indicator that words have been combined)
2. *proto-coordination* (rise of an all-purpose segmental indicator that words have been combined, the first functional category)
3. *hierarchical functional stage* (all-purpose proto-conjunctions give rise to specialized functional categories and functional projections; Move and recursive structure become available as a consequence of these developments)

(from Progovac 2009a, 2010b)

Grammars developed within the minimalist program, even those of the most radical sort, contain at least two components: words (understood as bundles of syntactic features) and the recursive hierarchical operation Merge. Consequently, the evolution of words and the evolution of Merge must both be part of any minimalist account of the evolution of syntax.[26] Berwick (1998: 338) makes this clear: "Merge cannot tell us everything we need to know. *It does not say how words came to be*, and will have little to say about the word features particular to each language" (emphasis added).

The claim that the evolution of syntax involved at least the evolution of words and the evolution of a recursive operation giving rise to hierarchically structured expressions is similar to Jackendoff's (2011) perspective on language evolution (see section 2). For Jackendoff (see, for example, Jackendoff 2011: 587), knowledge of syntax includes an inventory of principles of phrase structure (principles of constituency and principles of linear order), understood as units or complexes of units of hierarchical phrase structure stored in memory. On this

[26] On the assumption that words are, by definition, syntactic entities.

view, as discussed in section 2, the evolution of syntax must have involved at least the evolution of these units. These units are combined with words to build larger structures. Jackendoff (2011: 599, summarizing Pinker & Jackendoff 2005): "what makes language LANGUAGE is that recursion combines WORDS (and/or morphemes, depending on one's view of the lexicon), where words are long-term memory linkings of structured sound, syntactic features, and structure meaning. That is FLN [faculty of language in the narrow sense—BC] includes the capacity to learn WORDS in profusion and to apply recursion to THEM". To reiterate, the syntactic component of the human language faculty includes at least two components: words and a recursive operation. Both must be accounted for by any model of the evolution of syntax. Minimalists have given the evolution of words less attention than the evolution of the recursive operation Merge.

Chomsky's (2005, 2010) exaptationist view of the evolution of language is that the root of combinatoriality in syntax (Merge) is to be found in thought rather than communication. That is, combinatoriality in externalized, communicative linguistic behavior served a different function in early hominins (thought) and one stage of language evolution involved recruitment for interaction of that system (see Fitch 2011: 4 for a compact summary of Chomsky's perspective). This recruitment of the hierarchical combinatorial operation Merge for interaction generally (and communication in particular) was a crucial step in the evolution of language subsequent to the evolution of Merge itself. Chomsky, Berwick, and others call this step in the evolution of language *externalization*: "When the beneficial mutation [giving rise to Merge—BC] has spread through the group, there would be an advantage to externalization, so the capacity would be linked as a secondary process to the sensorimotor system for externalization and interaction, including communication as a special case" (Berwick & Chomsky 2011: 36).

As noted above, a key step in the evolution of syntax (along with the emergence of Merge and externalization) was the evolution of units with syntactic valence (words). In recent work, Boeckx (2011, 2012) argues that Merge — for him, concatenation — is "as primitive as one can get" (Boeckx 2012: 498) and that lexicalization instead was the key step in the evolution of syntax, making recursive Merge possible (Boeckx 2011: 53; see also Ott 2009). Lexicalization, on Boeckx's view, is the cognitive capacity to combine virtually any concept with any other concepts.[27] What lexicalization does is endow a concept with a property called an *edge feature*, a property that makes an item active syntactically. Chomsky (2008: 139): "A property of an LI is called a *feature*, so an LI has a feature that permits it to be merged. Call this the *edge feature* (EF) of the LI. If an LI lacks EF, it can only be a full expression in itself; an interjection". An Edge feature allows a concept to engage in Merge (Boeckx 2011: 54). These mergeable concepts are formally encoded as linguistic words.

[27] Boeckx appears to be using the term *lexicalization* differently from the ways in which that term has been used elsewhere in the linguistics literature. The term *lexicalization* is sometimes used to refer to a synchronic process by which conceptual structures are formally encoded. In the historical linguistics literature, *lexicalization* has been broadly defined as a diachronic process involving additions to the lexicon. See Brinton & Traugott (2005) for discussion. Jackendoff (2011: 608) critiques recent minimalist views on concepts and the conceptual-intentional interface.

There might have been a proto-language stage prior to lexicalization in which syntactic relations were dependent on conceptual content, as in Jackendoff's model (e.g., Agent First and Focus Last). Further, at a later stage, after the origin of the lexicalizing function, autonomous syntactic principles (such as parametric differences between languages involving linear order) might have developed as a consequence of the externalization of language (i.e. the recruitment of the sensorimotor systems to externalize the structures produced by the syntactic engine; Boeckx 2012: 500).

To recapitulate, the evolution of syntax on minimalist assumptions must have involved at least the three steps in (11):

(11) a. evolution of Merge
b. evolution of lexical items (lexicalization)
c. externalization linking the syntactic component of grammar to the sensorimotor systems[28]

Can any of these three steps be split up into further steps, while preserving minimalist assumptions? It is important to keep in mind that Merge is typically understood as a grouping operation that combines two elements to form a labeled set. Repeated application of Merge produces a nested hierarchical phrase structure. This is to be distinguished from string-concatenation. Repeated application of string-concatenation yields a flat structure (see Samuels 2012: 310 and Samuels & Boeckx 2009 for discussion). As noted above, if the emergence of Merge was an important innovation in the evolution of syntax, earlier combinatorial stages are not necessarily ruled out. Stages prior to the emergence of Merge might have involved no concatenation (the one-word stage) and/or string-concatenation (the two-word stage), in which syntactic relations are dependent on conceptual content (for example, Agent First and Focus Last). Crucially, these earlier stages in the evolution of syntax do not involve a recursive combinatorial operation which gives rise to labeled hierarchical phrase structures.

Hauser *et al.* (2002) distinguish between the faculty of language in a broad sense (FLB) and the faculty of language in a narrow sense (FLN). FLB consists of all the mechanisms involved in language. FLN is that (perhaps empty) subset of FLB unique to humans and unique to language. Hauser *et al.* hypothesize that FLN is the abstract linguistic computational system, perhaps consisting only of Merge.[29] FLB includes the computational system of the human language faculty

[28] Jackendoff takes issue with the role that externalization has played in Chomsky's (2010) speculations about language evolution:

> [Chomsky] sees 'externalization' as a second step in the evolution of language. But for him, externalization includes all of phonology and all of morphology, plus most of the aspects of syntax that differentiate one language from the next: word order, agreement, overt case marking, the distinction between WH-movement and WH-in-situ, and so on — in short, most of the things that most linguists think of as 'language'.
> (Jackendoff 2011: 616)

[29] Pinker & Jackendoff (2005) critique the hypothesis that FLN consists only of a recursive operation. For example, they argue that "words appear to be tailored to language — namely […] they consist in part (sometimes in large part) of grammatical information, and […] they are bidirectional, shared, organized, and generic in reference" (Pinker & Jackendoff 2005: 217).

combined with other organism-internal systems that are both necessary and sufficient for language (such as the sensory-motor system).

Longa *et al*. (2011: 602) argue that Merge was "the result of an intricate evolutionary pathway", involving both FLB and FLN components. For example, they propose that the recursive property of Merge might not be "a bona fide characteristic of FLN". Recursion could be evolutionarily ancient, whereas other properties of Merge (e.g., asymmetric labeling) might have emerged more recently in human evolution (see Hornstein 2009 for a related view). This proposal concerning Merge is consistent with proposals in Pinker & Jackendoff (2005) and Jackendoff (2011) that recursion (an operation characterized by the property that it can take the output of a previous application of the operation as part of the input for the next application) is of "considerably earlier ancestry than the human lineage" (Jackendoff 2011: 593).

Merge itself has been argued to be a compound operation composed of a concatenation operation and a labeling operation (see fn. 15). If this is correct, the labeling operation developed later in the evolution of syntax than the concatenation operation (which arguably underwrites the vocal sequences observed in a range of non-human primates).

In conclusion, syntactocentric architectures like the one presupposed by most work within the Minimalist Program are compatible with an incremental view of the evolution of syntax. The evolution of syntax on minimalist assumptions must have involved several distinct stages, including the evolution of Merge, the evolution of words, and externalization. One or more of these stages (for example, the emergence of Merge) might have involved further stages, once FLB and FLN are distinguished.

4. Conclusion

Jackendoff (2010) claims that the parallel architecture and syntactocentric architectures are committed to different models of the evolution of the human language faculty. As discussed in the introduction, he argues that there is a dependency between theories of language and theories of language evolution: "Your theory of language evolution depends on your theory of language". Jackendoff's (2010) focus was the evolution of the human language faculty generally, rather than the evolution of any particular subcomponent of that faculty (for example, phonology or syntax). In this article, I limited my attention to the evolu-tion of syntax. I argued that there is not a dependency relationship between theo-ries of syntax and theories of syntactic evolution. The parallel architecture is compatible with a view that the biological evolution of syntax involved just one stage (the recruitment of skeletal headed hierarchical phrase structure from elsewhere in cognition). The syntactocentric architecture assumed in most minimalist work is compatible with a view of the evolution of syntax that involves at least three stages, where, perhaps, some of those stages (e.g., the emergence of Merge) involved further evolutionary stages.

Thus, the simple take-home point of this article is that your favored theory of syntax does not determine your theory of syntactic evolution.

It is, of course, possible to turn this argument around. Just as your theory of syntax influences your theory of syntactic evolution, your theory of syntactic evolution influences your syntactic theory. The relationship between theories of syntactic evolution and theories of syntax is many-to-many. Most syntacticians would agree with the following claim:[30]

> Language is a system that is grounded in biology. As a biological endowment in our species, it must have evolved over a particular time scale, and in particular steps, as with our other biological endowments.
>
> (Kinsella 2009: 91)

If you start with the incremental view that syntax evolved over a long time scale and in multiple steps, then you are likely to be led to the view that syntax is a complex system along the lines suggested by Pinker & Bloom (1990) and Jackendoff's parallel architecture. If you start with the saltational view, then you are likely to be led to the view that syntax is a simple system and adopt a minimalist methodology (Kinsella 2009: 91, fn. 15).

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[30] Both of the approaches to syntax that I compared in this article assume that the object of study is the linguistic competence of individual speakers. For both Jackendoff and Chomsky, among many others, the properties of linguistic competence do not depend on relations to the external environment. There are other possibilities, of course. The object of study may be neither purely a matter of individual psychology nor exclusively a social phenomenon. Ludlow (2011: chap. 5) argues that syntacticians need not make an exclusive choice between an internal and external perspective on language.

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Erratum

The authors of the forum piece 'Review of the 9th International Conference on the Evolution of Language (Evolang9)', Christophe Coupé, Lan Shuai, and Tao Gong (*Biolinguistics* 7, 112–131), would like to announce an erratum.

The original sentence "At the center of discussions — and in opposition to a generativist framework minimizing the value of such an attempt (Chomsky 1972, Berwick 1998) — laid an effort to account for the properties of the faculty of language in light of modern evolutionary theory (Hurford *et al*. 1998)." (p. 112) should be reformulated as follows:

> At the center of discussions laid an effort to account for the properties of the faculty of language in light of modern evolutionary theory (Hurford *et al*. 1998), partly in reaction to a generativist framework which often had minimized the relevance of adaptionist approaches (Chomsky, 1972).

Consequently, Berwick (1998) should be removed from the references.

The updated paper can be downloaded as a supplementary file from:

http://www.biolinguistics.eu/index.php/biolinguistics/editor/downloadFile/284/1269

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★ FORUM ★

Disentangling the Neanderthal Net: A Comment on Johansson (2013)

Lluís Barceló-Coblijn & Antonio Benítez-Burraco

1. Johansson's approach to Neanderthal language

Sverker Johansson provided a very useful piece of work in which he skillfully reviews most aspects and scientific areas that have dealt with the Neanderthal language issue, including (but not limited to) genetics, archaeology, linguistics and modeling. Johansson's main conclusion is that *Homo neanderthalensis* had some form of language, at the very least, a proto-language, which he understands as "a system possessing lexical semantics but not syntax" (Johansson 2013: 6). At the same time, he notes that many aspects are still obscure, and that the data reported until now is still not conclusive. In particular, "whether they had syntactic language can be neither confirmed nor refuted" (p. 23).

We agree with Johansson when he says that Neanderthals had to count on some form of language. The amount of evidence he has reviewed points in this direction without doubt. We also agree with him in conceding Neanderthals a much more sophisticated capacity for oral production than as sometimes been depicted in the past. Nevertheless, we think that the real, productive debate is whether or not Neanderthals had the *same* faculty of language that anatomically modern humans (henceforth, AMHs) have. The author distances himself from this debate and, at the end, he does not take a stance. According to Johansson, the main reasons for not taking any clear position in this regard are related to an inherent problem of the sources of evidence and of the methodology:

(A) The data are few and not always trustworthy.

(B) Some conclusions imply difficult, even illicit inferential steps from the data.

Our criticism to Johansson's position is double. First, although we entirely agree with Johansson regarding the additional difficulty incorporated by an extinct species, we think that we actually can proceed with a null hypothesis: In our opinion, current evidence supports that the Neanderthal language was not like

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AMH's because it lacked *modern* syntax (and *only* because of this). This is a plausible conclusion that can be reached from the very data reviewed by Johansson. Second, some aspects of Johansson's approach, both related to the evidence and the methodology need to be improved. Plausibly, it is this circumstance that hinders him from reaching any firm conclusion about this issue.

2. Some remarks on (the interpretation of) evidence

2.1. The nature of language

Johansson's definition of language is as follows: "Language is a symbolic communication system that is not fixed; extensibility is an integral part of the system. This amounts to the presence of something like lexical semantics, flexibly and learnably mapping forms to meanings" (Johansson 2013: 6). He further states that "a system that has units that are combined in syntax-like patterns, but that lacks a mapping to meanings, such as birdsong, is likewise not language. A system possessing lexical semantics but not syntax, I would call a protolanguage" (p. 6). Johansson has pointed out that *lexical semantics* is something one could attribute to Neanderthals. In turn, "there is no real evidence one way or the other concerning syntactic abilities [among them] […]. This means that Neanderthals had at least a spoken proto-language; whether they had syntactic language can be neither confirmed nor refuted" (p. 23).

Consequently, Johansson has focused his attention on (1) *lexical semantics* as the core property with which language (or protolanguage) is endowed; (2) *syntax* as the property that distinguishes protolanguages from modern language; and (3) *communication* as the function that language fulfills (and plausibly evolved for). All these assumptions can be eventually problematic, particularly if some of these concepts are used loosely, as sometimes seems to be the case.

For instance, lexical semantics in frameworks like Hale & Keyser's (1995), Mateu's (2002), Borer's (2005a, 2005b), or Acedo's (2010) does not separate syntax from the meaning of the lexicon. It is still an important open debate whether lexical items are or can be detachable from syntax.

Moreover, exactly what does *extensibility* (or even "flexibly and learnably mapping forms to meanings") mean or imply? And what is the ultimate source of such extensibility or flexibility? After all, semantic extension allows for such expansion of meaning and for flexibly mapping forms to (new) meanings. However, this ability has been attested in great apes reared in captivity (Gardner *et al.* 1989; Savage-Rumbaugh & Lewin 1994; Savage-Rumbaugh *et al.* 1998). The other source of semantic extensibility is, crucially, that of semantic compositionality. But this kind of compositionality relies heavily on syntax: new meanings emerge when the same words are combined in a different fashion.

Additionally, it is important to always make clear in which case one is referring to modern, complex syntax, or instead is using the term *syntax* loosely (perhaps in the etymological sense of 'to put in order' or even 'to arrange'). For example, Johansson's dismissal of Piattelli-Palmarini's rejection of a language without syntax obviates that Piattelli-Palmarini is referring to the former type. More importantly, different kinds of grammars have been actually hypothesized within the very Chomskyan paradigm. Syntax is not an all-or-nothing question

within this framework. But neither is it a continuum.[1] Ultimately, when we look at "animal communication" through a "syntactic" lens, we actually find "grammar" among extant species (see Hurford 2011: 3–99). Hence, ape strings of lexigrams or signs, such as those performed by Washoe, Kanzi, or Sarah, were not syntax-free. On the contrary, they are the output of a system that can be described by means of a regular grammar. Allegedly, some species (such as Gentner's *et al.* 2006 starlings) might have access to recursive grammars, though once again in experimental conditions only.[2] Perhaps all hominin proto-languages were the output of systems akin to regular grammars (such as apes' 'languages') or perhaps to recursive grammars. In fact, it is our contention that the available data do not support non-AMH hominin 'languages' being syntactically structured like AMH languages are.

On another front, Johansson argues that Pirahã (if it actually lacked recursion) or pidgins are functional communication systems with lexical and propositional semantics. He further states that, although they "lack one or more components of full modern human language […], these systems also deserve the label 'language'" (p. 6). This can plausibly be the case, according to his own characterization of *language* (see above). But once again, this entails placing structurally different linguistic objects under the cover term of *language*, obviating the fact that they rely on grammars endowed with different (formal) properties. In the end, one always can argue that the string of symbols productively generated by Washoe or Sarah were also endowed with lexical and propositional semantics, and allowed them to communicate with their caregivers. The suppression of boundaries between systems (i.e. gradualism) places apes' performance in an ambiguous position, becoming able to be classified under the pidgin umbrella as well. Notice also that, behind a pidgin there is always an AMH brain. A pidgin is never entirely independent from the influence of a prototypical, non-simplified/reduced adult language (these systems have been simplified by adult AMHs who already spoke a full modern language). Ultimately, it seems to us that what is really worth characterizing is the proto-typical AMH language, and then to determine whether or not Neanderthals could have developed something like this.

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[1] In fact, Johansson mentions the controversy about a partial Merge, and specifically about a gradual evolution of Merge with precursor stages. However, fifty years earlier, Noam Chomsky himself postulated as well different classes of grammars according to the type of set of strings of symbols (= formal languages) that can be generated under certain general admissibility conditions (Chomsky 1956, 1959). Cross-serial dependencies (i.e. dependencies among nodes in a hierarchy that are not expressible as hierarchical nodes) suggest that natural languages could be characterized as Type 1, or context-sensitive languages within his hierarchy. Nonetheless, both Type 2 (context-free) and Type 3 (regular) grammars were also hypothesized to exist. Notice that context-free grammars are also able to generate sets of strings recursively. Eventually, regular grammars generate strings of symbols as well, although they are arranged in a linear fashion without any internal structuring. Currently, different subtypes of both regular grammars ('first-order Markov grammars' and 'state chain grammars'), and context-sensitive grammars ('mildly context-sensitive' and 'context sensitive [*stricto sensu*])') are postulated. It is true that formal grammars could not properly apprehend the complexities inherent to natural languages (Pullum & Rogers 2011), but they help to understand some of their basic properties. As a consequence, it seems justified to rely on them to gain a clearer insight on some basic properties of hominin [proto] languages.

[2] This is a well-known open debate. See Gentner *et al.* (2006), van Heijningen *et al.* (2009), or Berwick *et al.* (2011).

Lastly, one should avoid conflating *language* with one of the functions it fulfills, namely *communication*. As discussed above, different types of (formal) languages (including proto-languages), all endowed with lexical and propositional semantics, can effectively function as communication devices. As a consequence, it is the structural properties of languages that matter — and, more specifically, how linguistic structures are generated. In fact, modern biology heavily supports this view, given that only biological structures along with their activities evolve, but not functions (see Love 2007 for discussion).[3] This clarification is important also for correctly addressing the *form-function* problem that Johansson has repeatedly come into and has extensively reviewed. Admittedly, modern functions cannot be automatically inferred from human-like, language-related biological structures (the descended larynx, the mirror neurons, etc.). Conversely, modern functions can exist even if some human-like, language-related biological structure is absent (e.g., sign languages and speech-hearing organs). Nonetheless, biological structures *do* exhibit a measurable degree of evolutionary continuity that allows making justified inferences from them regarding language evolution.[4]

2.2. The substratum of the language faculty: Neural connections and the brain

We completely agree with the way in which Johansson has reviewed the fossil evidence of speech and hearing organs. However, we have some concerns regarding the way in which he has discussed the neuronal substrate of language. Concerning the neural wiring, Johansson only mentions nerves that control tongue movements and breathing, plausibly because some of their properties can be confidently inferred from the fossilized nerve canals. But this is informative only with regard to speech. Although brain nerve tracts do not fossilize, we are not here in total darkness. For instance, as brains become larger, structural changes in the form of internal reorganization do occur. Typically, we observe a connectional invasion of disjointed areas, thus plausibly allowing different cognitive systems to interface. Brain allometry is another interesting source of evidence. Different brain morphologies plausibly imply different brain interconnection patterns. Whereas Neanderthal and AMH skulls (and brains) are quite similar at birth, they differ progressively across development (Gunz *et al.* 2010, 2012). Importantly, it is after birth when essential changes in the wiring of the brain take place under the influence of environment and, when fully functional,

[3] According to Love (2007), *functions* can be construed as the uses given to biological structures because of their connections with other structures, but also because of the relationships existing between the organism and the environment.

[4] For instance, we have contended elsewhere (Balari *et al.* 2011) that the Chomsky Hierarchy has a neurobiological correlate that can illuminate how human language evolved. Hence, the automaton in the Chomsky Hierarchy equates with a computational device relying on a pattern generator (or sequencer) and a memory 'stack'. Simply put, more memory resources allow the automaton to generate more complex structures. Following Lieberman (2000) or Ullman (2001), a plausible neural substrate for the sequencer is the basal ganglia (see section 2.5). Working memory plausibly relies on the activity performed by diverse cortical structures. Our point was, then, that the evolutionary trajectory of this computational device is more informative regarding the evolution of language than that of the functions it fulfills or of the functions fulfilled by language (communication, symbolic behavior, modern behavior, and the like).

computational devices eventually appear (Balaban 2006; Ramus 2006; Petanjek *et al*. 2011). For instance, according to Boeckx (2012), it is the more globular configuration of adult AMH brains that allowed modern syntax to emerge. Such a configuration would offer the possibility of more efficient connections and information exchanges and, eventually, of computational efficiency. In sum, the different skull developmental trajectories plausibly do reflect real differences between these two hominins at the neurobiological level.

On the other hand, we consider that the importance given by Johansson to lateralization should be reduced. There is only a weak correlation in our species between general verbal skill and precociousness of language development on the one hand, and the degree of lateralization in hand use, on the other — in general, between lateralization and cognitive abilities, including language (Nettle 2003). Additionally, language seems not to depend so much on a specific pattern of structural and functional lateralization of the brain, but on specific interconnections between neuronal populations that link them functionally. For instance, language integrity is not substantially affected, either qualitatively (patterns or types of linguistic structures) or quantitatively (number of utterances, size of the lexicon, etc.) when language is transferred to the right hemisphere in some pathological conditions or in some left-handed people (Liégeois *et al*. 2008). Moreover, at the genetic level, Lambert *et al.* (2011), in their study of the expression of genes in human fetal cerebral cortex, have found no significant differences in gene expression patterns between left and right neurons from Broca and Wernicke language areas.[5] Lastly, fossil evidence of brain structural and functional asymmetries predates the evidence for (modern) right-handedness among hominins (Kyriacou & Bruner 2011). In fact, functional (or even structural) brain lateralization is an archaic feature among mammals.[6]

2.3. On the role of genes, the environment and development

We have some major concerns regarding the way in which Johansson reviews the genetic evidence.

First, it is true that "there is no such thing as *the* gene for a complex trait" (Johansson 2013: 16). However, this does not automatically preclude that a "single genetic change conferr[ed] language" (p. 15). This mutation can affect a master or hub gene (see for example, Seo *et al.* 2009). These genes establish connections with many other elements in the genome/proteome. A single mutation or change will plausibly affect the relations with the rest of elements within its interactome,[7] thus provoking many downstream changes. More importantly, when one speculates about *the* mutation that yielded language (e.g., Chomsky, cited by the author [p. 16]), one does not normally think about a

[5] Importantly, they also concluded that "cortical evolution in different mammalian species may be driven in part by species-specific changes in the regulation of the same genes and pathways, which are potentially important in brain patterning in many species" (Lambert *et al*. 2011: 10).

[6] Proven in gorillas and chimps (Cantalupo & Hopkins 2001; Hopkins & Cantalupo 2004; Hopkins *et al*. 2007), but also in many other vertebrates (Rogers 1989).

[7] That is, the whole set of molecular interactions in cells. *Genome*: the whole set of genes; *proteome*: the whole set of proteins.

mutation that gave rise to *all* components of language from scratch. This mutation could just help to functionally interconnect the diverse, previously evolved, elements of language. Or it could just optimize some specific component (such as syntax?) that was already connected to all the remaining, important-for-language pieces? For more on this, see section 4 below.

Another aspect that should not cause confusion is that of *the function* of a gene. In fact, this is related to the dual sense that *function* has in biology (see also section 2.1). Hence, it is true that *FOXP2* has a very well preserved expression pattern and plays practically the same physiological role in all mammals (and perhaps in all vertebrates). Nonetheless, this does not preclude that the brain circuitry *FOXP2* contributes to fulfill different functions in different organisms — i.e. support different behaviors like ultrasound generation, song learning and performance, or language. The possibility that this gene contributed to a different function in Neanderthals is neither implausible nor non-parsimonious (e.g., vocalizations related to some musical behavior, as suggested by Mithen 2006).

In the third place, the differences between Neanderthals and AMHs at the genetic level are real. There are differences in genes involved in brain development and, more significantly, in genes related to language disorders. For instance, Neanderthals exhibit the ancestral allele in some positions of the gene *MCPH1*, which controls neuronal proliferation and whose mutation gives rise to microcephaly (Green *et al*. 2010). A similar case is the gene *CNTNAP2*, one of FOXP2 targets and a candidate for specific language impairment and autism. This gene shows a fixed single nucleotide change in Denisovans, the closest hominin to Neanderthals (Meyer *et al*. 2012). Finally, Maricic *et al.* (2013) found an AMH-specific substitution within a regulatory region of *FOXP2* that is likely to alter its expression.

Furthermore, it seems to us that Johansson has, to some extent, oversimplified the role of genes in relation to the environment and in development. He wisely takes into account West-Eberhard's (2003) book. However, it is West-Eberhard who has shown us that the same genotype can develop different phenotypes in different environments. Neanderthals and AMHs actually evolved and lived in different environments (Finlayson 2005; Carrión *et al.* 2011). Consequently, even if they were endowed with the same 'linguistic genotype' (though, remember, this is not the case), we cannot automatically rule out the possibility that the former had a different faculty of language. In fact, as we pointed out above, it has been recently proved that their skull ontogenies (and, hence, brain development) diverge at some point after birth. Ultimately, evolutionary innovations can arise in the absence of genetic modifications (i.e. in neutral conditions) because of the very dynamics and generative properties of developmental systems (Müller & Newman 2005; West-Eberhard 2005). These considerations substantially minimize the role of genes both in development and evolution.

Last but not least (and related to our latter concern), genes are less important during the last steps of development, when the definitive wiring of the brain takes place and cognitive abilities finally emerge in response to environmental stimuli.

2.4. Effects of introgression

In section 5 of his article, Johansson rightly distinguishes between *hybridization* and *introgression*. Introgression is a case of partial hybridization between species; it is "an invasion of the genome", in Mallet's (2005) terms, something that happens quite often in nature, and therefore it is not an anomaly (Mallet 2008); within European mammals, 10% hybridize. Mallet observes that "introgression can be highly selective, *affecting only some parts of the genome*, whereas other genomic regions strongly affected by divergent selection remain virtually isolated" (Mallet 2005: 6; our emphasis). Neanderthals and AMHs interbred.[8] But so did Denisovans and AMHs (Meyer *et al*. 2012).

Interestingly, Johansson concludes that "evidence of successful interbreeding would […] add some modest weight to the case for Neanderthal language […] But it is not clear what form of language is supported". In essence, his argument goes as follows:

(1) Neanderthals could not be reproductively successful (within an AMH reproductive group) if they had lacked language.

(2) Therefore, they most likely had "a functioning language faculty" (Johansson 2013: 17).

(3) "A genetic endowment heterozygous[9] for the relevant genes [was] sufficient [for language]" (idem) — perhaps with the exception of *FOXP2*.

Notice, however, that:

(A) Communication was not out of range of Neanderthals, and a proto-language should have been sufficient for communication.

(B) Hence, one cannot take for granted that Neanderthals automatically had full language. Mallet's words caution against quick conclusions. This is important if we take into account the differences in development (see sections 2.2 and 2.3 above).

(C) Certainly, an AMH interacting with other AHMs by means of a pidginized version of the group language (or of her own language) resembles the scenario posited by Johansson quite closely. However, we do not derive from this that they are endowed with a different faculty of language or that there are cognitive differences between them.

(D) *FOXP2* is perhaps more the rule than the exception concerning the effect of heterozygosity on language abilities. In fact, many cognitive disorders in which language is impaired are caused by changes in gene dosage. For

[8] Johansson says that Africans do not have Neanderthal genes. This is technically inaccurate (Green *et al.* 2010 say *Sub-Saharians*), though we attribute this generalization to the recency of the paper published by Sánchez-Quinto *et al.* (2012), who show that North-Africans do have Neanderthal genes.

[9] That is, endowed with different alleles of the same gene; in this case, with only one copy of the language-ready genome.

instance, the duplication of a small fragment within the chromosomal region 7q11.23 gives rise to a mild to severe language impairment (Somerville *et al*. 2005; Berg *et al.* 2007). Conversely, language is more preserved (but still disordered) in hemizygotic people — the disease is known as Williams-Beuren syndrome —, while the visuo-spatial abilities are substantially impaired (Mervis & Becerra 2007; Martens *et al.* 2008).

However, our main criticism against any relevant role of the interbreeding fact in granting Neanderthals a modern faculty of language is of quite a different nature. For the sake of argument, we will leave aside the circumstance that current DNA analyses have only proved a gene flow from Neanderthals to AMHs, but not vice versa (Green *et al*. 2010). Obviously, we will obviate as well that evidence of modern language in Neanderthals is at least controversial, as Johansson himself discusses. To begin with, it seems that the introgression event did not prompt any significant cultural change among Neanderthals, in spite of the role commonly granted to (modern) language in dynamizing cultures. Hence, according to Green *et al.* (2010), the admixture took place ≈ 50–100kya BP, but the Châtelperronian and other modern-like techno-complexes only emerged ≈ 40kya BP (d'Errico *et al.* 1998; Langley *et al*. 2008).[10] Moreover, the introgressed DNA should have contained all the AMH-specific variants of the 'language genes' and of the corresponding regulatory mechanisms of gene expression. Importantly, these genes are scattered throughout the genome (Smith 2007; Benítez-Burraco 2012). However, we have direct evidence that Neanderthals exhibit the ancestral alleles in some cases (see above). Finally, if we could attest that the introgression event actually provided Neanderthals with the whole AMH-specific molecular machinery needed for language, an AMH faculty of language can still not be taken for granted. As we discussed at the end of the previous section, there is no direct link between the genotype and the phenotype.

2.5. Archaeological evidence

As he did before with other sources of data, Johansson has proficiently reviewed the extant evidence of symbolism and of 'modern behavior' among Neanderthals. However, we think that his analysis would benefit from a change of focus. To begin with, it is not symbolism or symbolic behavior *per se* which is at stake. From a semiotic perspective, human languages are certainly 'codes'. But natural languages are more than codes. As we discussed in section 2.1, linguistic meaning is compositional by nature (see also Hurford 2011). Modern, human-like language is a system of representation (and ultimately, of communication) that combines symbols — both hierarchically and recursively — to generate complex structures that include different sorts of dependencies between distal constituents (Chomsky 1965, 1980; Baker 2001; Hauser *et al.* 2002). What matters in our opinion is, above all, how linguistic structures are generated. Moreover, even if we found evidence of a symbolic culture among Neanderthals, we could not

[10] Some authors (*e.g.,* Bar-Yosef & Bordes 2010) even cast serious doubts on a possible Neanderthal authorship of the Châtelperronian industry.

automatically infer that they were endowed with a modern faculty of language. Symbolic cultures are opaque by nature (Eco 1976), whereas linguistic meaning is open, productive by nature. Ultimately, as we have already argued, other extant primates can learn and use symbols (Premack 1971; Savage-Rumbaugh 1986; Gardner *et al*. 1989; Savage-Rumbaugh & Lewin 1994).

Conversely, Johansson has just taken a quick look at archaeological evidence of syntax as originally posited by Uriagereka & Camps (2006). Johansson merely points out that these pieces of evidence are considered controversial or uninformative, according to some other authors (e.g., Lobina 2012). However, two lines of evidence suggest that this piece could be more informative than currently assumed:

First, under this fresh hypothesis, the computational system of language is thought to be functionally unspecific by nature. The functions to which it contributes depend on the systems with which it interfaces.[11] In fact, this is what ultimately qualifies knots as a proxy for syntactic abilities (Barceló-Coblijn & Gomila 2012). Motor behavior will help us to prove this:

- The field of motor disorders is familiar with the idea that voluntary motor actions are decomposable into motor primitives or 'movemes' (Del Vecchio *et al.* 2003). Movemes combine in diverse fashions according to specific combinatorial or syntactic rules (Flash & Hochner 2005). Moreover, the brain seems to rely on basic neural 'binding mechanisms' (like cortical synfire chains) to generate any kind of composite objects at the representational level. As Flash & Hochner also remark, "activities in synfire chains might bind and form a hierarchy of representations as required for language, [but] they might also offer a unique neural mechanism for compositionality of motor elements" (p. 663).
- There is also ample evidence suggesting that movements are controlled by a 'central' device, with peripheral, biomechanical factors playing a subsidiary role (Dipietro *et al.* 2009). fMRI studies suggest that motor processing activates cortical and subcortical areas that greatly match those involved in language processing. Significantly, the signal of two relevant components of that network (the bilateral ventral premotor area and the right posterior inferior temporal cortex) is transmitted via the arcuate fasciculus, which also plays a relevant role in language processing (see Makuuchi 2010 for a review).
- Lastly, there is ample evidence as well of the comorbidity between motor and language disorders. In most cases, this is due to the affectedness of the same specific brain areas, which probably perform some basic computation relevant for both language and motor planning. For instance, specific language impairment positively correlates with fine and gross motor deficits affecting limb movements (but not with rhythmic timing skills) (Zelaznik & Goffman 2010). In the same vein, dyslexia can be comorbid with drawing deficits. It has been argued that dyslexics suffer from a visuo-

[11] This idea goes back to the seminal paper by Hauser *et al*. (2002).

constructive deficit (Eden *et al.* 2003; Lipowska *et al.* 2011). Probably, they specifically suffer from a deficit in the rule abstraction mechanism inherent to sequential learning, which can simultaneously impair visuo-motor and linguistic tasks.[12] Similarly, Huntington disease is a neurodegenerative condition caused by the atrophy of the basal ganglia.[13] In this condition, both linguistic and motor deficits are observed (Teichmann *et al*. 2005; 2008; Robins Wahlin *et al.* 2010). Interestingly, knock-in mice expressing the human pathogenic *HD* gene exhibit abnormal prefrontal and cortico-striatal functions, which impair rule learning abilities, and ultimately, give rise both to visuo-spatial and motor deficits (Trueman *et al.* 2009).

A second line of evidence supporting the knotting hypothesis is that of cultural dynamism among hominins, which we have introduced in section 2.4. Contrary to the emergence of modern behavior within our clade — a 'classic' proxy in the field of language evolution, which Johansson has greatly clarified in his paper —, the controversy around cultural stasis *versus* cultural dynamism in Neanderthals and AMHs is not so frequently addressed. Johansson marginally mentions this: "[T]hroughout most of their existence, Neanderthals used Mousterian tools" (Johansson 2013: 20). The important point is not merely that complex language seems a key requirement for cultural dynamism in AMHs. It is that static and non-static cultures plausibly entail different organizations of mind, and particularly, different working memory capacities (Coolidge & Wynn 2005). Ultimately, more working memory resources could allow more complex linguistic structures to be generated (see the discussion in fn. 4). Consequently, if only AMH cultures are non-static, some important (for language) cortical reconfiguration plausibly occurred only in *Homo sapiens*, allowing full-fledged language to emerge (see Balari *et al*. 2011 and Balari & Lorenzo 2012 for details).

3. Some remarks on the methodology

Johansson has postulated a concept of 'proxy' for language (see his section 2.3). The advantage of this conceptual tool is that it is well defined and it constrains the possibilities. According to the author, for something to be qualified as a proxy for language:

- It has to be uniquely human: "[A] feature that is shared between humans and language-less non-humans is not a useful proxy for language" (Johansson 2013: 7).
- The absence of the proxy should ideally entail the absence of language: "Preferably, the entailment should be two-sided, so that the absence of the proxy likewise entails the absence of language" (Johansson, 2013: 7).

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[12] It would impair visuo-motor tasks demanding implicit learning of sequential stimuli along with generating complex motor patterns, such as drawing (Vicari *et al*. 2005), but also the ability for implicit learning of modified artificial grammars (Pavlidou *et al.* 2010).

[13] This subcortical area implements a sequencer device in some models of language processing (see Lieberman 2000; Ullman 2001; Balari *et al*. 2011; see also fn. 4).

Johansson correctly advocates as well not relying on just one single proxy for inferring either the presence or the absence of language in other species: "Instead of relying on any single one, a more prudent approach is to see if there is a common pattern to be found among multiple proxy-candidates" (p. 19).

That said, we think that Johansson has not followed his own advice till the very end. One the one hand, sometimes, we do not see any profound difference between his approach and the common search for something that is uniquely human and unique in AMH language, like the famous FLN/FLB distinction (Hauser *et al*. 2002).[14] But this approach ultimately entails that an AMH language is just a hominin language with an extra component. On the other hand, each of these proxies, when examined one by one, seemed essentially inconclusive to Johansson. Nonetheless, it is important to see the picture that emerges when *all* are simultaneously considered. If we consider language as a complex feature, then all the elements that play a role therein are important. Paraphrasing Gould & Lewontin (1979: 585) language does not seem to be "a collection of discrete objects" — in which one of them represents the kernel of language and a completely independent entity —, rather "an integrated entity".

Let us illustrate this with an example. From the continued discussion about the Neanderthal vocal tract between Philip Lieberman and Louis-Jean Boë,[15] we can extract at least a relevant aspect from each side:

a. Cavities and shape of the vocal tract are not enough, but the precise control of the organs, such as, for example, the jaw, lips, and tongue.

b. The neural substratum for the control and execution of speech is very particular in AMHs, involving cortical and subcortical areas, the basal ganglia in particular, although similar (i.e., homologue) circuitries are observed in other, non-related species (e.g., song birds).

Both (a) and (b) are connected, and hence both describe part of the reality. A modern vocal tract certainly does not entail *per se* modern language, but neither does a human-like neural circuitry. However, a modern vocal tract controlled by a human-like circuitry is plausibly suggestive of modern *speech*.

Summing up, because language is a complex feature intervened by many factors, we need to consider *all* small details in order to enhance our knowledge. Ultimately, it is not so much a matter of when *a* component of language appeared, but, above all, of when *all* the components were put together (i.e. functionally interconnected). And of course, we should also consider the possibility that some properties of language are emergent by nature. In other words, they

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14 Fitch, one of the proponents of the FLB/FLN distinction, has recently said:

> Given the fact that human cultural capacities themselves rest upon a unique biological basis, the debate actually hinges on a distinction between 'general cognitive' and 'specifically linguistic' neural mechanisms in our species. I suggest that, from a biological viewpoint, this distinction is unproductive and misleading, and that the debates surrounding it have led cognitive science down a blind alley.
> (Fitch 2011: 383).

15 For example, Lieberman & Crelin (1971), Boë *et al.* (1999), Boë *et al.* (2002), Lieberman (2007), Boë *et al.* (2007), and Lieberman (2012); see also Barney *et al.* (2012).

cannot be predicted from the nature and the interplay of their components at a particular level of analysis (see Deacon 2005).

4. A stringent biolinguistic approach to Neanderthal language

We think that a stringent biolinguistic approach to Neanderthal language is actually possible. An outline of this could be as follows. If our hypothesis is correct, all hominin proto-languages (including Neanderthals') could have been endowed with regular-like grammars (like apes' 'languages') or perhaps with context-free-like grammars. Conversely, a fully-fledged modern syntax (which is mildly context-sensitive) would have only emerged along with our own species. This entails that the Neanderthal-AMH divide would basically consist of a brain reconfiguration that improved the computational abilities of the latter. Perhaps, it enhanced their working memory, or alternatively, it allowed more efficient information exchanges (see Balari & Lorenzo 2012 and Boeckx 2012, respectively, for details). But it was this reconfiguration that eventually facilitated the advent of modern syntax.

In turn, most components of speech were very probably shared both by Neanderthals and AHMs — allowing for minor differences.[16] The Neanderthal interface between this audio-vocal (exteriorizing/interiorizing) system and a conceptual system (responsible for thought) could have been firmly established as well. As a consequence, an oral, "symbolic communication system that is not fixed" (Johansson 2013: 6) in Neanderthals is granted; at the same time, the grammar of this language (or protolanguage) would almost certainly have been different from that of AHMs' languages.

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[16] The hypothesis that orality replaced gestuality sometime after the split from extant apes is conceivable, though a scenario of co-evolution and co-intervention is also plausible. Eventually, the latter, in particular, would explain why sign languages and oral languages co-exist in modern humans and obey the same neural pattern regarding the centers of comprehension and production, although they obey different peripheral paths of input and output.

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★ FORUM ★

Neanderthals between Man and Beast: A Comment on the Comments by Barceló-Coblijn & Benítez-Burraco (2013)

Sverker Johansson

1. Neanderthal data and Neanderthal perspectives

Lluís Barceló-Coblijn and Antonio Benítez-Burraco (2013; henceforth BB13) have provided some insightful comments on and pertinent criticisms of my previous article 'The talking Neanderthals' (Johansson 2013; henceforth J13).

First, I appreciate their kind words about my review of the evidence. It appears that we largely agree on the facts of the matter, and also that we agree on the main conclusion of J13, that, as they express it, "Neanderthals had to count on some form of language" (BB13: 199). Our disagreements are more a matter of perspective, interpretation, and methodology. BB13 have two main criticisms:

(1) They believe it is possible to infer "that the Neanderthal language was not like AMH's [anatomically modern human's] because it lacked *modern* syntax" (BB13: 199–200, original emphasis), and imply that I am too timid in refraining from drawing that conclusion in J13.

(2) They disagree with my interpretation or methodology on a number of specific points throughout J13.

It is also interesting to read J13 and BB13 in the light of another recent review of the same topic by Dediu & Levinson (2013), published shortly after BB13. Dediu & Levinson reach largely the same conclusions as J13 (but along slightly different routes) and go one step further in that they (like BB13) do take a stand on whether Neanderthals had modern language. But their conclusion is the opposite from BB13: Neanderthals did have "essentially modern language" (Dediu & Levinson 2013: 1).

2. On language, communication, and productive debates

As BB13 now concede the presence of "some form of language" in Neanderthals, and also state that "[s]yntax is not an all-or-nothing question within this frame-

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work" (BB13: 200), they apparently accept a non-monolithic language concept. But it is clear from their section 2.1 that we do not fully agree on the details, though it is less clear precisely what their language concept looks like.

Within this non-monolithic framework, BB13 still give a privileged place to AMH language, and find interest in Neanderthal language only as it relates to AMH language: "[T]he real productive debate is whether or not Neanderthals had the *same* faculty of language that [AMHs] have" (BB13: 199, emphasis in original) and "what is really worth characterizing is the proto-typical AMH language, and then to determine whether or not Neanderthals could have developed something like this" (BB13: 201). I agree that this is an interesting question. But I do not agree that this is the *only* interesting question, or even "the real productive debate" concerning Neanderthal language. Evaluating Neanderthal language solely on whether they match AMH or not is too flavored with *scala naturae*, with us as the pinnacle of creation, for my taste. Neanderthals, and the Neanderthal language faculty, are well worth studying in their own right, not just as poor relatives of AMHs.

If the Neanderthal language faculty were the same as the AMH language faculty, this would not tell us much new about the computational structures of language — but it would have far-ranging implications for the study of language evolution, falsifying a long list of AMH-specific hypotheses of language origins.

On the other hand, if the Neanderthal language faculty were *different*, as BB13 contend, and given that we already agree that they *did* have some form of language, and thus a language faculty, this would show that there is more than one way to build a language faculty. Such a discovery could catalyze fruitful investigations into possible alternative structures of language faculties, today overshadowed by our focus on the AMH language faculty.

The choice of null hypothesis is a key issue here (cf. section 2.4 in J13). BB13 state that "… we actually can proceed with a null hypothesis: In our opinion, current evidence supports that the Neanderthal language was not like AMH's because it lacked *modern* syntax…" (BB13: 199–200). This sounds like they are positing their conclusion as null hypothesis, which skirts very close to the *petitio principii* fallacy, assuming your conclusion. Later, they state that "[i]n fact, it is our contention that the available data do not support non-AMH hominin 'languages' being syntactically structured like AMH languages are" (BB13: 201).[1] I actually agree with this contention — but also with its complement, that the available data likewise do not support that non-AMH languages are *not* structured like AMH languages. As I said in J13, "whether they had syntactic language can be neither confirmed nor refuted." (p. 23). In the absence of evidence, the null hypothesis becomes the conclusion by default. There are several statements in BB13 where they reiterate their assumption that AMH language is unique among hominins, notably in their conclusions at the end; but nowhere in BB13 can I find any actual positive evidence supporting this assumption (as opposed to a lack of evidence to the contrary).

On syntax, I can concede that I did not define it carefully enough in J13.

[1] The scare quotes on 'languages' here seem to contradict their earlier statement that "Neanderthals had to count on some form of language" (BB13: 199). Did Neanderthals, in the opinion of BB13, have language or 'language'?

Instead of going into a full discussion here of all the complexities of this issue, I will in the interest of brevity simply state that I largely agree with the model posited by Jackendoff & Wittenberg (in press); see also Johansson (2005). This circumvents, among other things, the false dichotomy that BB13 raises between syntax in the loose sense 'to put in order' and in the strong sense of modern AMH syntax.

Concerning lexical semantics, there are indeed many current theories on lexicon structure, another issue that I will abstain from discussing here at the length it properly deserves. Suffice to say that most people agree that modern language contains some kind of trilateral mappings between form, meaning, and syntactic features. There are various frameworks for describing this mapping, including both frameworks that don't call it 'lexicon' at all, and frameworks that give primacy to the lexicon over syntax (e.g., Boeckx 2013), but I will leave that aside; *some* way of connecting form and meaning, and plugging it into syntax, is required for modern language. Removing one of the three sides of the trilateral mappings leaves something that one may or may not wish to call a lexicon, but it is not logically incoherent; it is simply a bilateral mapping. Boeckx (2013) apparently argues for an early stage in language evolution with bilateral mapping connecting syntax with meaning, lacking the externalization ('form') mapping. I would instead argue for the possibility of a bilateral mapping with form and meaning (cf. Jackendoff & Wittenberg, in press); such a bilateral mapping, freely extensible, is the minimum required for me to call something 'language'. I concede the logical possibility (if not the plausibility; see Johansson, in preparation) of the scenario of Boeckx (2013), but would not call it language.

Ape 'language' (Kanzi *et al.*) is invoked by BB13 in an apparent attempt at a *reductio ad absurdum* of my language definition, both concerning syntax and lexical semantics. This *reductio* fails on several points: (1) it works as a *reductio* only if ape language is inherently absurd, which it is only if language in all forms is assumed *a priori* to be unique to humans, (2) apes do not display these language-like behaviors in the wild, only when taught by humans,[2] (3) there is scant evidence that apes do any (proto-)lexical mapping on their own, beyond those mappings provided by humans, and (4) the productive 'syntax' displayed in ape utterances fits only a very loose definition of syntax, much looser than the one I adopt from Jackendoff & Wittenberg (in press).

The issue that BB13 raise concerning function versus structure (p. 202) is not a productive debate. It is a fallacy to place questions of structure and function in opposition — instead they are complementary questions, belonging to different levels in Tinbergen's (1963) classification of explanations in biology. The function and the structure of a biological feature, such as language, are both interesting questions, and neither should be neglected in a proper biolinguistic analysis, nor should either be given primacy over the other. With that said, there are methodological considerations involved in inferring structures and functions in extinct species, that BB13 do not fully take into account. To put it briefly and simply: Only structures that fossilize can be studied directly; functions may be

[2] *Why* apes have a capacity to learn language-like behaviors, a capacity that is to all appearances unused in the wild, is a very interesting question, but is beside the point here.

inferred either from fossilizable structures, or from behavioral traces; non-fossilizable structures, such as computational devices, can only be inferred indirectly through the behavioral traces produced by their functioning. Even for modern humans, where we have full access to both behavior and soft tissue, there is still no consensus on the structure of the computational device behind language.[3] For this reason, the emphasis of BB13 on the evolutionary trajectory of this computational device, ahead of function, is misplaced, as this is effectively unknowable without going through functional inferences that are difficult and contentious even in living humans.

3. Interpretative and methodological issues

3.1 Globular or lateral brains?

Concerning the neural substrate of language, BB13 first invoke allometry and the structural changes that may follow from size changes. This is highly relevant when comparing for example the brains of humans and chimpanzees, as it is a non-trivial issue to disentangle which structural differences are just byproducts of the threefold size difference. But as there is no significant difference in size between Neanderthal and AMH brains,[4] allometry does not contribute anything informative to the issue at hand. The average brain development trajectory , as invoked by BB13, may indeed be different (Gunz *et al.* 2012), though this issue is not totally settled yet. But it remains to be shown whether this difference is relevant for language. As noted by Benítez-Burraco (2013), language ontogeny in AMH is highly robust against perturbations, presumably including the full range of development trajectories of AMH, and there is no evidence that this robusticity does not extend to the Neanderthal pattern.

The argument from globularity of Boeckx (2012) is more interesting, as it focuses on the main difference between Neanderthal and AMH skulls (and presumably brains), the more globular shape of AMH skulls. But while the idea is intriguing and well worth pursuing further, especially in connection with developmental patterns, at present the proposed link from globularity to language is purely speculative and cannot warrant any conclusions concerning Neanderthal language.

On lateralization, I do not see any major disagreements between J13 and BB13. As is clearly stated in J13, the proposed link between handedness and language is not strongly supported, and cannot stand on its own as evidence of language. But I think BB13 are overstating their case for continuity somewhat; while lateralization in various respects is indeed ubiquitous and ancient among many animals (not just mammals), the population-level handedness ratio of humans is not.[5]

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[3] Nor is there consensus on whether the primary function of language is communication or something else, though the vast majority of language-evolution researchers work on communicative hypotheses. But in the interest of brevity I will leave that debate for a different day (Johansson, in preparation).

[4] If anything, the Neanderthal average brain size is slightly *larger* than that of AMH. But the difference is slight, and all known Neanderthals are well within the AMH size range.

[5] Whether there is *any* population-level handedness among other apes remains a contentious issue (J13: 47, fn. 7). Hopkins and associates, cited by BB13, are just one side of that debate.

3.2 On linguistic genotypes

On the genetic issues, there is again little disagreement between me and BB13 on substantial issues, but much disagreement on interpretation.

Concerning FOXP2, as noted also by BB13 this is a gene with both sequence and physiological role highly conserved, certainly among mammals and likely among vertebrates.

Concerning other possibly language-related genetic changes in the human lineage, Dediu & Levinson (2013) review these in more detail than either J13 or BB13. In the interest of brevity I will not go through the whole list here, but instead just state that I largely agree with the analysis of Dediu & Levinson. Worth mentioning is just the work of Maricic *et al.* (2013), cited by both BB13 and Dediu & Levinson (2013), according to which a regulatory region of FOXP2 has changed in AMH but not Neanderthals. What Dediu & Levinson (2013) but not BB13 mention is that Maricic et al. (2013) find the ancestral allele present at a frequency of about 10% in some modern African populations. If this change made a key difference with respect to language, the effect on language in these populations ought to be obvious.

The statement of BB13 that "even if they [Neanderthals] were endowed with the same 'linguistic genotype' […], we cannot automatically rule out the possibility that the former had a different faculty of language" is not supported by the argument of Benítez-Burraco (2013) that language development is highly robust in humans, also against variations in the 'linguistic genotype': "In particular, we argue that developmental dynamics (and hence, an assorted set of regulatory factors) strongly canalizes variation, to the extent that the same phenotype can robustly emerge at the term of growth from diverse genotypes" (Benítez-Burraco 2013: 1). It is technically true that different outcomes from the same genotype cannot be ruled out — in some contexts this is even fairly common, influenced by environmental cues (cf. West-Eberhard 2003). But language development of AMHs is clearly highly robust against variation in the external environment, with AMH babies throughout history developing normal AMH language faculties despite a range of environmental variation that encompasses and exceeds the typical environment of Neanderthals. What remains is the possibility that the 'internal environment' in the child during ontogeny differs systematically in Neanderthals in ways that go beyond the robusticity limits of language development; it is unknown whether this is actually the case, and pure speculation either to assume that it is or that it isn't.

3.3 Introgression among strawmen

In their section 2.4, BB13 say they are arguing against section 5 of J13. However, the position that they are apparently attacking is nowhere to be found in J13.

Their statements — "[h]ence, one cannot take granted that Neanderthals automatically had full language" (p. 205) and "[h]owever, our main criticism against any relevant role of the interbreeding fact in granting Neanderthals a modern faculty of language is of quite different nature" (p. 206) — indicate that BB13 are arguing against someone who believes that interbreeding proves that Neanderthals *had* full language. Whoever that someone might be, it is not J13,

where this conclusion explicitly is *not* drawn, notably in the two sentences quoted by BB13 on their page 205, repeated here in full from J13 (p. 18): "Evidence of successful interbreeding would thus add some modest weight to the case for Neanderthal language, despite some caveats about heterozygotes and mating systems. But it is not clear what form of language is supported." The second sentence in the quote makes it abundantly clear that J13 does *not* jump to the conclusion that Neanderthals had full modern language, but instead leaves the issue open. It is also explicitly stated in J13: "[W]hen I talk about 'some form of language', this includes proto-language" (p. 7), but in several places BB13 are arguing as if "language" in J13 means full modern language. BB13 are criticizing a strawman here, possibly caused by misreading how I use the word "language" in J13.

Another strawman is erected in the final paragraph of section 2.4 in BB13. Here they are apparently arguing against a claim that introgression from AMHs to Neanderthals transferred 'language genes' to the Neanderthal population, giving them language. I fail to see how anybody could possibly misunderstand J13 as making such a claim. Possibly Benítez-Burraco (2012), where the same counterargument appears, is contaminating their reading of J13?

Concerning North Africans (BB13: 205, fn. 8), their point is technically correct, but irrelevant. I wrote "Africans" as shorthand for "Sub-Saharan Africans" (J13: 17), which was admittedly sloppy, but the status of North Africans (Sánchez-Quinto *et al.* 2012) has no impact on the argument of J13, which does not hinge on the specific AMH populations possibly affected by introgression.

BB13 are quite correct in noting that introgression is common in the animal world, and I would not be surprised if AMH and Neanderthals did interbreed. But BB13 jump to conclusions too quickly when they treat interbreeding between AMH and Neanderthals as if it were a proven fact. The case for interbreeding is still not robust enough for categorical statements like "Neanderthals and AMHs interbred" (BB13: 205) or "the interbreeding fact" (BB13: 206), even though some further indirect support exists from, for example, Yotova *et al.* (2011).[6]

3.4 To knot, or not to knot?

On the 'symbolic' archeology, I see no major disagreements, except the issues of structure vs. function and language *sensu* J13 vs. language *sensu* BB13 already discussed earlier.

When it comes to alleged archeological proxies for syntax, however, our conclusions differ substantially. As noted by BB13, the attempt by Camps and associates (Camps & Uriagereka 2006; Balari *et al.* 2012) to tie a knot between knots and syntax is given rather short shrift in J13. This is not only because I find the knot-syntax connection *per se* unconvincing (cf. Lobina 2012; Lobina & Brenchley 2012), but also because it would be uninformative with respect to Neanderthal language even if it were established that a knot is a proxy for syntax. As noted in J13, we have no direct evidence of knot-making among Neanderthals. But we do have indirect evidence in the form of technologies — hafting (e.g., Cârciumaru *et al.* 2012), clothing (e.g., Wales 2012), and possibly

[6] Incidentally, Yotova *et al*. use the same "African" shorthand for which BB13 berate J13.

pendants (e.g., Zilhão *et al.* 2010) — that typically involve knots, so it would be imprudent to assume that a Neanderthal couldn't tie a knot, and unwarranted to conclude from this that they had no modern syntax.

The various connections between language and the motor system invoked by BB13 in this context are unobjectionable in themselves. But BB13 fail to show how these points tie into the knot-syntax argument. A general language-motor tie is not evidence of a specific knot-syntax connection.

Otherwise, I have considerable sympathy for the hypothesis that the computational machinery behind language is not domain-specific, but used for computations in many areas, be it tool use (cf. Barceló-Coblijn & Gomila 2012), navigation (cf. Kinsella 2009), music (cf. Asano 2013), or whatever. I am just not convinced that actual knot-tying (as opposed to doing knot theory) is one of those areas.

3.5 Who is dynamic, and who is static?

In support of the knot notion, BB13 invoke the cultural dynamism of AMH. How knotting causes dynamism is not shown, but the issue of cultural dynamism is interesting in its own right, with or without knots, and I thank BB13 for pointing this out, as J13 did not give it enough attention.

By 'cultural dynamism' I presume that BB13 mean the much more rapid rate of cultural and technological change, and the cumulative effects of such change, in some human populations compared with earlier hominids. This difference in rate is certainly real when comparing *Homo* with other extant apes, among whom the rate of cumulative cultural change is indistinguishable from zero. And even in *Homo erectus* the rate is very modest, with a recognizably Acheulean tool kit changing very gradually over a million years or so. But during the last few hundred thousand years, the situation is more complex. Compared with today's Western culture, the rate of change remained glacial well into the Holocene — but compared with any previous population, it accelerated dramatically, both among early AMHs and among other contemporary humans, including Neanderthals. In the last 50,000 years, further acceleration took place among some, but not all, populations of both AMHs and Neanderthals. On one hand, the Châtelperronian shows the accelerating cultural dynamism of a subset of Neanderthals (see, e.g., Soressi *et al.* 2013 for one recent piece of evidence) — and on the other hand, the recognizable continuity of San culture back to 44,000 years ago (d'Errico *et al.* 2012) shows that not all AMH populations accelerated in the same way. Cultural dynamism is an issue that clearly deserves more attention as a proxy for cognitive evolution, and most recent AMHs are indeed more dynamic than most Neanderthals — but it is far from a clear-cut case of all AMHs being dynamic and all non-AMHs static.

It is also interesting to note that BB13 invoke differences in working memory (Coolidge & Wynn 2005) as an explanation for the difference in dynamism. This is a defensible, if speculative, hypothesis — but working memory is part of the performance system, well outside the core language systems, and the dynamism issue would in that case *not* support any difference in core syntax between Neanderthals and AMHs.

4. Summing up: Stringent, stringenter, stringentest

Both J13 and BB13 agree on the basic methodological issue that we should not seek a single "magic bullet" proof of language, but instead look at the total pattern formed by all the various proxies and other types of evidence available. Likewise, we both agree that our own implementation of this methodology is the proper stringent one, and in the other paper it is done wrong.

But I also believe that BB13 in their Section 3 are barking up the wrong methodological tree. Our different conclusions do not really hinge on differences in how we apply the pattern-forming methodology that we have in common. Instead our key differences are two:

(1) *What, if anything, is the proper null hypothesis in Neanderthal studies?*

As discussed in section 2 above, BB13 posit a null hypothesis that, in the absence of positive evidence one way or the other, subsequently becomes their conclusion. I think it is methodologically more stringent to avoid *petitio principii* and refrain from conclusions in such a case.[7]

(2) *Is language a monolithic integrated entity, or can there be different ways of having language?*

My position, here as well as in J13, is that we should not assume *a priori* that language faculties that are not identical to the AMH one are impossible. The position of BB13 on this issue is unclear, or possibly inconsistent; on one hand, they admit that Neanderthals had "some form of language" (p. 199) or "could have been endowed with regular-like grammars" (p. 210) but deny them full modern language, which entails that BB13 grant Neanderthals a language faculty different from the AMH one. On the other hand, in Section 3 they appear to be arguing that language is "an integrated entity" (p. 209) and "it is not so much a matter of when *a* component of language appeared, but, above all, of when *all* the components were put together" (p. 209, emphasis in original), which sounds more like an argument for a monolithic indivisible language faculty. And throughout their paper, as discussed in section 2 above, they argue as if the only issue were whether Neanderthals have an AMH language faculty or not, which likewise sounds as if they believe different language faculties are either impossible or irrelevant.

We agree that somewhere along the human lineage things happened that "improved the computational abilities" (BB13: 210) that are relevant for language. But unlike BB13, I would argue that it remains to be shown both (i) whether this was a single step, or multiple steps, and (ii) whether the step(s) took place before or after the split between AMHs and Neanderthals. We agree that Neanderthals had some kind of language, and thus some kind of language faculty, which entails that at least some of the above-mentioned steps took place before the split. But for the reasons given throughout both J13 and this paper, I remain agnostic on how the language faculty that we agree that the Neanderthals did have compares with the AMH language faculty.

[7] I likewise believe that Dediu & Levinson (2013) are somewhat premature in jumping to the opposite conclusion from BB13.

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The Creative Aspect of Language Use and the Implications for Linguistic Science

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The creative aspect of language use provides a set of phenomena that a science of language must explain. It is the "central fact to which any significant linguistic theory must address itself" and thus "a theory of language that neglects this 'creative' aspect is of only marginal interest" (Chomsky 1964: 7–8). Therefore, the form and explanatory depth of linguistic science is restricted in accordance with this aspect of language. In this paper, the implications of the creative aspect of language use for a scientific theory of language will be discussed, noting the possible further implications for a science of the mind. It will be argued that a corollary of the creative aspect of language use is that a science of language can study the mechanisms that make language use possible, but that such a science cannot explain how these mechanisms enter into human action in the form of language use.

Keywords: creative aspect of language use; externalism; (Chomskyan) internalism; linguistic science; science of the mind

1. The Creative Aspect of Language Use

The creative aspect of language use provides a set of phenomena that a science of language must explain. It is indeed the "central fact to which any significant linguistic theory must address itself" and thus "a theory of language that neglects this 'creative' aspect is of only marginal interest" (Chomsky 1964: 7–8). As a result, the form and explanatory depth of a science of language will be restricted in accordance with this aspect of language. I will discuss the implications of the creative aspect of language use for a scientific theory of language, noting along the way the possible further implications for a science of the mind. I will argue that a corollary of the creative aspect of language use is that a science of language can study the mechanisms that make language use possible, but for reasons to be explored such a science may be unable to shed light on how these mechanisms enter into free human action in the form of language use.

The creative aspect of language use refers to the kind of linguistic creativity

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that is displayed in ordinary human linguistic production and comprehension. All humans have the ability to produce and understand an infinite number of novel sentences — sentences that are new in the linguistic experience of the speaker/hearer and perhaps also new in the history of their language. Descartes saw an essential difference between humans and other animals that was most clearly exhibited by our linguistic ability to form new statements, which express new thoughts and are appropriate to but not directly caused by their contexts. Chomsky (2002 [1966]: 53) summarises Descartes's views as follows:[1]

> […] it is the diversity of human behavior, its appropriateness to new situations, and man's capacity to innovate — the creative aspect of language use providing the principal indication of this — that leads Descartes to attribute possession of mind to other humans, since he regards this capacity as beyond the limitations of any imaginable mechanism. Thus [according to Descartes] a fully adequate psychology requires the postulation of a 'creative principle' alongside of the 'mechanical principle' that suffices to account for all other aspects of the inanimate and animate world and for a significant range of human actions and 'passions' as well.

The creative aspect of language use thus poses a problem for a science of language because human language, "being free from control by identifiable external stimuli or internal physiological states, can serve as a general instrument of thought and self-expression rather than merely as a communicative device of report, request, or command [as animal communication systems appear to be]" (ibid., 57). In other words, the problem is how to account for the creative aspect of language use in a scientific context when it appears to be a form of free human action. The solution to this problem involves accepting that the mechanisms underlying the creative aspect of language use can be a fruitful subject matter for a science of language, but that language use itself may not be.

The main issues that Descartes raised in regard to human language use are that (1) it allows for an unbounded expression of thought and (2) it is independent of direct stimulus control yet at the same time (3) it is appropriate to new situations and coherent in new contexts.

1.1. Unboundedness

Linguistic productivity is the ability to produce and understand an unlimited number of sentences that one has not previously encountered. Descartes viewed productivity in all domains — language, mathematics, vision, etc. — as deriving from a single source. Modern cognitive science has taken a modular approach, insisting that each domain has its own productivity engine (cf. Brattico & Liikkanen, 2009). In order for a grammar to be able to produce from the set of finite primitive elements an infinite set of expressions it must be recursive. The details of the notion of recursion need not concern us here, suffice it to say that it involves embedding a structural object within another instance of itself — as when a noun phrase is embedded within another noun phrase (for more on recursion, see Parker 2006, Tomalin 2007, and Zwart 2011). Non-linguistic examples

[1] Cf. den Ouden (1975), Bracken (1983), and D'Agostino (1984); see also Schouls (2000) for detailed discussion of Descartes' views on the nature and possibility of science.

include the way in which the set of natural numbers is defined recursively, recursion in music (e.g., Jackendoff & Lerdahl 2006), or the recursion that is displayed in spatial reasoning and navigation. Fitch *et al.* (2005, p. 186) illustrate recursion by asking the reader to consider "such concepts as ((((the hole) in the tree) in the glade) by the stream) and ask whether there is an obvious limit to such embedding of place concepts within place concepts (… in the forest by the plain between the mountains in the north of the island…)".

1.2. Stimulus Freedom

The second issue Descartes raised in regard to the creative aspect of language use relates to the fact that a person's use of language is stimulus-free in the sense that verbal behaviour is "free of identifiable external stimuli or internal physiological states" (Chomsky 2002 [1966]: 110, fn. 11). That is:

> Though our language use is appropriate to situations, it is not controlled by stimulus conditions. Language serves as an instrument for free expression of thought, unbounded in scope, uncontrolled by stimulus conditions though appropriate to situations, available for use in whatever contingencies our thought processes can comprehend. (Chomsky 1980: 222)

One can easily think of examples that show this sort of stimulus freedom. One can speak of elephants when there is nothing in the speaker's environment that could conceivably be called a stimulus that caused the utterances. Or one could speak of Federico Lorca's *Poet In New York* when the only conceivable stimulus in the speaker's environment is elephants and the African landscape. Under no notion of causality can such utterances be said to have been caused by anything in the speaker's environment. If one does attempt to offer a casual explanation it will not be causality as scientifically construed, but rather the interpretation of a speech event as part of a pattern that can only be identified a posteriori (cf. McGilvray 2001).

Stimulus-freedom implies not only that language use has no direct causal relation with the environment of the speaker/hearer; Chomsky also argues that there is a sense in which language use has no strict causal relation with internal states either. Thus, he remarks that "Descartes and his followers observed that the normal use of language is constantly innovative, unbounded, *apparently free from control by external stimuli or internal states*, coherent and appropriate to situations" (Chomsky 1988: 5, my emphasis). Elsewhere, Chomsky refers to a normal feature of everyday usage of language: "the fact that it is typically innovative, *guided but not determined by internal state and external conditions*, appropriate to circumstances but *uncaused*, eliciting thoughts that the hearer might have expressed the same way" (Chomsky 1996: 17, my emphasis).

The issue at hand, however, is not the existence of internal or external causes, but rather the viability of including environmental causes or specific internal causes of language use within a scientific theory of language. A scientific theory of language cannot be a fruitful and deeply explanatory one if it insists on including such purported causes or correlations with the environment — or, given the proper qualifications, with internal states.

1.3. Coherence and Appropriateness to Circumstance

"The normal use of language", writes Chomsky, "is thus free and undetermined but yet appropriate to situations; and it is recognised as appropriate by other participants in the discourse situations who might have reacted in similar ways and whose thoughts, evoked by this discourse, correspond to those of the speaker" (Chomsky 1988: 5). In other words, linguistic "discourse is not a series of random utterances but fits the situation that evokes it but does not cause it" (ibid.). People assume that the utterances of their interlocutors are relevant, coherent, and appropriate to the circumstance at hand. And even when an utterance fails to do so, we impose an interpretation on it in which it is assumed to be relevant, coherent, and appropriate.

A science of language has to deal with the fact that novel sentences are appropriate to though not determined solely by the circumstances of their use. If in addition to the mechanisms that make language use possible, a theory insists on including within its scope aspects of language use then it must contend with the fact that it is unclear what counts as a relevant or appropriate circumstance (e.g., Giora 1997). Claiming that a circumstance is that which is *judged* to be coherent by a speaker/hearer only poses the question to be answered and does not provide any insight. Wilson & Sperber (2004: 611), for example, believe that:

> The fact that ostensive stimuli create expectations of relevance follows from the definition of an ostensive stimulus and the Cognitive Principle of Relevance. An ostensive stimulus is designed to attract the audience's attention. Given the universal tendency to maximise relevance, an audience will only pay attention to a stimulus that seems relevant enough. By producing an ostensive stimulus, the communicator therefore encourages her audience to presume that it is relevant enough to be worth processing.

However, everyday language use is replete with ambiguities, allusions, metaphors, and many other similar phenomena, and contexts of speech are enormously varied and only tenuously related to particular utterances. It is thus unlikely that one can construct a theory that, say, systematically lists the circumstances to which a particular utterance is supposed to be appropriate. The reason is that, as Descartes noticed, although expressions are appropriate to circumstances, they are stimulus free and causally unrelated to the speaker's environment. A fortiori, *being appropriate* cannot be equated with *being caused by environmental conditions*, for the purported correlation between language and the world is suspect (cf. McGilvray 2001). This is the externalist conception of semantics criticised below.

It is important to stress that the claim is not that correlations do not exist. Rather, the claim is that even though correlations may exist in some form, they are not a fecund subject matter for a serious science of language. One may object that, say, relevance theory in pragmatics or formal semantics do not aim at the rigour, formal structures, or explanatory methods or models of science per se. However, there are plenty of theorists who explicitly claim that their theory of language is scientific in the sense that it can posit lawful correlations between linguistic behaviour and aspects of the environment and the contexts in which utterances are produced. Paul Horwich (1998, 2005) is a case in point — he claims

that his use-based theory of semantics is compatible with a linguistics construed as an empirical science.

To recap, then, the main issues that Descartes raised in regard to the creative aspect of language use are: that language use allows for an unbounded expression of thought and is independent of direct stimulus control, yet at the same time it is appropriate to new situations and coherent in new contexts. Before detailing the implications that such observations have in regard to a science of language, what follows are some remarks about linguistics and science.

2. Linguistics and Science

For the purposes of this article one can make a distinction between two methods of constructing a scientific theory of language: an externalist approach and an internalist approach. The classic arguments for externalism are found in Putnam (1975), Burge (1979), and Kripke (1980).[2] The main externalist claim is that mental states are individuated by reference to environmental features or social contexts, and therefore in order for a person to have intentional mental states they must be related to the environment in the right way. Externalism entails that if two individuals are physically identical their respective utterances of, say, *water*, can still have different meanings if the relevant features of their environment are different.

Externalism has become a widely held position that is especially popular within the philosophies of mind and language. Indeed, some feel that "externalism has been so successful that the primary focus of today's debate is not so much on whether externalism is right or wrong, but rather on what its implications are" (Wikforss 2008: 158), and that "Over the past 30 years much of the philosophical community has become persuaded of the truth of content externalism" (Majors & Sawyer 2005: 257). Externalism has thus become "almost an orthodoxy in the philosophy of mind" (Farkas 2003: 187).

Internalism, on the other hand, holds that, *for the purposes of scientific inquiry* into language, the internal properties of the human mind are the relevant and fruitful subject matter of scientific research. Internalism (more specifically, *Chomskyan* internalism) has thus recast the notion of language qua social phenomenon or abstract object into a form that is susceptible to empirical scientific inquiry. Hinzen provides the following succinct definition of Chomskyan internalism:

> Internalism is an explanatory strategy that makes the internal structure and constitution of the organism a basis for the investigation of its external function and the ways in which it is embedded in an environment.
>
> (Hinzen 2006: 139)

[2] Cf. also Burge (1986), Davidson (1987), and McGinn (1989). Wikforss (2008) is an excellent overview and discussion of externalism. It should be noted, however, that even though the umbrella term *externalism* applies to them all, these citations of externalists should not be taken to imply that they all necessarily have similar arguments or that they are in agreement with one another.

Internalism thus studies the internal structure and mechanisms of an organism; the external environment comes into the picture when the internal processes are ascribed content by the theorist, thus explaining how the internal mechanisms constitute a cognitive process in a particular environment. Such content ascriptions vary with the theorist's interests and aims, but the (ascription of) content is not an essential part of the internalist theory itself (cf. Egan 1995).

I argue that whatever merits externalism may possess and despite its popularity, it is unable to provide a fruitful framework for a scientific theory of language. One might object, however, that externalists do not see their enterprise as scientific and thus it is a moot point to compare it to other scientific pursuits. But there are externalists (Putnam, Davidson, Horwich, Fodor, Burge, and others) who explicitly state that their theory is a scientific one. Thus, since both externalists and Chomskyan internalists claim their theories to be scientific, it is possible and illuminating to compare the two from the perspective of scientific explanatory strategies and to ask which of the two is the most promising avenue in regard to constructing an explanatory scientific theory of language.

In other words, while it is true that externalists discuss their theories in terms of the determination of mental content, this does not preclude assessing their theories from the point of view of explanatory scientific strategy. As is the case with Chomskyan internalists, externalists attempt to explain the phenomena of language production and comprehension, and thus it is valid to assess the success of these explanations and compare them to competing theories that also try to explain the same phenomena. That is, substantive theoretical or philosophical differences are necessarily also ones of explanatory strategy. Since the aim of science is to construct theories that explain and predict phenomena, it is valid for one to compare these two approaches that claim to be scientific from the point of view of explanatory strategies.

2.1. Internalism, Externalism, and Science

Debates about the scientific status of linguistic theories are of course nothing new. Robert Lees's review of Chomsky (1957) argues that it was one of the first serious attempts at linguistic science "which may be understood in the same sense that a chemical, biological theory is ordinarily understood by experts in those fields" (Lees 1957: 377). Lees is one of the first in a long tradition that has supported the scientific claims of generative linguistics. Recently, John Collins remarked that "the greatest service Chomsky has provided for philosophy is to do philosophy of science via the construction of a new science" (Collins 2008: 25; see also Collins 2006). James McGilvray argues in regard to the "cognitive aspect of the faculty of language, or the computational system itself" that "there is a serious scientific enterprise devoted to its investigation, and with respect to capturing its structure, at least, there has been considerable progress" (McGilvray 1998: 238). Moreover, he says that he is "perfectly happy to say that the various branches of syntax are physical sciences, even if they are sciences of what is in the head, for all that 'physical' means is that one has an honest science" (p. 243).

Another example is Alec Marantz, who states that mainstream generative linguistics "operates at the nexus of computation, philosophy of language, and

cognitive neuroscience" (Marantz 2005: 431). Cedric Boeckx and Massimo Piattelli-Palmarini write that "[t]he Chomskyan revolution in linguistics in the 1950s in essence turned linguistics into a branch of cognitive science (and ultimately biology) by both changing the linguistic landscape and forcing a radical change in cognitive science to accommodate linguistics […]", and thus they "are persuaded, on solid grounds we think, that in the past 50 years [generative] linguistics has progressively established itself as a genuinely scientific discipline" (Boeckx & Piattelli-Palmarini 2005: 447).

How should one assess these claims? What definition or methodology of science can one appeal to in order to argue for or against the scientific status of a theory of language? Lees hints at a key distinguishing factor that can identify good science: an axiomatic system and an overarching explanatory theory. He compares Chomsky's approach to studying language to the development of chemistry: It was only after Lavoisier's work in the late eighteenth century that chemistry developed from its beginnings in alchemy to a scientific discipline. Lavoisier's work allowed chemistry to achieve its scientific status by pushing the discipline to concern itself not so much with the correctness of its postulates — though that is of course essential — but with explanatory theory construction.

I take it that for a given approach to qualify as scientific it must possess an overarching explanatory theory and an accompanying axiomatic system. I will gloss over the details of what makes an approach scientific because, for the purposes of this article, both externalism and Chomskyan internalism can be said to have the form and methods of a scientific theory. I want to argue that externalism is not unscientific but rather bad science in the sense that it has chosen a subject matter that is not amendable to *fruitful* scientific theorising. This is so due to the creative aspect of language use. In other words, criteria for a given approach to qualify as scientific such as possessing an overarching explanatory theory, though necessary for qualifying as scientific, are not sufficient to distinguish a fecund and deeply explanatory science from one that is not. Chomskyan internalism proposes an explanatory theory, but, arguably, so does externalism: Putnam remarks that "a better philosophy and *a better science of language*" must encompass the "social dimension of cognition" and the "contribution of the environment, other people, and the world" to semantics (Putnam 1975: 193, my emphasis). Horwich (2001: 371) argues that Davidson's externalist truth-theoretic program "became widely accepted, instigating several decades of 'normal science' in semantics." Davidson himself is somewhat ambivalent, but still holds that "my own approach to the description, analysis (in a rough sense), and explanation of thought, language, and action has […] what I take to be some of the characteristics of a science" (Davidson 2004: 123). Burge (2003: 465) remarks that he sees no reason why formal semantics, which postulates "reference, or a technical analogue, as a relation between linguistic representations and real aspects of the world, should not be an area of fruitful systematic scientific investigation".

So apart from the construction of a self-consistent explanatory theory, which both externalism and internalism arguably have, what can distinguish the two in regard to their scientific fecundity? I propose that the distinguishing criterion should be the subject matter of their theories. It is not enough to have an

explanatorily self-consistent theory: Your theory must be aimed at a scientifically tractable aspect of the world. In other words, if your theory fails to divide nature at the joints, then no improvement of its methodology or its explanations will matter. To repeat, I want to suggest that externalists, when claiming to be doing science, are simply doing bad science — their research is aimed at a scientifically intractable aspect of the world. Observations of the creative aspect of language use imply that if one takes language use — or performance as opposed to competence — as the subject matter of one's theory, as externalists do, then such a theory is unlikely to yield a deeply explanatory science. Before I offer an argument for this, a few remarks of clarification are in order.

2.2. Internalism versus Individualism

Putnam constructs various thought experiments to argue for the externalist claim that the individuation of meanings is impossible if one only considers thinkers in isolation, and thus a semantic theory must consider the person's interaction with the environment and with other language users. The Twin Earth thought experiment is the most famous, but there are others that make the same point. One of which concerns the difference between an elm tree and a beech tree. Putnam claims to have the same concept for both elm trees and beech trees because, unlike botanists, he cannot tell them apart. But Putnam claims that 'elm' and 'beech' nevertheless have different meanings when he utters them. This is so even though, *ex hypothesi*, his mind-internal phenomena are identical whenever he utters 'elm' or 'beech'. Therefore, according to Putnam, considering the mind-external environment — the expert botanists, in this case — is the only way to discern the meaning of his utterance of 'elm' or 'beech'. He argues that one's "individual psychological state *certainly* does not fix its extension; it is only the sociolinguistic state of the collective linguistic body to which the speaker belongs that fixes the extension" (Putnam 1975: 146, emphasis in original).

It is hard to argue with such a claim; of course one can only discern what a person's utterance refers to by consulting the external environment. In order to determine the extension of Putnam's utterance of either 'elm' or 'beech' one must consult not only Putnam's mind-internal states and knowledge but also the knowledge of an expert who can distinguish between an elm and a beech, as well as the environment in which the utterance was produced. Be that as it may, however, the question arises as to the connection between such a search for individuation conditions and a science of language. That is, what is the connection, if any, between the search for the conditions under which one is justified in ascribing a particular meaning to an utterance, and a science of language that seeks to explain how linguistic utterances are produced and comprehended? I argue that studying the mechanisms in the mind by which meaning is made possible is one enterprise, the ascription of meaning to particular utterances another.[3]

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[3] Cf. Devitt (1984: 385): "[T]houghts are one thing, their ascription another [… it is a mistake for philosophers to] start with the theory of thought ascription, leaving the theory of thought pretty much to look after itself".

Millikan (2004b: 227) concurs when she says in regard to Putnam's argument that if "we explain the externalist idea in this crude way […] it becomes hard to see how anyone could deny it". That is, "[i]f the question were, merely, how are the referents or extensions of thoughts determined, it seems patently obvious that nothing inside someone's head could, by itself, determine that anything in particular existed outside the head". Millikan says that externalism so defined should not be so obviously true, but instead of turning against externalism she clings to it. But her remedy does not help and in fact complicates the matter further. Her externalist theory defines "inner representations by the way they function, not just in the head, but as parts of much larger systems that include portions of the environment" (p. 229). The functions of the inner representations, on Millikan's account, were selected by natural selection in the course of the organism interacting with its environment in a 'normal' way. Thus, it is "this reference to a certain kind of history of selection and/or development that adds the radically externalist twist to this theory of mental representation" (ibid.).

Millikan believes that mental representations can only be individuated by reference to their function, and thus she argues that we must adopt an externalist and evolutionary stance to individuation because "[w]hat a thing was designed to do is not always evident just from its inner function, even from its inner function plus the structure of its current environment" (ibid.; see also Millikan 1984, 1993, 2004a). She remarks that "whether an inner happening or structure is a representation is not merely a matter of its inner structure". But the question again arises as to whether this claim is relevant to scientific theories of meaning or mental representations that attempt to discover the mechanisms by which language production and comprehension are possible? Externalists claim that the criteria of the ascription of meaning or of function belong in a scientific theory of language, but I argue that this will not yield a fruitful science.

As a final remark, it should be noted that Chomskyan internalism is compatible with the view that the *individuation* of meanings is impossible without considering the environmental context of an utterance. If the aim of your theory is to discover the conditions under which an outside observer can make a correct judgement as to the meaning of a specific utterance (relative to the way the meaning is used within the linguistic community of the speaker), then of course such a theory must include within its domain the environment outside the head. But such a claim has little to do with a *scientific* theory of meaning. The externalist claim that it does follows from their glossing over an important distinction between the theory itself and the way in which the theorist *uses and interprets the theory* to achieve certain explanatory goals (cf. Egan 1995, 1999, 2003). This ambiguity is evident in remarks such as Ben-Menahem's (2005), who notes in regard to one of Putnam's examples that "to speak of coffee tables it does not suffice for us merely to have the concept of a coffee table, but we must be in contact with *actual* coffee tables" (p. 10, emphasis in original). In other words, there's an ambiguity between a theory that explains our ability to have the concept of, say, a coffee table, and a theory that purports to explain how it is that we use this concept to talk about actual coffee tables. Or, more generally, the difference is between a theory of the mechanisms in virtue of which language production and comprehension is made possible, and a theory of the *use* of those mechanisms in,

say, social interaction. When externalists claim that a science of language must encompass the social dimension of linguistic behaviour, it is not clear whether the claim is that this aspect of linguistic behaviour must be included within the scope of the theory itself, or whether this aspect can be connected to the theory by what Egan calls the theory's interpretation function. This distinction is important, for failure to adhere to it results in a defective explanatory theory.

3. Can Externalism Form the Basis of a Fruitful Science of Language?

Let us now look at an externalist theory of language in detail in order to assess whether it can form the basis of a fecund and explanatory scientific theory of language.

3.1. Horwich's Use-Theory of Meaning

Horwich (2005, 2008, 2010) claims that his use-based semantics is compatible with a linguistics construed as an empirical science. I give a brief sketch of his theory — by contrasting it with truth-theoretic semantics — and then argue that the reasons for doubting Horwich's scientific claims are the same as the reasons for rejecting externalist theories of meaning in general as candidates for fruitful scientific theories of language.

Horwich (2008) is a critique of mainstream formal semantics in which he argues that there is no reason to think that language has a truth theoretic basis. He correctly points out that while the problems truth-theoretic semantics presents "are highly challenging, requiring considerable skill and ingenuity, and that enormous progress has been made in these endeavours over the last forty years or so", citing such progress "is not enough to vindicate truth-theoretic semantics as an *empirical* subject, as an integral part of the global scientific enterprise" (p. 318, fn. 12, emphasis in original). He argues that in order to be scientific, truth-theoretic semanticists must show how their derivations have contributed to the explanation of observable events. However, "that has not, and cannot, be done" (ibid.).[4]

Horwich's main objection to truth-theoretic semantics has to do with compositionality and the assumption of formal semanticists that the focus of semantics should be sentence meanings. Davidson's truth-theoretic approach, for example, involves a compositional theory of meaning in which the meanings of sentences depend on the meanings of their constituent words. Horwich takes the opposite approach, for he believes that compositionality is relatively easy to accommodate and thus one needs to first identify the meanings of words and then "presupposing compositionality, to trivially deduce the theoretical-meanings of sentences" (ibid., 314).

[4] I think this is too strong a claim. Externalist truth-theoretic approaches to semantics that have as their subject matter language use are unlikely to yield a fruitful scientific theory of linguistic meanings. However, the *internalist* construal of truth-theoretic semantics has promising and illuminating results (cf. Hinzen 2006). I share Horwich's scepticism in regard to truth-theoretic semantics but I think that an internalist take on it has some value (cf. also Pietroski 2005, 2010).

Inverting the focus of semantics from sentences to words has the deflationary effect of nullifying truth-theoretic semantics because truth conditions apply to sentences and not to words. Given this focus on words, Horwich suggests that the theoretical characterisation of word meanings should be deduced not from sentence meaning but from sentence *usage.* And so his alternative is an externalist semantic theory that rejects truth conditions in favour of the claim that "the underlying basis of each word's meaning is the (idealized) law governing its usage—a law that dictates the 'acceptance conditions' of certain specified sentences containing it" (Horwich 2005: 26). This law of acceptance conditions purportedly solves the puzzle of why it is that, say, 'The sky is blue' tends to be recognised as true.

Horwich believes that the phenomena that semantics needs to explain are those of sentence acceptance. He elaborates: "I don't mean 'accepted as *grammatical*', but 'accepted as *true*', i.e. 'in the belief-box'." Moreover, acceptance "sometimes leads to *utterance* (depending on the speaker's desires); therefore explaining the acceptance of a sentence may contribute to explaining its being uttered" (Horwich 2008: 315, fn. 9, emphasis in original). According to Horwich, there are scientific laws that govern sentence acceptance. Given such laws, "it will be relatively easy to see how word-meanings, alongside other factors, will be capable of explaining what needs to be explained (namely, the acceptance-status of *all* sentences containing it)" (p. 318, emphasis in original). And so insofar as linguistics is an empirical science, says Horwich, "standing alongside psychology, neurology, biology, physics, etc.", such acceptance-laws "should be testable against concrete observable events" (p. 315). Thus, "the semanticist of a given language ought to be looking, concerning each word, for the basic law governing its use" (p. 319), and if such laws are forthcoming and explanatorily fruitful, Horwich believes that "[s]emantics would then somewhat resemble fundamental physics" (p. 318). In other words, the claim is that there are law-like regularities of word use, which are purportedly "characterised in non-semantic, non-normative terms" — that is, in naturalistic, scientific terms. These regularities are then used to derive facts about which rules of language use people implicitly follow. These regularities and rules, then, "suffice to fix what we *mean* by our words and hence sentences" (Horwich 2010: 113, emphasis in original).

3.2. Problems with Use-Theories of Meaning

Horwich writes that if "a semantic theory explains the phenomena of sentence-acceptance — and if it coheres with theories of phonology, syntax, and pragmatics to yield a science that explains all the phenomena of linguistic activity — then it is a good theory" (Horwich 2008: 319). He argues that truth-theoretic semantics cannot yield such a science but that his use-based semantics can. However, since both are externalist theories that claim to find scientifically tractable regularities in language production, and due to the creative aspect of language use, I argue that they cannot yield a fruitful and explanatory science of language.

As noted above, Horwich believes that "the underlying basis of each word's meaning is the (idealized) law governing its usage" (Horwich 2005: 26).

He claims that in order to make linguistics an empirical science semanticists need to look for the basic laws governing the use of words, but this assumes that there are scientifically interesting regularities in language use; and that is far from obvious. Moreover, the phenomenon of, say, a particular word's usage, is merely the effect of the internal psychological mechanisms of language. The regularities of language use, such as they are, do not explain anything but rather are *what needs to be explained*. Cummins (2000) talks of the 'scandal' of the widely held belief that scientific explanation is subsumption under law: "Laws tell us what the mind does, not how it does it. We want to know how the mind works, not just what it does" (p. 140). It is the capacity for language use that science seeks to explain, and laws of word use that Horwich postulates are at best the effects of this capacity. The laws describe the data to be explained, but the explanation itself involves the mechanisms in virtue of which language use is made possible. In fact, most scientific explanation in general follows what Thagard (2012) calls the *mechanista* view of scientific method, which holds that to explain a phenomenon is to describe a mechanism that produces it. Thus, in order to be an explanatory theory, use-based semantics needs not only laws of word use, the existence of which is tenuous at best, but also the mechanisms in virtue of which word use is made possible.

More specifically, sentence acceptance, a main tenet of Horwich's theory, is deeply problematic, and it is unclear whether it can be generalised beyond the examples that Horwich gives (cf. Schiffer 2000). But even if the notion of sentence acceptance can be spelled out, use theories of meaning, as Gupta (2003) remarks, rest "on an unacceptable identification: an identification of principles that are fundamental to an explanation of the acceptance of sentences with principles that are fundamental to meaning" (p. 654; cf. also Gupta 1993). That is, sentence acceptance may overlap to some extent with sentence meaning, but they are not the same thing. Gupta argues that there is little reason to think that explanatorily basic patterns of sentence acceptance in Horwich's theory can provide the meaning of a word. This is because "the acceptance of sentences depends not just on the meanings of words but also on the methods of obtaining information (and misinformation) about the world" (Gupta 2003*:* 666).

3.3. Problems with Externalist Theories in General

Whatever the details of use theories of meaning and their idiosyncratic difficulties, they are still externalist theories and thus face the same general problems as all externalist theories.

The fact that sentence acceptance depends not just on the meanings of words but also on the methods of obtaining information about the world hints at the main reason for the inability of externalist theories such as Horwich's to serve as fruitful scientific theories of language: The problem is the subject matter and scope of the theories. The reason is the same reason given by Katz & Fodor (1963: 179) fifty years ago. They ask the reader to compare the following three sentences: *Should we take junior back to the zoo? Should we take the lion back to the zoo? Should we take the bus back to the zoo?* They then remark that, for example, "[i]nformation which figures in the choice of the correct readings for these sentences

includes the fact that lions, but not children and busses, are often kept in cages". After listing a handful of other examples, they note that the "reader will find it an easy matter to construct an ambiguous sentence whose resolution requires the representation of practically any item of information about the world he chooses". Thus, a linguistic theory that takes it upon itself to resolve such ambiguities clearly must include within its scope every feature of the world that speakers may need in order to arrive at the correct reading of an ambiguous utterance. But practically *any* piece of information about the world is potentially relevant. Further problems arise when theorists investigate the truth of an utterance in relation to the mind-external world.

A theory that includes language use and the mind's relations to the world within its explanatory scope cannot hope to find reliable relations of this sort — let alone systematise them into a fruitful explanatory scientific theory. This is due to the creative aspect of language use: If language use is indeed uncaused in the above sense, but is at the same time coherent and appropriate to the circumstances at hand, then there will be no scientifically interesting regularities of the sort Horwich and other externalists claim to exist. This is in addition to the fact that even if there were such regularities, they would merely be a rewording of the phenomena to be explained.

One can of course still ask *why* we should not hope for a serious science of phenomena that are uncaused yet appropriate to circumstances. The relevant science does not exist at present, it can be objected, but this does not prove that such a science is impossible.[5] The reason we should not hope for a serious science of these phenomena is not so much, as Chomsky at times avers (e.g., Chomsky 1988a: 35–36, 2000a: 145), that it may be beyond our cognitive reach. Rather, the reason is that explanatory science deals with mechanisms, not with laws of use (in this case explanatory science deals with the mechanisms in virtue of which language use is made possible, not with language use itself). Laws tell us what the mind does, not how it does it, and it is the latter that an explanatory science seeks to illuminate. I do not want to imply that an externalist science of linguistic meaning is impossible in principle. Externalism is not unscientific but rather bad science in the sense that it has chosen a subject matter for itself that is not amendable to *fruitful* scientific theorising.

Another problem is that meaning is defined in externalist theories in a way that makes them unable to distinguish between the speaker's linguistic knowledge and their world knowledge. In Putnam's example of elms and beeches, the theorist must consult not only the mind-internal mechanisms of the speaker but also their, and other speakers', world knowledge. To *really* know whether Putnam's utterance means 'elm' or 'beech' the theorist must, according to externalism, (1) consult Putnam's linguistic knowledge, (2) his world knowledge about elms and beeches (and whether he can tell them apart), and (3) the world knowledge of other speakers (the expert botanists who can tell the difference between elms and beeches). Clearly, then, externalists demand that a theory of linguistic meaning include within its scope not only the internal linguistic

[5] I thank an anonymous reviewer of an earlier draft of this article for bringing this potential objection to my attention.

mechanisms of the speaker, but also the world knowledge of the speaker and the relation that holds between the speaker's utterance and the world. But if all of the aforementioned must be included in the same theory, then externalism cannot *in principle* distinguish between linguistic knowledge and world knowledge (cf. Haiman 1980).

In other words, a linguistic ability is couched by externalists in terms of representations of all the knowledge about the world that speakers share. However, as Katz & Fodor remark, "since there is no serious possibility of systematizing all the knowledge of the world that speakers share, and since a theory of the kind we have been discussing requires such a systematization, it is ipso facto not a serious model for semantics" (Katz & Fodor 1963: 179).[6] The same holds for all externalist theories of meaning: They are not a serious model for *scientific* theories of meaning because their subject matter is too wide in scope. That is, if the creative aspect of language use is the subject matter of your theories, and if Descartes was right to point out the uncaused yet appropriate nature of language use, then externalist theories of language use will not yield a fruitful and explanatory science. As outlined in the next section, however, a scientific theory of the mechanisms that *underlie* language use *is* possible.

4. The Internalist Explanation of the Creative Aspect of Language Use

I argue that the Chomskyan internalist approach to linguistic science avoids the pitfalls of externalist theories of language and thus provides a promising candidate for an explanatory and fecund linguistic science. Externalism construes linguistic meanings in a way that makes construction of a fruitful science of them unlikely. Internalist linguistic meanings, on the other hand, are in a form that is amendable to fruitful explanatory science.

The subject matter of generative linguistics is taken to be linguistic competence, the speaker-hearer's knowledge of their language, as opposed to linguistic performance, which is the actual use of this knowledge in language production and comprehension. This distinction forms the foundation of generative linguistics and Chomskyan internalism. The actual use of the knowledge of one's language involves many other factors and phenomena, only one of which is one's competence. It is only under strict idealisation conditions that performance might be seen as reflecting competence, and the actual causal sequence that brings about a speech act is not directly related to competence.

Another distinction is that between I-language and E-language (Chomsky 1986). Externalised (E-)language refers to actual speech events, with some account of their context of use. From the E-language point of view, then, a grammar is a collection of statements that describe linguistic performance. Moreover, on this account there need not be one 'real' or 'correct' grammar that corresponds to the corpus data: As long as it yields a correct description of the

[6] It is worth noting that, as I have argued elsewhere, Fodor appears to have changed his mind about what a serious model of semantics entails. Since at least the 1980s he has argued in favour of an *externalist* semantics. Cf. Asoulin (2012).

corpus data, any number of grammars could in principle apply.[7] David Lewis, for example, says that he can find no way to "make objective sense of the assertion that a grammar G is used by population P whereas another grammar G' which generates the same language as G, is not" (Lewis 1975: 177). He believes that a language is an abstract, formal system that a population selects by convention (cf. Lewis 1969). Similarly, Dretske (1997) claims that "everything we in fact call a language, at least a natural language, is the product of social factors" (p. 289). Another manifestation of E-language can be seen in Devitt & Sterelny (1989), who argue that rather than being about competence, linguistics is about the properties and relations of observable, external, linguistic symbols (cf. Devitt 2006).

According to the E-language conception, then, language is, as it were, 'out there', it is not intimately related to the mind. A case in point is Deacon (1997), who is critical of the Chomskyan approach to studying language acquisition, and says:

> They [Chomskyans] assert that the source for prior support for language acquisition must originate from *inside* the brain, on the unstated assumption that there is no other possible source. But there is another alternative: that the extra support for language learning is vested neither in the brain of the child nor in the brains of parents or teachers, *but outside brains, in language itself.* (Deacon 1997: 105, emphasis in original, my emphasis[8])

On the internalised (I-)language perspective, however, language is conceived as being intimately related with the mind in that there is some structure in the mind of the speaker/hearer that is responsible for their language. So, unlike the E-language conception of grammar, the grammar qua I-language is a theory of a real mental structure to which "questions of truth and falsity arise […] as they do for any scientific theory" (Chomsky 1986: 22). An I-language is a generative procedure in the mind of a speaker/hearer that creates a structural description that combines phonetic, semantic, and structural properties.

The Chomskyan internalist claim is that the proper subject matter of a scientific linguistics should be the knowledge a speaker/hearer has of their language, the knowledge (a structure in the mind/brain) that underlies and makes possible, along with other factors, the speaker/hearer's language production and comprehension.

4.1. Semantics and Chomskyan Internalism

In the Chomskyan internalist approach to semantics, the language faculty derives an expression *Exp* by assembling features from the array of lexical items and mapping them to the *Phon* and *Sem* representations (i.e. *Exp* = <*Phon*, *Sem*>). The semantic features of an expression (*Sem*) are mental instructions that interface with, and thus give information to, the conceptual-intentional systems. *Sem* is the

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[7] Cf. Quine (1972) and Lewis (1975), both of whom Chomsky (1986) cites as indicative of the E-language approach. For other E-language approaches, see, for example, Wallace (1977), Devitt & Sterelny (1987, 1989), and Devitt (2006). See also Millikan (2003) and Chomsky (2003).

[8] Cf. Chomsky (2000: 22) for discussion of Deacon's view.

interface between the language faculty and the systems of thought. This approach mirrors the approach to phonology in which phonetic features of an expression (*Phon*) are mental instructions that interface with, and thus give information to, the sensorimotor systems. The arrays of semantic features that are part of *Sem* are, as many have repeatedly noted, much more complex and difficult to investigate than the phonological representations. Nevertheless, valuable and fruitful progress has been made in regard to semantic features.

Pietroski (2006) compares linguistic meanings in Chomskyan internalism to blueprints, which are produced by the language faculty for constructing concepts from lexicalised elements. At a higher level is the I-language, which is a biologically-instantiated procedure that pairs phonological instructions with semantic instructions; *other systems* then execute these instructions. *Sems* are thus not to be thought of as concepts, for construing them as concepts "may be a category mistake, like evaluating *an instruction to fetch* a rabbit as male or female" (Pietroski 2010: 252, emphasis in original). In other words, what we have are *instructions to build concepts*, which provide the inputs to other systems that enter into various human actions, one of which is communication. Chomskyan internalist semantics, then, concerns the nature of the instructions given by the language faculty to the systems of thought; it concerns *not* the concepts themselves but *the instructions to fetch, build, and combine concepts*. In other words, it concerns the mechanisms of concept creation (cf. Pietroski 2008).

This is of course one step removed from what externalist semantics studies, which is the concepts themselves, their role in language use, their relation to the speaker's environment, and their truth values. As Pietroski remarks, the work of a Chomskyan internalist "will take the form of saying how meaningful I-expressions can be used to build concepts that are *inputs to* a more complex process of building concepts that we can use to make truth-evaluable judgements" (Pietroski 2010: 272, emphasis in original).

Externalist theories that include within their scope the relation of, say, concepts to the world, run into overwhelming problems, some of which I discussed above. Whereas Chomskyan internalist theories study the mechanisms in virtue of which concept construction and language use is made possible: These are expressions produced by an internal linguistic engine whose components have no direct relation to the outside world. The *Sem* features are used to construct concepts that are then used by other systems to make truth-evaluable assertions, or communicate an idea, or any number of uses to which language can be put.

The current abstract form of the *Sem* features will of course be refined until the theoretical vocabulary of a serious science of meaning emerges. But they are a good starting point, for they help recast the notion of linguistic meaning into a form that is susceptible to fruitful scientific investigation. The instructions at the *Sem* interface that are interpreted by the performance systems are used in the act of talking and thinking about the world. And so, on this view of meaning, the instructions to create concepts play the role of "focus[ing] attention on selected aspects of the world as it is taken to be by other cognitive systems, and provide intricate and highly specialized perspectives from which to view them, crucially involving human interests and concerns even in the simplest cases" (Chomsky 2000: 125).

In summary, then, Chomskyan internalism postulates a mind/brain-internal generative procedure — an I-language — that generates expressions of the form *Exp* = <*Phon*, *Sem*>. This expression (via the *Phon* and *Sem* interfaces) is then used by systems outside of the language faculty (but internal to the mind/brain) in language production and comprehension. Chomskyan internalism argues that what is relevant to and tractable by a scientific theory of language are the mechanisms operating within the mind/brain, thus avoiding the problematic aspects of externalist theories discussed above. This of course does not mean that the mind is completely detached from the outside environment (it's not), nor does it mean that one must individuate meanings by making use of only individualistic or organism-internal vocabulary — for there is the distinction between the computational theory itself and its interpretation by the theorist. Rather, the upshot of Chomskyan internalism is that whatever connection the mind has with the out-side world, that connection is unlikely to be within the scope of a fruitful scientific theory of language.

5. Concluding Remarks

The argument against externalist theories of language qua fruitful scientific theories that appeals to the creative aspect of language use is as follows: Since language use allows for an unbounded expression of thought and is independent of direct stimulus control but at the same time it is appropriate to new situations and is coherent in new contexts, a fruitful externalist scientific theory of language use is unlikely. People can produce and comprehend an infinite number of novel utterances, and it is problematic at best to try to account for their linguistic behaviour directly: No scientifically interesting lawful correlations or predictions of potential linguistic behaviour will be found.

This of course does not mean that the *mechanisms that make language use possible* cannot be studied, but it does mean that the creative aspect of language use will perhaps remain, as Chomsky puts it, not merely a problem but a mystery (cf., among many others, Chomsky 1982: 429). Though one possibility of dispelling the mystery, still as remote today to pursue seriously as it was when Descartes suggested it, is to postulate a 'thinking substance', a new aspect of mind. As Bracken (1970a) explains, the Cartesians saw no way of extending their physical explanations to cover mental phenomena, and so it was suggested that a new principle, the 'creative' principle, must be added to the vocabulary of science. This is on the analogy of the postulation of the then new principle of gravity: The occult qualities of gravity were methodologically objectionable to both the Cartesians and to Newton but they accepted it "largely because the powerful mathematical model Newton employed carried against all a priori objections" (ibid., 237).

The explanatory success of theories of mind is of course far smaller than that of Newtown's theory of gravity, but it is worth remembering that even Newton regarded the postulation of gravity as "inconceivable" and "so great an absurdity that […] no man who has in philosophical matters any competent faculty of thinking can ever fall into it" (quoted in Chomsky 1993: 38). But

scientists were eventually forced to accept it due to its mathematical and explanatory power, for gravity gave an account of the essence of matter. The Cartesians, especially as their ideas developed with the Port-Royal tradition, attempted to do the same to the mind. That is, "in grammar we can derive an account of the essence of mind parallel to the account which geometry gives us of the essence of matter" (Bracken 1983: 22). The Cartesians had no model by which to explain the essence of mind that was equivalent to Newton's postulation of gravity as the essence of matter, and that is what the Port-Royal tradition attempted to provide. Today, Chomsky sees generative linguistics as reviving the Port-Royal efforts to provide a mathematical model of the mind that would take some steps towards an account of the essence of mind, but now with a more restricted subject matter and armed with modern mathematical tools such as those provided by Alan Turing and others (cf. Bracken 1970a, 1970b, 1983).

To recap, then, externalist theories of language are concerned with normative and epistemic notions such as truth and reference, and these notions are clearly aspects of language use. But if Descartes and Chomsky are right to argue that the creative aspect of language use is now — and perhaps to remain — beyond the scope of scientific explanations then an externalist theory of language that is an explanatorily fruitful scientific theory is unlikely. As McGilvray (2005: 204) puts it: "Because people use words for all sorts of purposes, because the use of language is a form of free action, and because there is little reason to think that there can be a science of free action, there is little reason to think that there can be a naturalistic externalist theory of meaning".

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Biolinguistics or Physicolinguistics? Is the Third Factor Helpful or Harmful in Explaining Language?

Sverker Johansson

Noam Chomsky (2005) proposed that a 'third factor', consisting of general principles and natural laws, may explain core properties of language in a principled manner, minimizing the need for either genetic endowment or experience. But the focus on third-factor patterns in much recent biolinguistic work is misguided for several reasons: First, 'the' third factor is a vague and disparate collection of unrelated components, useless as an analytical tool. Second, the vagueness of the third factor, together with the desire for principled explanations, too often leads to sweeping claims, such as syntax "coming for free, directly from physics", that are unwarranted without a case-by-case causal analysis. Third, attention is diverted away from a proper causal analysis of language as a biological feature. The point with biolinguistics is to acknowledge the language faculty as a biological feature. The best way forward towards an understanding of language is to take the biology connection seriously, instead of dabbling with physics.

Keywords: causal analysis; Fibonacci; natural law; physics; third factor

1. Explaining Language — Principled and Causal Explanations

Chomsky (2005) identifies three separate factors that can jointly explain the language faculty in the human brain:

(1) Genetic endowment, the "universal grammar" (UG).
(2) Experience, the stimulus available to the language learners.
(3) The 'third factor', principles not specific to the faculty of language.

Helpful comments from Rie Asano and two anonymous reviewers are gratefully acknowledged. The current structure of the paper is largely due to constructive suggestions from one of the reviewers. Early versions of this work have been presented at *Language, Culture and Mind V* and *19th International Congress of Linguists*; thanks to a number of conference attendees for insightful questions and comments. Any remaining lack of logic and clarity is of course my own responsibility.

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But what does it mean to explain language? What kind of understanding should we aim for, and how does this three-way split help us? Generative linguists have long distinguished three levels of theoretical goals in linguistics (Chomsky 1965):

- 'observational adequacy', that a theory describes language usage.[1]
- 'descriptive adequacy', that a theory accounts for the phenomena observed in adult language competence.
- 'explanatory adequacy', that a theory accounts for how children can acquire adult language competence.

The minimalist program entails a desire to move "beyond explanatory adequacy" (Chomsky 2004), adding a new level of theoretical goals, explaining not just what language is like and how it can be acquired, but also explaining in a principled way why it is that way. Chomsky (2007b) associates 'what' questions with descriptive adequacy, 'how' with explanatory, and 'why' with going beyond explanatory. Chomsky (2007b, 2010) calls an account of language "principled" if it goes beyond explanatory adequacy, grounding features of language in general non-linguistic principles, notably principles of efficient computation, which belong to the third factor.

Fujita (2007, 2009) calls the new level "evolutionary adequacy", a requirement that a theory accounts for how our faculty of language could emerge during human evolution, and Narita (2010) uses the term "biological adequacy" in much the same sense. Also Jenkins (2006) indicates 'How does language evolve in the species?' as a question to be asked in biolinguistics, in addition to the 'what' and 'how' questions underlying descriptive and explanatory adequacy. In an evolutionary context, such a theoretical goal makes sense, and Jenkins (2000) draws an explicit analogy between a theory of language acquisition providing an explanatorily adequate account of language competence, and a theory of language evolution providing an explanatorily adequate account of language acquisition.

'Evolutionary adequacy' also fits in well with the general distinction in biology between proximate and ultimate explanations (Mayr 1961), further developed by Tinbergen (1963) in his well-known four 'why' questions (adaptation, history, proximate cause, and ontogeny), to which we will return below. Descriptive and explanatory adequacy clearly deal with proximate explanations of language, whereas the level of evolutionary adequacy would be about ultimate explanations, *sensu* Mayr.

But Chomsky's (2005, 2010) quest for a new level appears to be heading in a different direction, seeking to explain language in terms of fundamental principles, rather than in terms of either adaptation or evolutionary history. Chomsky (2007b) explicitly contrasts the question of language evolution with the 'why' question beyond explanatory adequacy. Jenkins (2006) likewise regards the 'why' of language as a question beyond just evolutionary origins. The third factor is given a key role in this quest, to the extent that a principled explanation is even defined as one that is based on the third factor (Chomsky 2008).

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[1] Observational adequacy is included in early Chomsky (e.g., 1965), but is typically absent from more recent literature (e.g., Chomsky 2007b), where the adequacy levels start with descriptive adequacy.

2. What Is the Third Factor?

The third factor can be defined as everything that is part of the explanation of language, but is not language-specific. Looking at the examples and descriptions of the third factor in the literature, it turns out to be a rather heterogeneous collection of component factors, including several types of general theoretical principles, but also biological and human-specific factors like developmental constraints and canalization in our embryology. The components of the third factor are divided by Chomsky (2005) into two classes:

- principles of data analysis
- architectural, computational, and developmental constraints

Physical and mathematical principles ("laws of form") are also included in the third factor, according to several authors (Chomsky 2004, Carstairs-McCarthy 2007, Piattelli-Palmarini & Uriagereka 2008, Di Sciullo *et al.* 2010), though they do not fit neatly into either one of the two classes above. Furthermore, it is clear from Chomsky (2005) that the interface conditions imposed by the sensorimotor (SM) and conceptual-intentional (C-I) interfaces are regarded as part of the third factor as well.

Compiling all the various suggestions for third-factor components that I find in the literature, I end up with this list (with likely but unclear overlaps between the different points):

- principles of data processing and analysis
- economy of derivation
- interface conditions
- performance systems
- general cognitive capacities, general learning strategies
- architectural and computational constraints
- developmental constraints and canalization in embryology
- physical law
- mathematical principles, e.g., symmetry
- mathematical patterns, e.g., Fibonacci series
- laws of form (*sensu* Thompson 1917)

There is no clear consensus on what is and is not included in the third factor. Its vague, negative definition makes it difficult to exclude anything. As noted by one anonymous reviewer, even adaptation through natural selection would count as part of the third factor, as every non-linguistic principle affecting language is by definition included. But others, such as Medeiros (2008), explicitly contrast third-factor explanations with "adaptationist accounts" (2008: 188), and reject the latter.

Chomsky (2007b: 15) regards the third factor as closely (causally?) connected with the conjectured optimality and perfection in language: "Insofar as third-factor properties function, language will satisfy these [interface] conditions in an optimal way, meeting conditions of efficient computation". In Chomsky (2011), optimality and principles of efficient computation are apparently equated with natural law:

> We can regard an account of some linguistic phenomena as principled insofar as it derives them by efficient computation satisfying interface conditions. A very strong proposal, called "the strong minimalist thesis", is that all phenomena of language have a principled account in this sense, that language is a perfect solution to interface conditions, the conditions it must satisfy to some extent if it is to be usable at all. If that thesis were true, language would be something like a snowflake, taking the form it does by virtue of natural law, in which case UG would be very limited.
>
> (Chomsky 2011: 26)

But natural laws are only a small part of all the different things going into the third factor.What the various component factors that make up the third factor have in common are really just two things:

- They are not language-specific.
- They are believed to play some explanatory role behind language.

Apart from these two points, there are no obvious commonalities that unite them and motivate the bundling of them into a single factor. Even the second point above is really not a commonality at all, until it is shown that there is actually some substance behind the faith in their explanatory power.

A more useful classification of these factors might be according to their origin and epistemological status:

(A) Some factors have an aprioristic character, notably mathematical laws and some abstract computational principles.

(B) Others are empirical but not biological, like physical laws.

(C) Others are distinctly biological, and are contingencies of our evolutionary history, placing them in the same ontological category as our genetic endowment. The developmental constraints belong here. On the issue of developmental constraints, Chomsky (2007a, 2010) relies on the general developments in biology called 'evo-devo' (Carroll 2006, Benítez-Burraco & Longa 2010). The general developmental processes behind our nervous system are shared with many animals, and are thus biological but not human-specific. Computational constraints that directly relate to the neural implementation of computations may also belong here.

(D) Yet others are human-specific. Some details of the developmental constraints in our embryology are human-specific, as are some architectural constraints. The interface conditions can be included here as well, as they depend on the SM and C-I systems, of which at least some parts can be assumed to be human-specific, as can some parts of our general cognitive capacities.

Another way to classify the third-factor components might be according to how they may influence the human language faculty, and what causal powers they have. But also in that respect, it is obvious that the various component factors are a highly disparate collection. I will return to the causal analysis in more detail in section 3 below.

As the third factor is intimately connected with Chomsky's quest for principled explanations, it is also relevant to note that only a subset of the proposed third-factor components can by any stretch of the imagination be called "principled". Quirks of primate embryology or the human vocal tract do not lend themselves to principled explanations of anything. Even in the fundamental minimalist picture of syntax as an optimal bridge between the C-I and SM interfaces (e.g., Chomsky 2008), posited as explainable in a principled way, the interfaces themselves are beyond principled explanation (Narita 2009).

Chomsky (2008) recognizes that the third factor is still unfinished, a work in progress, and that it is a matter of empirical inquiry which of its components are actually relevant to language:[2]

> It is hardly necessary to add that the conditions that enter into principled explanation […] are only partially understood: we have to learn about the conditions that set the problem in the course of trying to solve it. The research task is interactive: to clarify the nature of the interfaces and optimal computational principles through investigation of how language satisfies the conditions they impose – optimally, insofar as SMT holds. This familiar feature of empirical inquiry has long been taken for granted in the study of the sensorimotor interface (SM). Inquiry into acoustic and articulatory phonetics takes cues from what has been learned about phonological features and other such properties in I-language research and seeks SM correlates, and any discoveries then feed back to refine I-language inquiry. The same should hold, no less uncontroversially, at the semantic/ conceptual-intentional interface (C-I). And it should also hold for third factor properties. (Chomsky 2008: 135–136)

In general, Chomsky is quite careful in print[3] to note that both the SMT and the third factor as an explanation for language are mere conjectures. But some biolinguists, for example Piattelli-Palmarini (2012), are less prudent and make sweeping claims for the powers of the third factor, an issue to which I will return in section 4 below.

But even as a conjecture, how useful is the third factor? Does it make sense to talk about *the* third factor, when there is nothing uniting the components except the negative feature that they are not language-specific, and when there is not even any clear agreement on which components should be included? Such a pseudo-concept is unlikely to contribute to our understanding of language.[4] In section 4, we will look closer at the results of this lack of clarity and coherence.

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2 Thanks to an anonymous reviewer for drawing my attention to this statement by Chomsky.

3 When giving talks, Chomsky can be less circumspect at times. In print: "If that thesis [that all phenomena of language have a principled account] *were* true, language *would be* something like a snowflake, taking the form it does by virtue of natural law […]" (Chomsky 2011: 26). In his plenary talk at the 19th International Congress of Linguists in Geneva (July 2013): "Language *forms* like a snowflake, in the simplest possible way" (emphasis added in both).

4 One anonymous reviewer interpreted my criticism as merely an objection to the expression "the third factor", and argued: "Change 'the third factor' to 'third factor principles' and most of the argument falls apart, because people use 'the third factor' and 'third factor principles' interchangeably all the time". But I do not criticize just the expression, my point is that it is unwarranted and potentially misleading to invoke third-factor considerations in linguistic arguments as if there were a coherent third-factor concept that in itself explained anything. Using singular and plural expressions interchangeably is not a solution to this issue; instead it is a symptom of the problem.

The incoherence of 'the' third factor does not, however, imply that its components are all irrelevant for language. It is eminently possible, and in some cases even highly likely, that several third-factor components can contribute to our understanding of language. But if so, their contributions are likely to be as disparate as the components themselves. Some may act as constraints on what is possible, either in principle or in the specific case of humans. Others may form parts of the support system of language (FLB *sensu* Hauser, Chomsky, & Fitch 2002). Yet others may provide the basis for adaptive advantage. And conceivably some may provide explanations that are both causal and principled. But they contribute as separate components, not because they belong to 'the' third factor, and their contributions must be evaluated on a case-by-case basis.

3. Causal Analysis and Explanations in Biology

In a biolinguistic perspective, language is a feature of human biology (Boeckx & Grohmann 2007), and it would appear natural to model an explanation of the causes of language on the type of causal analyses employed in the study of other biological features. In general, the explanation for any feature in an organism needs to answer the four 'why' questions of Tinbergen (1963) mentioned above.

It must be emphasized that the four questions of Tinbergen are *complementary*, not competing explanations. A full understanding of any biological feature requires answers to all of them. The answers to the different questions are typically quite distinct, both in content and conceptually, with each answer providing only a partial explanation of a feature:

(1) *Adaptation*? The focus here is on why the feature evolved, what made it spread in the population. This is the question to which it is usually most difficult to give a stringent answer. An answer typically starts with a functional analysis of the feature, what it is *for*, and proceeds from there to an analysis of the causal chain from its function to how it could spread in the population through natural selection and/or other evolutionary processes. Demonstrating causation in evolutionary processes is a non-trivial matter, which biologists may handle with experimental (e.g., Sinervo *et al.* 1992) or statistical (e.g., Shipley 2000, Lomolino *et al.* 2012) techniques.

Example: Sickle-cell anemia is present in some human populations *because* heterozygotes are resistant to malaria, and if malaria is common this produces enough of a selective advantage to make the sickle-cell gene spread in the population up to an equilibrium level where the homozygote disadvantage balances the heterozygote advantage.

(2) *History?* This is a matter of determining how the feature emerged over evolutionary time, what it evolved from, and what intermediate stages it went through. Answering it is largely a matter of mapping character changes onto a phylogenetic tree, with comparative anatomical, genetic, embryological and fossil evidence being used.

Example: Sickle-cell anemia is present in some human populations *because* a point mutation occurred in the β-globin gene some thousands of years ago, in a population exposed to malaria.

(3) *Proximate cause?* This is typically the most reductionist answer, explaining a feature in terms of the underlying mechanisms.

Example: Sickle-cell anemia is present in some human populations *because* the mutated β-globin gene codes for a valine at position 6, creating a hydrophobic patch in the protein that under low oxygen conditions causes hemoglobin S molecules to aggregate and form fibrous precipitates.

(4) *Ontogeny*? How does the feature emerge during ontogeny, from what combination of genetic, epigenetic and environmental factors?

Example: Sickle-cell anemia is present in some human populations *because* when the activity of the mutated β-globin gene is triggered after birth, it starts producing the mutated form of hemoglobin.

Full causal explanations, with adequate coverage of all four questions, are not very common in biology, as the analysis is very difficult and laborious for any non-trivial feature, and the historical information may simply be unavailable for features that do not fossilize. But in some simple cases, like the sickle-cell anemia used as an example above, we do understand it well enough. And even for less tractable features, including language (Jenkins 2011), Tinbergen's questions provide the appropriate framework for pushing the analysis as far as it can be done.[5] For another example of a full Tinbergen analysis, somewhat closer to language than sickle-cell anemia, see the analysis of Zeifman (2001) of infant crying.

Tinbergen proposed his questions 50 years ago, and much has happened in biology since then. We have today a much better and more sophisticated understanding of genetics, development and evolution, than we did in 1963. Tinbergen's basic analysis of the problem remains sound, but the questions need to be refined, taking into account e.g. developmental constraints as aspects of both history and ontogeny, and considering non-adaptive evolutionary processes together with adaptive explanations.

It can be noted that this kind of causal analysis does not directly involve any quest for principled explanations. Past attempts to find principled explanations for non-trivial biological features have met with very limited success, and the mainstream consensus in biology assigns a much larger role to the tinkering of Jacob (1977) than to the laws of form of Thompson (1917).[6]

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[5] One anonymous reviewer argues that such an analysis is hopeless, invoking in support a statement from Chomsky in 1980: "We can, post hoc, offer an account as to how [the] development [of an organism] might have taken place, but we cannot provide a theory to select the actual line of development, rejecting others that appear to be no less consistent with the principles that have been advanced concerning the evolution of organisms" (Chomsky 1980: 36). However, the opinion of Chomsky 33 years ago on evolution, a topic on which he is far from an expert and which has changed dramatically since 1980, carried little weight then and less weight today. See, for example, Berwick (2011) and Jenkins (2011) for recent analyses from a biolinguistic perspective that do not reject the problem as hopelessly intractable.

[6] Boeckx (2011) discusses from a favorable perspective the few biologists who disagree, while conceding that the majority agrees with my position.

3.1. Causal Analysis in Biolinguistics

The Poverty of the Stimulus, the insufficiency of the environmental input, is axiomatic in biolinguistics, which means that the second factor of Chomsky (2005) cannot be a major part of any explanation of language. In early versions of generative grammar, the genetic endowment was regarded as the main ontogenetic explanation for our language capacity. The language acquisition device, presumably genetically encoded, was believed to be rich and highly complex, the evolution of which would be very difficult to explain (Chomsky 2007b). The Principles and Parameters program eased this difficulty (Chomsky 2007a, Boeckx & Piattelli-Palmarini 2005); the main focus remained on the genetic endowment, but much less complexity and structure was required. In the minimalist program, however, the main explanatory burden is explicitly shifted to the third factor, with a minimal genetic endowment hypothesized (Chomsky 2005). This also entails a shift away from language-specific to non-specific explanations (Benítez-Burraco & Longa 2010, Di Sciullo *et al.* 2010), as well as a shift in explanatory level, as discussed in section 1 above. All three factors remain part of the equation,[7] but it is clear that the main thrust of recent work by Chomsky and others is in the direction of minimizing the contribution of the first two factors, with the third factor expected to do the lion's share of the causal and explanatory work.

This shift in explanatory emphasis makes it vital to understand the third factor, and how it can and cannot be used in a causal analysis of language. But as noted in the previous section, 'the' third factor is really a disparate collection of unrelated factors, the contributions of which to language are likely to be quite distinct from each other. Any causal analysis involving 'the' third factor must treat the various component factors separately, each on its own terms.

But any explicit causal analysis of language in third-factor terms is effectively absent from the biolinguistic literature; the causal power of 'the' third factor is too often simply taken for granted (see section 4.1), and there is rarely any systematic consideration of the different types of biological causes summarized in Tinbergen's four questions, despite the explicit appeal to these questions in the founding issue of *Biolinguistics* (Boeckx & Grohmann 2007), as well as in other biolinguistic work, from Chomsky (1976) to Jenkins (2011). Third-factor arguments typically skip most of the questions, leaving too much unexplained.

One may attempt to map the various components of 'the' third factor onto the Tinbergen questions. Arguments based on developmental constraints provide partial answers to the history question. Arguments based on efficiency and optimality can provide indirect answers to the adaptation question, as efficiency can sometimes explain why a certain feature has high fitness, but this needs to be made explicit. Arguments based on natural law can either likewise provide a background to the adaptation question, as with bird wings, or they can provide a proximate cause, as with cell shape (see section 4.3 below); but natural laws rarely provide both, and do not on their own explain biologically useful complexity (Mayr 2004).

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[7] As noted by Rie Asano (p.c.), with only the non-specific third factor we could not explain the *differences* between language and, for example, music.

Epstein (2007) draws an interesting parallel between the three factors of Chomsky (2005) and the "dual causation" of Mayr (2004).[8] The first of Mayr's two causes here consists of a genetic program that may be affected by environmental input, and thus maps onto Chomsky's first two factors, whereas Mayr's second cause is natural law, making it the equivalent of a subset of Chomsky's third factor. But Mayr (2004) regards the role and effect of natural law as quite limited, in contrast with the major explanatory burden with which Chomsky (2005) endows the third factor. Mayr distinguishes *teleomatic* processes coming from natural law, which are limited and without goal, from the seemingly purposeful *teleonomic* processes typical of biological systems.

Physical laws have inescapable causal powers that can never be ignored, and can often be a proximate cause in biological systems, with the ultimate biological causes acting to set up the situation so that the physical causes do something biologically appropriate — see section 4 below. But more often physical laws function either as constraints or in shaping selective pressures. Jenkins (2000: 159) quotes Chomsky as calling it the "space of physical possibility within which selection operates", which is a nice way of catching the relationship between physics as providing constraints, and biology as operating within, and sometimes exploiting, those constraints. But the constraints themselves do not provide any biologically useful complexity for free. As Mayr (2004: 50) puts it: "[T]he very general terminal situations effected by natural laws are something entirely different from the highly specific goals coded in programs".

And even in cases where the proximate causes of biological phenomena are physical, it does not follow that the other three questions of Tinbergen (1963) can be answered by appeals to physical law, nor can they be ignored. It goes without saying that biology cannot violate physical laws — but this is a far cry from saying that physical laws *explain* biological features in any interesting sense (Mayr 2004). The burden of proof rests squarely on anybody proposing that language is an exception in this regard, a complex biological feature that physical principles *can* explain.

Principles of efficient computation and the like, in contrast with physics, do not have causal powers *per se*, and do not drive even teleomatic processes on their own. They necessarily need to work through regular biological pathways. One may argue that our neural wiring, or faculty of language, is the way it is *because* that way is optimal in some sense. But the word 'because' here does not describe any direct causation by principles of optimality; it should be taken as shorthand for a long causal chain, going through all four of Tinbergen's questions:

In order for a neural system to be, say, computationally efficient, we need (1) a mechanism that can be the proximate cause making nerves connect in a way that provides computational efficiency; we need (2) a developmental system that provides an ontogeny in which the proximate cause can do its work; we need (3) an evolutionary history which can lead up to the appropriate mechanism and ontogeny; and we need (4) a selective environment in which efficient compu-

[8] Note that this is quite different from Mayr's (1961) distinction between proximate and ultimate causes.

tation is favored in a way that makes it spread in the population. This does not mean that we need the path of every nerve to be genetically specified; there are certainly much cleverer ways to do things in the interplay between genetic and epigenetic processes. But the point is that principles of efficiency *per se* do not cause anything, and do not explain anything, except in roundabout ways mediated through normal biological processes.

It is sometimes suggested by Chomsky and others (e.g., Chomsky 2008, 2010, Berwick & Chomsky 2011) that language emerged saltationally, as the result of a single mutation that made everything fall into place: "The simplest assumption […] is that the generative procedure emerged suddenly as the result of a minor mutation" (Berwick & Chomsky 2011: 29). Having a single large-effect mutation lead to something that is viable and perhaps even useful is not common, but does happen occasionally, due to robust developmental constraints and modular architecture. But having it lead to something that is optimal or perfect in any reasonable sense is unlikely in the extreme. Positing a saltational origin for language effectively amounts to positing sheer dumb luck as an explanation; I would hesitate to call such an explanation principled.

Hinzen & Uriagereka (2006) apparently have a perspective on what constitutes an explanation in biology that is fundamentally different from mine: "In the case of phyllotaxis, the Fibonacci arrangement originates as a mathematically (provably) best solution to a dynamic system of opposing forces in a compact space. That solution arguably has an adaptive effect for which it may have been selected, *but this is not its explanation"* (p. 89, emphasis added).[9] Instead they locate the explanation in "this universe's very topology" (p. 89). But there are several problems with having the universe's topology explain most biological features.[10] For one thing, as noted by Mayr (2004), quoted above, this kind of "explanation" is too general, not explaining the specifics of the biological situation. There is also a hidden normative in the explanation of Hinzen & Uriagereka (2006): "*best* solution". But the normativity here comes from biological considerations, the solution is "best" only with respect to the particular combination of adaptive desiderata and developmental constraints in the growth of plants. It is not "best" in any universal sense, which means that their argument falls apart if biological considerations are excluded.

In a biolinguistic analysis of language that takes the biological nature of the language faculty seriously, a causal analysis modeled on the style of analysis that has been fruitful in other parts of biology would appear to be the most natural way to proceed. This is no guarantee of success — it is an enormously challenging task — but going the opposite route, attempting to take an illegitimate shortcut with appeals to a uselessly vague third factor virtually guarantees failure. A critical review of a few such misguided "explanations" is the topic of the next section.

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[9] See section 4.1 below for more on Fibonacci.

[10] The situation is somewhat different in physics; e.g., the inverse-square laws that are ubiquitous in Newtonian physics do follow from the (not quite accurate) Newtonian assumption that the universe's topology is a three-dimensional Euclidean space.

4. "For Free, Directly from Physics"

Some biological features are postulated to come "for free, directly from physics" (Cherniak 2005: 103), caused by third-factor principles with no need for either genetic specification or environmental input (Benítez-Burraco & Longa 2010, Narita & Fujita 2010, Cherniak 2011), and thus no need for adaptive evolution either (Carstairs-McCarthy 2007). This reliance on physics as a basis for language goes back to Chomsky (1965: 59): "principles of neural organization that may be even more deeply grounded in physical law" (though Chomsky prudently conjoined this statement with "millions of years of evolution", as pointed out by an anonymous reviewer), and is reiterated both in Chomsky (1988) and in more recent work, for example Chomsky (2007b).

Effectively equating the third factor with physical law like this neglects the fact that physical law is only a small part of the complex of disjoint components that make up 'the' third factor, as discussed in section 2 above. But let us leave that aside for the moment, and see whether the physical component of the third factor has been shown to do any biologically useful explanatory work.

4.1. Do Aspects of Language Come for Free, Directly from Physics?

The actual workings of third-factor principles in language are not well understood, though Chomsky (2007b: 20) believes that "there has been considerable progress in moving towards principled explanation in terms of third-factor considerations". Unfortunately, I have yet to see such "considerable progress" in print. The few publications I could find that actually tried to do anything specific with third-factor principles (Walkden 2009, Carnie *et al*. 2005, Piattelli-Palmarini & Uriagereka 2008, Medeiros 2008, Soschen 2008, Kuroda 2008, Hinzen & Uriagereka 2006, Uriagereka 2008, Boeckx 2011)[11] did not provide results that warranted any conclusions about the causal efficacy of third-factor principles.

Walkden (2009) shows that the final-over-final constraint in syntax is consistent with being derived from computational principles. This is interesting, but does not show that such principles actually play any causal role behind the constraint, either in ontogeny or phylogeny.

Carnie *et al*. (2005), Soschen (2008), Medeiros (2008) and Piattelli-Palmarini & Uriagereka (2008) all focus on apparent Fibonacci patterns[12] in different aspects of language. The Fibonacci series is commonly seen in nature in the growth patterns of many organisms, for example leaves around a stem on a plant, and also in some inorganic systems (Douady & Couder 1992). It typically turns up in systems where a number of units are added one after the other to an area, and the units repel each other or otherwise try to keep as far apart as possible, but are prevented by opposing forces from moving around freely. This can either be a purely physical repulsive force as in the system studied by Douady and Couder

[11] Thanks to an anonymous reviewer for providing additional examples.

[12] The Fibonacci series of numbers is a sequence where each number is the sum of the two preceding numbers (e.g., 1, 1, 2, 3, 5, 8, 13, …). The series is named for Leonardo Pisano Bigollo (c. 1170 – c. 1250), nicknamed 'Fibonacci'. The ratio between two successive numbers in the series converges towards the 'golden ratio' 1.61803….

(1992), or a biological system where natural selection provides the repulsive 'force', or a biological system that has evolved to exploit physical forces to achieve a biologically desirable configuration. As noted by Piattelli-Palmarini & Uriagereka (2008), the forces involved in the formation of a Fibonacci pattern could act either over evolutionary time (becoming genetically encoded), over ontogenetic time (channeling development), or dynamically in real time throughout an organism's life. But regardless of the time scale, something force-like is required; Fibonacci is just an emergent mathematical pattern, without causal or even explanatory force of its own.

Under certain theoretical assumptions, Carnie *et al.* (2005) find Fibonacci-like patterns in syntactic trees. The patterns are not necessarily found in actual trees, though, but in maximal trees where all branching possibilities in the chosen theory of syntax are utilized down to a given depth in the tree. They connect the occurrence of these patterns with certain properties of Merge. Somehow, their argument proceeds from the observation that Fibonacci-like patterns do occur, to the claim that there is pressure for Fibonacci-like patterns to occur, and that these pressures have a causal role in shaping syntax: "Syntactic structure, […], strives towards the particular mathematical symmetry found in the Fibonacci sequence" (Carnie *et al.* 2005: 8), though they admit that this is speculative. It is not clear to me how striving follows from the arguments given.

Piattelli-Palmarini & Uriagereka (2008) extend the tree work of Carnie *et al*. (2005), connecting it with syntactic phases, and also identify Fibonacci-like patterns in syllable structures. They do not make any causal claims for Fibonacci in their 2008 paper, merely concluding that the Fibonacci patterns are real but their causes are not well understood, but in a later conference abstract presenting the same work, Piattelli-Palmarini (2012) does claim that "core properties of syntax come 'for free, from physics'" (p. 1).

Soschen (2008) extends the Fibonacci work in a somewhat different direction, trying to connect the apparent Fibonacci patterns in syntax with other apparent Fibonacci patterns in microtubules at the intracellular level inside neurons. Connecting syntax with neural processes could charitably be seen as an attempt to answer Tinbergen's "mechanism" question. But in any plausible model of neurolinguistics, there are multiple intervening levels of neural organization between syntax and microtubules, and Soschen (2008) does not provide any kind of coherent argument for how the Fibonacci pattern at one level is connected with the Fibonacci pattern at another level, quite distant from the first. Soschen (2008), like Carnie *et al.* (2005) and Piattelli-Palmarini (2012), also appears to have faith in the causal powers of Fibonacci: "Natural Law (N-Law), a physical phenomenon exemplified as the Fibonacci patterns […], can be observed in language, […]" (2008: 197) and "[t]his suggests a strong possibility that N-Law or general physical laws that ensure efficient growth apply to the universal principles that govern linguistic representations as well" (2008: 198). Again, it is not made clear precisely how a mathematical pattern acquires causal powers. Soschen (2008) also appears to share with Piattelli-Palmarini (2012) a curious conflation of mathematics with physics — Fibonacci is a mathematical pattern, and somehow the appearance of this *mathematical* pattern is equated with *physical* law.

Medeiros (2008) likewise proceeds from the same observations of Fibonacci patterns, but with more restraint and sophistication. There is much less of the sweeping, grandiose claims for the magical powers of Fibonacci; instead, Medeiros (2008) provides a mainly thoughtful and nuanced discussion of the implications of Fibonacci patterns in syntax. He still concludes that "laws of form" are at work, without actually having shown this in a causal analysis, but his work is nevertheless much more prudent and careful than the other Fibonacci-related papers discussed above.

The logic of much Fibonacci work appears to follow something like this chain of inferences:

(i) Apparent mathematical patterns are observed in some aspect of language.
(ii) → This pattern is taken as evidence of the third factor at work.
(iii) → The third factor causes language to be this way.
(iv) → This aspect of language is now explained.
(v) → This aspect of language comes "for free, directly from physics".

It is clear that every step in this chain is a *non sequitur*. The various authors cited above differ in how far down the chain of dubious inferences they proceed. But at least Piattelli-Palmarini (2012) appears to go all the way to the bottom, starting with "…Fibonacci growth patterns and principles of optimization are apparent in the structure of human language" and ending with "[t]his is a plausible instance of 'third factor' (Chomsky 2005) explanation; core properties of syntax come 'for free, from physics'" (p. 1).

The work of Kuroda (2008) is both mathematically and conceptually more sophisticated than the Fibonacci work discussed above, but part of it is similar in spirit.[13] Instead of the Fibonacci series, Kuroda identifies another mathematical pattern in a theoretical description of language: ζ functions.[14] Different ζ functions, it is argued, can represent different phrase structure languages. But Kuroda does not jump to conclusions right away — instead he makes the accurate assessment: "Speculative fantasies like this are easy to come by, but we are not in a position to tell if they might possibly have any linguistic significance" (2008: 35).

The other strand in Kuroda (2008) consists of an analogy between the structure of language and the structure of mathematics, and an argument for an ontology of language that parallels the Platonic-like mathematical realism favored by Kuroda, with both the logical structures of language and mathematics having some kind of real existence as objects in the natural world, and mathematics also existing in a Platonic world. He regards structural and processing perspectives on language as two complementary aspect that both exist at the

[13] As my knowledge of Japanese is limited, my discussion of Kuroda (2008) is mainly based on the English summary at the end of the paper. Thanks to Rie Asano for double-checking against the main Japanese text and clearing up some issues. Any remaining misunderstandings are my own.

[14] A ζ function is a function of a real number s that is defined as the infinite sum of some indexed function f_i with each term in the sum raised to the power s. It turns up in many different places in mathematics and also has some applications in physics.

same time, and he identifies an 'invisible' level of reality where mathematics and linguistics have strong parallels. Mathematical and linguistic abstract concepts both have a real existence at this invisible level, and are ontologically similar in Kuroda's view. To understand the visible parts of language, one must also understand the invisible. Kuroda (2008) calls his metaphysics "naturalistic realism". But, unless one endorses Kuroda's metaphysics, the structural analogies carry little explanatory weight.

Hinzen & Uriagereka (2006), like Kuroda (2008), draw parallels between mathematics and language, both structurally and metaphysically. Hinzen and Uriagereka also regard our mathematical abilities as both biologically and metaphysically closely connected with the language faculty. As with Kuroda (2008), the metaphysical aspects of their work has no explanatory force for anyone not accepting their unorthodox ontology of language.

More interesting in this context, however, is the claim of Hinzen & Uriagereka (2006) that one aspect of language — implicational hierarchies — comes for free if syntax uses the right mathematical tools. This does carry some explanatory force, but not because it belongs to 'the' third factor, which is wisely not mentioned in the paper. The careful linguistic/mathematical analysis done by Hinzen & Uriagereka (2006) is not an example of a third-factor explanation, despite being suggested as such by one anonymous reviewer. It is better regarded as an example of good Popperian hypothetico-deductive science. They take a hypothesis about the mathematical structure of syntax, and test it by deriving a surprising consequence of the hypothesis, which is then empirically validated.

Uriagereka (2008), also invoked by an anonymous reviewer as impressive third-factor work (cf. Narita 2009), appears to be at the core an attempt to extend minimalism beyond the traditional location of the C-I interface, integrating semantics with syntax (or, on a more radical alternative, syntax *constructing* semantics). A central concept is the co-linearity thesis (CLT), according to which it is postulated that "syntax and semantics turn out to be narrowly matched, perhaps trivially so" (p. xvii), together with a reinterpretation of the Chomsky Hierarchy. This is an ambitious work, making a bold and interesting conjecture about the nature of semantics and what goes on around and beyond the C-I interface. But it remains both conjectural and quite abstract, far from the level of empirical substantiation where it can reasonably be called an explanation for language.[15] Furthermore, Uriagereka (2008) does not, as far as I can find, even mention the term 'third factor' in the book, though the review by Narita (2009) hails it as important third-factor work. Instead of wading into the misty 'third factor' swamp, Uriagereka (2008) is quite properly being specific and clear about what principles and considerations he is using in building his model. Much of it certainly belongs somewhere among all the different things people include in the third factor, but labelling it as such would in no way strengthen Uriagereka's case, rather the opposite.

[15] As even the otherwise favorable review by Narita (2009) concedes: "Admittedly, most of Uriagereka's proposals await much finer empirial [sic] revision and substantiation" (p. 7 in preprint version).

Relativized Minimality (Rizzi 1990) is also held up by an anonymous reviewer as impressive third-factor work, even though it was published well before Chomsky coined the concept of a third factor. Rizzi does capture some important facts about language here (though there are plenty of complications; see e.g., Boeckx 2003), but there is no reason why the third factor should get any credit for that. The reason why Relativized Minimality largely holds may be due to some economy principle at work in syntax, or it may be due to performance effects in the parser, as argued by Ortega-Santos (2011); in both cases, the explanations do belong in the general third-factor bag, but they are clearly distinct from each other, and treating them both as 'the' third factor would be unhelpful.

Boeckx (2011) seeks a principled explanation for the binary branching in syntactic trees in an analogy with the work of Bejan (2000) on the general dendritic pattern of branching flows. Bejan shows that binary branching turns up in many different contexts of constrained flow, where the binary pattern minimizes flow resistance. This is interesting as an analogy for syntax processing, but it remains to be shown whether the various assumptions that go into Bejan's analysis actually hold for syntax. Furthermore, even if it were shown that Bejan's binary pattern would be optimal also for syntax, it would still be unwarranted to conclude that "[t]his need not be coded in the genome. As soon as Merge is available […] it would follow as a matter of course that Merge will exhibit a binary branching character if the FL is optimally designed" (Boeckx 2011: 57). Boeckx erects here an unfounded opposition between genetic encoding (and presumably adaptation) versus optimality, instead of recognizing that these are complementary, not competing, explanations;[16] cf. Jenkins (2011) and section 4.3 below. Jenkins expresses it as "principles of thermodynamic self-organization act in conjunction with genetically specified principles" (Jenkins 2011: 178).

Chomsky's own work invoking the third factor (e.g., 2008) has much in common with the approach of Uriagereka (2008). But the basic assumptions are not spelled out as clearly as one might wish; Chomsky postulates that language is "optimal" or "perfect" or "efficient" because of third-factor considerations, without really spelling out what this means — optimal with respect to what desiderata, efficient by what measure, and so on — even though Chomsky does recognize that this is an open issue: "We do not know a priori, in more than general terms, what are the right ways to optimize, say, neural networks; empirical inquiry into such matters is interactive in the same ways" (2008: 135-136). Too often the main criterion of perfection appears to be Chomsky's intuition. There is even a disturbing hint of circularity in some places, e.g., when Chomsky finds manifest imperfections at the SM interface, this is taken as evidence that the locus of perfection must be at the C-I interface (2008, 2010). Much of Chomsky's work here (e.g., 2008) erects impressive theoretical constructs, but they still fall well short of actually explaining language with 'the' third factor. There is too much leeway both in the definitions of optimality and so on, and in the selection of which aspects of language are to have principled explanations and which are shunted off beyond the interfaces, and in no small part this leeway results from the vagueness of 'the' third factor.

[16] Possibly this misunderstanding is connected with the 'ultra-Darwinist' strawman that Boeckx (2011) erects in this context?

The general conclusion I draw from reading putative third-factor explanations of language is that their stringency and explanatory force are in inverse proportion to how much emphasis is placed on the third factor. Those that achieve anything useful are precisely those that do *not* invoke 'the third factor' as if it were a causal force, but instead remain careful and precise about just what principles and assumptions they do rely on.

4.2. Does Neural Wiring Optimality Come for Free, from Physics?

Cherniak and associates (Cherniak *et al.* 2004, Cherniak 2005, 2011, Cherniak & Rodriquez-Esteban 2010) have investigated the optimality of neural wiring patterns, finding that the actual neural wiring in some model systems is remarkably efficient. This work, and especially the statement that this efficiency comes "for free, directly from physics" (Cherniak 2005: 103), is frequently cited in the biolinguistic literature on third-factor issues. But Cherniak and associates only show that under certain assumptions wiring patterns appear to be efficient; they do not demonstrate what causal factors lie behind such efficiency. Evidence that a certain configuration minimizes connection length, or minimizes internal wall drag (Cherniak 2011), or whatever, is not evidence that the configuration came for free, directly from physics. A causal link from principle to configuration is required.

The finding of Fornito *et al*. (2011) that there is genetic variation in the efficiency of neural wiring patterns in humans, and that efficiency differences are under strong genetic control, is relevant in this context. This result entails three pertinent conclusions:

- Neural wiring patterns, at the level studied, are under genetic control, and thus subject to all the usual biological processes. This contradicts the claim of Cherniak and others that the efficiency comes directly from physics, *without* genetic input.[17]
- Neural wiring patterns, at the level studied, are not always optimal, as some people have measurably more efficient patterns than others.[18]
- The efficiency of neural wiring is evolvable, as there is genetic variation that provides a handle for natural selection — assuming, of course, that efficiency is correlated with fitness.

As noted by both Cherniak & Rodriquez-Esteban (2010) and Fornito *et al.* (2011), there are many competing desiderata in the wiring of a neural network — connection cost, connectivity, computational speed, energy dissipation, robusticity, and so on. It is not well understood which of these is most important, and how the conflicting demands are balanced. This makes it effectively impossible to determine whether a neural network is optimal in any general

[17] For example, in Cherniak & Rodriguez-Esteban (2010: 52): "not via the genome but by the underlying physical and mathematical structure of the universe".

[18] Like Cherniak and associates, Fornito *et al*. (2011) do find quite high levels of cost-efficiency. But instead of concluding that some physical principle is at work, they conclude instead that brains evolved to be efficient.

sense, without assuming which desideratum should be optimized. This is in stark contrast with the abstract optimality proposed by Piattelli-Palmarini & Uriagereka (2008), who talk about "structures that can be characterized as optimal *irrespective of any functional correlate*" (2008: 209, emphasis in original), which is either vacuous or incoherent.

Recent empirical enquiry into actual neural network organization is reviewed by Bullmore & Sporns (2012: 347), who conclude that "the brain's connectome is not optimized either to minimize connection costs or to maximize advantageous topological properties […]. Instead, we argue that brain network organization is the result of an economical trade-off between the physical cost of the network and the adaptive value of its topology". This type of trade-off between multiple objectives, rather than optimization for any single objective (much less any abstract objective-less pseudo-optimization), is typical of biological systems (Noor & Milo 2012).

A further consideration is that neurons are rather unreliable as circuit elements, prone to misfiring and occasionally dying. This is a constraint on neural processing that has the consequence that an optimal neural circuit (assuming, plausibly, that reliability is a desideratum) needs to have enough redundancy to make it highly robust against neuron failure, rather different from what might seem optimal to an engineer used to reliable electronic components (Fitch 2009), or to a theoretician focusing on minimalistic elegance.

An explanation of neural wiring patterns, as with other biological features, needs to answer all four Tinbergen questions. Even if the patterns were shown to be optimal in some sense, an appeal to the third factor does not in itself provide such answers. Further analysis is needed. Cherniak & Rodriguez-Esteban (2010) do take a small step in this direction, reasoning in terms of proximate mechanisms in their discussion, noting that wiring fulfilling their efficiency criteria can be achieved if each nerve connection acts as a mechanical spring providing a force pulling the nerve cells together.[19] Minimizing the energy of such a spring network, which indeed is what it will spontaneously do if left to the laws of physics, also maximizes efficiency. But Cherniak & Rodriguez-Esteban (2010: 52) err in concluding that this efficiency comes "not via the genome but by the underlying physical and mathematical structure of the universe" — the genome still has to provide the coding for nerve connections to act as springs of appropriate strength, and for nerve cells to be free to move around in response to such spring forces, neither of which comes for free. Cherniak (2011) continues the argument of Cherniak & Rodriguez-Esteban (2010), considering both developmental and evolutionary time scales. He approaches, but does not quite reach, the reasonable conclusion that what we have here is not physics short-circuiting evolution, but rather the neural system evolving to take advantage of physical laws. The latter is ordinary evolutionary tinkering, genetically encoding the system to exploit physics, not getting anything for free. Nevertheless, Cherniak (2011: 116) concludes that this "constitutes a thesis of nongenomic nativism, that some innate complex biological structure is not encoded in DNA, but instead derives from basic

[19] A similar idea was proposed also by Van Essen (1997).

physical principles", a conclusion that does not follow either from his data or his arguments.

Most language-related neural wiring we simply do not understand well enough to tell whether it is optimal or not, and if so, optimal with respect to which desiderata. But there are a few cases that we do understand, which are clearly not optimal by any reasonable criterion. Precise control of the timing of laryngeal activities ought to be vital for speech, as parameters such as voice onset time need to be controlled with an accuracy of a few milliseconds at normal speaking speeds (Ladefoged 1971, Cho & Ladefoged 1999). The laryngeal nerve, providing motor signals to the muscles controlling the larynx, is made up of several branches. At least two aspects of the routing of this nerve are far from optimal:

- The left recurrent branch of the laryngeal nerve does not go straight to the larynx from its origin in the brainstem. Instead it goes down into the thorax, loops around the aorta, and then back up the neck to the larynx, adding many unnecessary centimeters to its length in humans, and thus adding a non-negligible propagation delay for motor signals. In animals with longer necks, such as giraffes or sauropod dinosaurs, the extra length can be measured in meters (Wedel 2012).
- The other branches of the laryngeal nerve all take different routes. The right recurrent branch only goes halfway down to the thorax, looping around the subclavian artery instead of the aorta, and the superior laryngeal nerve actually does take the direct route to the larynx without looping around anything. The difference in path length between the right and left recurrent branch in humans is 10 cm (O'Reilly & Fitzgerald 1985), adding a totally unnecessary complication in providing the left and right side of the larynx with precisely coordinated motor signals.

In some distant ancestor with anatomy quite different from ours, these roundabout routes may well have been direct paths. Today we're stuck with them, due to deeply rooted developmental constraints. No third-factor principle of efficiency has cleared the tangle of nerves and blood vessels in our neck; instead we have here developmental constraints, also supposedly part of the third factor, causing blatant inefficiency.

4.3. Proposed Non-Linguistic Examples of Biological Features Coming for Free from Physics

The biology and neurology of language is for the most part insufficiently understood to say much yet about causes and explanations. The biolinguistic literature on third-factor issues therefore invokes a number of better-understood examples from other areas of biology than language as examples of third-factor principles at work (e.g., Narita & Fujita 2010, Jenkins 2006). But the biology of these examples is well enough understood that we can say clearly and emphatically that they do not come "for free, directly from physics".

Bone structure is a case in point. It is quite true that bone is commonly structured in a way that achieves maximal strength with minimal material, as stated by Narita & Fujita (2010). But this does not come "directly from physics", it comes from the interplay between the laws of physics and a complex process of active remodeling in the bone. The biological material in the bone senses the strain on the bone, adding material where the strain is large, and removing material where the strain is small. The strain patterns do come from physics, but the remodeling does not. The strain-sensing adding/removing process is effected by a highly complex network of cellular and molecular systems (reviewed in Robling *et al*. 2006), genetically specified and presumably evolved through natural selection. It did not come for free. The only thing that physics provides for free here is a feedback signal; the system for sensing that signal and reacting appropriately had to evolve the hard way. Bone structure thus is *not* the pure result of third-factor principles at work; it is the result of biological processes in interaction with, and exploiting, physical laws.

Similarly, the shape of a bird's wings and feathers do come from the physical laws of aerodynamics, sort of. But they do not come for free. Birds do not automatically acquire wings with good aerodynamic properties. Instead, if good flying ability increases the fitness of a bird, then birds with wings and feathers providing better flying abilities will have more offspring and such wing shapes will spread in the population. The only role of the physical laws of aerodynamics in this process is to determine which shapes provide better flying abilities. The actual shaping has to be done through normal evolutionary and developmental processes.

Jenkins (2006) and Fujita (2007) invoke protein folding as an example of a biologically important process that comes from third-factor principles. This is a better example than bone or feathers, as a newly built protein typically does fold directly through physical processes into a configuration determined by its amino acid sequence. But biologically useful folding still does not come for free, for three reasons:

- The process is fairly reliable *in vitro* for most proteins, where no other factors interfere. But *in vivo* the folding frequently fails, and we have a fair amount of intracellular machinery dedicated to folding assistance, notably the chaperone proteins (Lee & Tsai 2005).
- There is no particular reason for the folding that comes from physics to be the biologically optimal one.
- The amino acid sequence that determines the folding is itself the product of a long evolutionary history, where appropriate and reliable folding is likely to have been a non-negligible selective pressure. Mutations that cause misfolding are common, and are a major cause of diseases such as cystic fibrosis.

Cells having spherical shape rather than, say, cubical, is another example, mentioned by Chomsky (2011) as following directly from physics without any genetic input needed. It is true that an isotropic elastic membrane will spontaneously make a spherical shape, in the absence of other forces than an

internal pressure. But many, probably most, cells are not spherical; there is a wide variety of cell shapes both among single-celled and multicellular organisms. Non-spherical shapes result either from a non-spherical distribution of forces on the cell membrane, or from non-uniformity of the membrane itself. As with protein folding, this is again a case of physics providing the mechanism doing the actual shaping, but with biological processes in control of the physical forces, setting up the situation so that the physical forces produce a biologically appropriate cell shape, which may or may not be spherical. Physics provides the proximate cause, but the ultimate cause why a certain cell has a certain shape is biological, not physical.

4.4. Is Physics Explained by Third-Factor Principles?

Shifting the explanatory burden to the third factor has a dual purpose. It is supposed to ease further the problem of accounting for the origins of the genetic endowment, but it is also an important aspect of the desire to go "beyond explanatory adequacy" (Chomsky 2004). Boeckx & Piattelli-Palmarini (2005) see deep parallels with the aims of fundamental physics, as envisioned by thinkers like Feynman or Einstein, whose work also involves going beyond just descriptive laws and explanatory theories, and arriving at a principled understanding of why nature is the way it is. As correctly noted by Boeckx & Piattelli-Palmarini (2005: 454): "The question is not whether this new development in the field is legitimate, but rather whether it is premature".

But the legitimacy of the quest for principled explanations does not obviate the need for causes that do the immediate work, the result of which may (or may not) be explained by fundamental principles. If nothing else, without working through the intermediate steps we cannot know if a putative principled explanation actually does explain anything, or if it is just a coincidental pattern resemblance, or even the result of wishful thinking in the quest for principled explanations.

In physics, the principle of least action can be regarded as the ultimate cause behind many processes in nature, providing a principled explanation behind many physical laws. One example is Snell's law of refraction, which in itself is purely descriptive, lacking any kind of explanatory adequacy. Least action provides a principled explanation of why Snell's law is the way it is. But behind Snell's law are also proximate explanations of why photons behave the way they do — see e.g., Feynman (1985). Feynman's explanation may even be generalized to a proximate explanation of why physics in general follows the principle of least action. I do not think I am alone in finding the combination of proximate cause and ultimate principle much more satisfactory than either one taken in isolation. This is related to the point made by Mayr (1961) and Tinbergen (1963), discussed above — their different levels of explanation are complementary, not exclusive, and the full complement of answers is needed for a satisfactory explanation.

Furthermore, it is not a given that every individual feature in nature does have a principled explanation. Some features do, but others are historical contingencies. This is true even within the physical sciences. For example: There

is a principled explanation why a star with the mass and composition of the sun has the temperature and luminosity that it has — but the mass and composition are matters of historical contingency, due to accidental circumstances during star formation once upon a time. Similarly for the planets of the Solar System: We do have a principled explanation for the general pattern, with small rocky inner planets and large gaseous outer planets, based on condensation processes in the original nebula — but the specific pattern of planets, with precisely four inner planets and four outer planets, and Earth and Venus having roughly the same size, is a pure historical contingency based on effectively random events during accretion. That the real world is not uniformly principled goes hand in hand with Chomsky's (e.g., 2008, quoted above) emphasis on the conjectural nature of his third-factor work, and should be kept in mind by biolinguists pursuing this conjecture.

5. Conclusions

The third factor of Chomsky (2005) has received much attention in recent biolinguistic work. But the attempts so far to actually use third-factor considerations as a major constituent in the explanation of language are uniformly unconvincing. The only supposedly third-factor based works that are worth taking seriously are those that do *not* invoke any blanket third factor, but instead are explicit about which specific principles their analysis is based on, e.g., Uriagereka (2008). Typical of the papers explicitly invoking the third factor, in contrast, is that no serious causal analysis is performed, and no causal connections from third-factor principles to linguistics are presented; instead the literature abounds with unsupported claims that this or that feature comes "for free, directly from physics". I regard this unfortunate state of affairs as the result of three conspiring factors:

(1) Insufficient attention is paid in biolinguistics to the causal analysis of the human language faculty. Tinbergen's four questions are rarely considered, despite their prominent position in "The *Biolinguistics* Manifesto" (Boeckx & Grohmann 2007).

(2) 'The' third factor is a vague catch-all category, mixing entities with totally different causal and epistemological status, rendering its analytical value highly dubious.

(3) At the same time, third-factor-based "principled" explanations are held up as a goal, especially by Chomsky (e.g., 2010).

The three points above conspire to give an undeserved air of legitimacy to sweeping, unwarranted claims of language "coming for free", as soon as something that might be the third factor is involved, tempting some biolinguists into drawing conclusions based more on their desire for principled explanations than on actual data and analysis. Instead of succumbing to this temptation, the following points should be kept firmly in mind:

- As 'the' third factor in its current form is not a coherent well-defined concept, any analysis invoking the third factor must carefully and explicitly consider just what kind of principles are being invoked.
- The observation of a putative third-factor pattern in an aspect of language does not in itself warrant the conclusion that the third factor *explains* that aspect of language. A pattern in language is a *clue* to possible explanations; it is not a principled explanation in itself, unless the causal connection is established and understood. A detailed case-by-case causal analysis is required.
- Searching for patterns can be a valuable heuristic in tackling problems that are difficult or intractable otherwise. But keep in mind that this is a heuristic only, a hypothesis-generator, not an end in itself.
- Postulating optimality or efficiency can likewise be a useful heuristic in the search for hypotheses in an evolutionary context. But this heuristic is useful only under the assumption that natural selection has optimized the feature in question[20], not otherwise, and its main value lies in providing clues to what natural selection has optimized *for* – cf. Tinbergen's first question.
- Mathematical elegance and beauty is nice, and at least in physics searching for elegance has a fair track record as a heuristic (Johansson 2006). But pursuing elegance for its own sake, beyond empirical support, can lead research badly astray (Woit 2007).
- The distinction that Dennett (1995) makes between skyhooks and cranes should also be kept in mind; third-factor arguments are too often used as skyhooks.
- While it would be nice if all aspects of language did have principled explanations, there is no guarantee that this will be the case. There is no serious quest for principled explanations for the vocabulary and quirks of individual languages; everybody agrees that such features are historical contingencies. The core features of syntax is the area where principled explanations can be hoped for and conjectured, but even there we have no guarantee. As repeatedly emphasized by Chomsky, the principled character of core syntax remains a conjecture. The goal of explaining language in a principled manner does not come closer by attempting to short-circuit the process with vacuous claims of getting something "for free, directly from physics".

I propose that a better way for biolinguistics to proceed is to return to its roots, as expressed in Boeckx & Grohmann (2007), taking seriously the biological nature of language that is supposed to be at the core of the biolinguistic enterprise. This means analyzing language in the same way as biologists analyze other biological features, basically as described in section 3 above. Many of the components of 'the' third factor likely have roles to play in that analysis — but unwarranted shortcuts do not.

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[20] Or under the assumption that language became perfect purely by random accident —but such a pseudo-explanation is both extremely unlikely and far from principled.

Of course we should proceed with due consideration given to the extraordinary nature of language compared with other biological features, but we should at the same time scrupulously avoid the "methodological dualism" that Chomsky (1995) warns against, not treating language as different in kind from other biological features, not seeking explanations for language that are different in kind from the explanations sought for other biological features. Physical laws and efficiency considerations should have the same kind of place in the explanation of language as in the explanation of, for example, the eye or any other biological feature.

The unification of linguistics with the rest of science remains a goal that we have in common. But it is misguided to attempt the unification of linguistics with physics before biology. We are doing biolinguistics, not physicolinguistics.

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Is Word Order Asymmetry Mathematically Expressible?

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The computational procedure for human natural language (C_{HL}) shows an asymmetry in unmarked orders for *S*, *O*, and *V*. Following Lyle Jenkins, it is speculated that the asymmetry is expressible as a group-theoretical factor (included in Chomsky's third factor): "[W]ord order types would be the (asymmetric) stable solutions of the symmetric still-to-be-discovered 'equations' governing word order distribution". A possible "symmetric equation" is a linear transformation $f(x) = y$, where function f is a set of merge operations (transformations) expressed as a set of symmetric transformations of an equilateral triangle, x is the universal base vP input expressed as the identity triangle, and y is a mapped output tree expressed as an output triangle that preserves symmetry. Although the symmetric group S_3 of order 3! = 6 is too simple, this very simplicity is the reason that in the present work cost differences are considered among the six symmetric operations of S_3. This article attempts to pose a set of feasible questions for future research.

Keywords: cost; economy; equilibrium; Galois group; geometry; symmetry; third factor; transformation; unmarked word order

1. Introduction

1.1. Problem

I would like to pose the question of whether the following phenomenon can be mathematically (Galois theoretically) expressed.[1]

I am grateful to the editors and anonymous reviewers for their patience in assessing this challenging article over the past two years. I would like to thank Makoto Toma for his valuable comments and suggestions. Without his constructive criticism regarding my amateurish mathematics, I could not have finished this. I thank Massimo Piattelli-Palmarini for allowing me to join his class on biolinguistics at MIT in 2003, which marked the beginning of this project. I am grateful to Lyle Jenkins for the insightful lecture on human language and Galois theory in Massimo's class and for taking the time to listen to my idea in a campus café. Finally, I would like to thank Enago for editing and proofreading my work, which clarified the reasoning that I wished to express. All remaining errors are my own.

[1] The author does not claim that the geometrical cost calculation proposed here is the 'third factor' (non-genetic and non-environmental) that is actually at work in C_{HL}. Rather, he claims that it may be a mathematically feasible way to express and translate the unmarked word order asymmetry into a language of geometrical cost calculation that leads us to

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(1) In terms of phylogeny,[2] C_{HL} shows the following language distribution: <*SOV*> = 48.5%, <*SVO*> = 38.7%, <*VSO*> = 9.2%, <*VOS*> = 2.4%, <*OVS*> = 0.7%, <*OSV*> = 0.5% (Yamamoto 2002).[3]

Why do we focus on *S*, *O*, and *V*?[4] There are four reasons. First, many reliable studies since the seminal work of Greenberg (1963) present relatively solid evidence regarding the probability of unmarked word orders. Second, we have reliable data from native speakers, who have relatively clear intuitions about what the unmarked order of the set {*S, O, V*} is for their languages. The third reason is simplicity: we should start from the simplest possible case. The fourth reason is reducibility: we can and should reduce seemingly complex structures to the simplest possible structures, namely *S* + *V* and *S* + *O* + *V*. *S* and *O* may be complex, but they are reducible to the simple *S* and *O*. *O* may be direct (*DO*) or indirect (*IO*), but we start from the simpler *DO*. Sentence structures contain CP, TP, *v*P, and VP, but the most basic semantic domain is *v*P+VP, in which *S*, *O*, and *V* appear originally. Yamamoto (2002: 85) contains a table that is useful for comparing the relevant percentages that have appeared in previous studies. Here I have included Yamamoto (2002), Dryer & Martin (2011), and Gell-Mann & Ruhlen (2011).[5] This full list is shown in Table 1.

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algebraic and group-theoretical analyses in the future. I thank an anonymous reviewer for clarifying the issue. With regard to Galois theory, Évariste Galois (a French mathematician; 1811–1832) developed the fundamental mathematical tool, the Galois group (algebraic structure of equations), for examining the symmetry of equations. Modern science would not exist without Galois theory. Group theory is a calculus of symmetry (Stewart 2007: 111). In Chomsky (2002), Fukui and Zushi mention Weil (1969), which is a group-theoretical analysis of an aboriginal kinship structure in Australia (Japanese translation of Chomsky 1982: 356). As regards other group-theoretical analysis on C_{HL}, see Laughren (1982), in which the author attempts a group-theoretical analysis of Walpiri kinship structure (languages of kinship in Aboriginal Australia). See also Jenkins (2013) for the introduction of Laughren (1982). In Chomsky (2002), Fukui and Zushi suggest a possibility of "Galois theory of phrase structure (I-language)" (Japanese translation of Chomsky 1982: 397–398).

[2] The phylogeny problem (species puzzle) asks why a language system (the current C_{HL}) behaves in a particular way, "the historical development of languages" (Di Sciullo 2013). However, we are concerned with synchronic phenomena (why the current C_{HL} appears like this; how it has come to have the property; what the cause is) and we put aside the actual diachronic analysis. The ontogeny problem (individual puzzle) asks how a human child acquires his or her mother tongue, i.e. "the growth of language in the individual" (ibid.).

[3] Yamamoto (2002) considers the largest number (2,932) of languages for typological analysis to date (gross=6,000). The actual number used for calculating the percentages is 2,537. Given that many previous studies have only considered 20 or 30 to 200 or 300 languages, Yamamoto (2002) offers a significantly reliable sampling. <…> indicates an ordered set of unmarked (basic) word order. The ratio is rounded to the first decimal place.

[4] I thank an anonymous reviewer for pointing out this fundamental question.

[5] I added Yamamoto (2002; gross: 2,537 languages), Dryer & Martin (2011; gross: 1,377), and Gell-Mann & Ruhlen (2011; gross: 2,011). In Dryer & Martin (2011), 189 languages have no dominant order. Selected language families and samples are provided below.

<*SOV*>: Niger-Congo, Semitic, Turkic, Indo-Aryan, Dravidian, Austonesian, Altaic, Chibchan, Native American languages, …

<*SVO*>: Indo-European, Niger-Congo, Tai-Kadai, Sinae, Austronesian, Arawakan, …

<*VSO*>: Celtic, Semitic, Niger-Congo, Austronesian, Native American languages, Chibchan, …

<*VOS*>: Malagasy, Batak, Seediq (Austronesian languages), Native American languages, Chibchan, …

	SOV	*SVO*	*VSO*	*VOS*	*OVS*	*OSV*
Greenberg 1966	37.0%	43.0%	20.0%	0.0%	0.0%	0.0%
Ultan 1969	44.0%	34.6%	18.6%	2.6%	0.0%	0.0%
Ruhlen 1975	51.5%	35.6%	10.5%	2.1%	0.0%	0.2%
Mallinson & Blake 1981[6]	41.0%	35.0%	9.0%	2.0%	1.0%	1.0%
Tomlin 1986	44.8%	41.8%	9.2%	3.0%	1.2%	0.6%
Matsumoto 1992	49.3%	35.0%	11.2%	2.8%	1.0%	0.6%
Yamamoto 2002	48.5%	38.7%	9.2%	2.4%	0.7%	0.5%
Dryer & Martin 2011	41.0%	35.4%	6.9%	1.8%	0.8%	0.3%
Gell-Mann & Ruhlen 2011	50.1%	38.0%	8.0%	2.0%	0.8%	0.6%
Average	45.2%	37.5%	11.4%	2.1%	0.6%	0.4%

Table 1: Unmarked Word Order Asymmetry Produced by C_{HL}

However, an anonymous reviewer points out many fundamental problems. Why should we focus on the ordering among *S*, *O*, and *V*? Is it not the case that *S*, *O*, *V* are the grossest levels of organization of the clause, hence encompassing the maximal level of complexity? Is it not the case that unmarked orders such as <*SOV*> and <*SVO*> are shadows, not the essential substances? Is it not possible that the unmarked <*SOV*> has many other derivations, hence leading to different varieties of unmarked <*SOV*>?[7] Why is <*SOV*> the base order? Why should the base order be the most common? If <*SOV*> is the cheapest, why is it not the case that all languages show <*SOV*> as the unmarked order? Why does an unmarked order such as <*OSV*> (0.5%) exist at all?[8] I attempt to answer these questions as far as possible. However, the questions are so fundamental that a complete answer is beyond the reach of this paper. Although the article faces many

<*OVS*>: Päri (Niger-Congo), Ungarinjin (moribund Australian aboriginal language), Hixkaryana (Carib language), Tuvaluan (Austronesian)

<*OSV*>: Kxoe (Kalahari), Tobati (Papua New Guinea), Wik Ngathana (Pama-Nyngan), Nadëb (Brazilian Amazon)

[6] 11% of languages are unclassified in this study. I thank an anonymous reviewer for pointing out that referring Yamamoto alone is insufficient.

[7] The reviewer suggests that distinct operations yielding the same superficial <*SOV*> unmarked order, for example, are parametrized.

[8] I thank an anonymous reviewer who pointed out this serious problem that my approach should solve. The readers can refer to Yang (2002) and Chomsky (2012) for the method behind the explanation of the statistical duality of irregular verbs. The reviewer's puzzle (a phylogeny issue) is particularly important in that it relates to an important statistical paradox (an ontogeny issue) as follows (Yang 2002, Chomsky 2012): Why do low-probability irregular verbs behave like high-probability regular verbs such that irregular verbs are as naturally and frequently used as regular verbs? Why do irregular verbs exist at all? Yang (2002) has discovered that irregular verbs are in fact 'regular' for they are grouped into distinct classes and the classes obey the relevant regular rules. For example, the blocking effect of the past tense form *went* over *goed* indicates that the 'weight' (probability) of the corresponding rule is 1.0 (must happen) or very close to 1.0 (very likely to happen) as a result of learning. Following his insight, I will argue later that a low-probability unmarked order such as <*OSV*> behaves like a high-probability order because the cost calculation is 'regular': The gross computational cost is within the threshold permitted for C_{HL} (the minimum cost). The blocking effect of unmarked <*OSV*> over <*SOV*> indicates that the 'weight' (probability) of the corresponding cost calculation is 1.0 (must happen) or very close to 1.0 (very likely to happen) as a result of cost equilibrium.

problems, let us first look at what typological studies have found with respect to the probability of unmarked (basic) word order asymmetry and see how far we can go within the geometrical cost approach.

Greenberg (1966) showed that <*SVO*> languages outnumber <*SOV*> languages, and Yamamoto (2002: 85) attributed this unlikely result to the smaller samples (30 languages) and a bias toward Indo-European and African languages, excluding the languages of New Guinea and Melanesia. The general ranking of unmarked word order seems to be clear:

(2) $SOV > SVO > VSO > VOS > OVS >? OSV$

It is significant that <*SOV*> and <*SVO*> account for more than 80%. C_{HL} is strongly biased for these two unmarked word orders. Can we say as follows? Starting from <*SOV*>, <*SVO*> involves flipping *O* and *V*, and <*VSO*> involves rotating one position rightward, <*VOS*> involves flipping *S* and *V* (or it is a one-dimensional mirror image of <*SOV*>). Where does the ranking in (2) arise from? Why does C_{HL} select this particular ranking? The main goal of this study is to show that the ranking is expressible as geometrical cost differences, which will ideally lead to a Galois-theoretic explanation, and that C_{HL} chooses the most cost-effective unmarked word orders with respect to the phylogeny (the issue of why we can observe the current probability regarding unmarked word order asymmetry in human language). However, it is also a fact that all six possible unmarked word orders show symmetry and they are each the result of the most efficient computation with respect to ontogeny (the issue of why all six word orders are respectively the most natural and frequent unmarked orders for the respective native speakers). In a sense, phylogenetically minor unmarked orders such as <*OSV*> are similar to irregular verbs because they show low probability (we do not find many samples) but simultaneously show high probability (they are the most natural, frequent, and unmarked orders for the respective native speakers). Why do minor unmarked orders show low probability but simultaneously show high probability? I will offer a possible answer to this paradox in the last part of Section 3. With regard to the basic statistical data, I tentatively adopt Yamamoto (2002) in the following sections because it contains the largest data set available at present (2,932 languages).

1.2. Chomsky's Third Factor

The biolinguistic approach tackles the problem of whether we can explain C_{HL} by natural laws, which Chomsky calls the third factor. The third factor includes "principles of neural organization that may be even more deeply grounded in physical law" (Chomsky 1965: 59) and "principles of structural architecture and developmental constraints that enter into canalization, organic form, and action over a wide range, including principles of efficient computation, which would be expected to be of particular significance for computational systems such as language" (Chomsky 2005: 6).[9] Approximately half a century of biolinguistic

[9] The first factor is the human genome (the DNA and brain that yield properties of C_{HL} such

research has revealed that there are parts of C_{HL} that obey the principle of efficient computation, informally stated as follows:

(3) *Economy Principle (Minimal Computation)*
Select the most cost-effective computation.

Measures of effective computation include the least effort, the shortest distance, the closest element, the fewest steps, the simplest structure, and the minimal search. The initial state of C_{HL} is an organic computational system that includes the Economy Principle that governs an inorganic world. The initial state of C_{HL}, which is given by the human genome, undergoes parameter setting in a linguistic environment until C_{HL} reaches the final state, the point at which the mother-tongue acquisition system deactivates.[10] C_{HL} is a system that exhibits the discrete infinity property, which typically appears at the molecular level or below. A system of discrete infinity obeys the Economy Principle, such as a snowflake's hexagonal shape emerging as the idealized (optimized) realization of the atomic structure of H_2O in midair, free from the noise of gravity and earth's thick air. As Chomsky often mentions, it would be interesting if an inorganic principle were operating on organic matter such as the human brain.[11]

I assume that the group-theoretical principles of an algebraic structure belong to the third factor. Jenkins (2000, 2003) suggested that "word order types would be the (asymmetric) stable solutions of the symmetric still-to-be-discovered 'equations' governing word order distribution" (Jenkins (2000: 164) and that "the tools of group theory may be able to aid in characterizing the symmetries of word order patterns" (ibid.: 164).[12] I believe that a study of the

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as discrete infinity and merge) and the second factor is the linguistic environment. The first factor is a force internal to C_{HL}, and the second and third factors are external forces (Yang 2000). The first and second factors are responsible for the ontogeny of C_{HL} (how C_{HL} grows in the brain of a human infant), while the third factor is responsible for the phylogeny (why C_{HL} has evolved in such a way). The interaction of these three factors determines the facts of C_{HL}. Boeckx (2009: 46) points out that Chomsky's 'three factors' resemble Gould's 'adaptive triangle' (Stephen Jay Gould, American paleontologist, evolutionary biologist, and historian of science; 1941–2002), which has three vertexes: (1) historical (chance); contingencies of phylogeny (mutation of DNA, 1st factor), (2) functional; active adaptation (environmental pressure, 2nd factor), and (3) structural constraints; rules of structure (physical laws, 3rd factor) (Gould 2002). See Uriagereka (2010) and Longa *et al*. (2011) for relevant discussions.

[10] C_{HL} is generally active for mother-tongue acquisition until approximately the appearance of secondary sex characteristics. Many mysteries exist regarding the issue.

[11] With regard to the connection between Hamilton's principle of least action in physics and the third factor in C_{HL}, see Fukui (1996). I thank an anonymous reviewer for suggesting that I should mention Hamilton's principle in this connection.

[12] The assumption here is that an asymmetric state is stable; a symmetric state is too tense and expensive to maintain and such an unstable symmetric state becomes stabilized (costless to maintain) when the symmetry is broken. For example, Kayne (1994) proposes that syntactic terms must be in an antisymmetric c-command relation. Moro (2000: 15–29) claims that a symmetric structure (a point of symmetry) is too unstable for C_{HL} to tolerate and that symmetry must be broken, and this drives movement, stabilizing the structure. Di Sciullo (2005, 2008) investigates symmetry breaking (as a result of 'fluctuating asymmetry (oscillation)') in merge and morphology. In contrast, from the viewpoint of physics, a symmetric situation is stable (highly probable). An example is a gas, in which every direction appears the same. Symmetry forming is information diffusion and obeys the

algebraic structure of equations (Galois group) will help us to express the phylogeny problem concerning permutation asymmetry in C_{HL}. I attempt to express and translate the unmarked word order asymmetry into Galois-theoretic language, by considering cost.

The rest of this paper is organized as follows. In Section 2, I claim that C_{HL} produces the universal base *v*P, where *S* c-commands *O*, and *O* c-commands *V*, and that this base *v*P corresponds to the identity element (*I*) in mathematics. In Section 3, I propose that geometrical cost asymmetry is a possible "language" to express the unmarked word order asymmetry. I would like to propose that the unmarked ordering asymmetry in C_{HL} can be expressed by Galois-theoretic language: the third factor.[13] In particular, I propose a possible "equation governing [unmarked] word order distribution". Moreover, I also attempt to answer an important question: Why is it not the case that all languages show unmarked <*SOV*> provided that <*SOV*> derives from the most efficient computation? Section 4 summarizes the paper.

2. The Universal Base *v*P as the Identity Element

I propose that C_{HL} creates the universal base *v*P, which is the identity element (identity syntactic relation) under the Merge operation.[14] The base *v*P has the c-command relation $S \gg O \gg V$, as shown in Figure 1.[15] The base *v*P is formed

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entropy law: Disorder develops (the second law of thermodynamics). Symmetry breaking is information condensation and disobeys the entropy law, i.e., order develops. An example is a crystal, in which things look different according to the viewpoint. For Kayne, Moro, and Di Sciullo, structure building is symmetry breaking, which produces information, disobeying the entropy law. On the other hand, Fukui (2012a) proposes that F(feature)-equilibrium (symmetry formation) drives structure building. F-equilibrium obeys the entropy law. For Fukui, structure building is symmetry formation: information loss. There is no contradiction. Kayne, Moro, and Di Sciullo discuss how structures produce phonetic (sound) and semantic (meaning) information, which must not be deleted, whereas Fukui talks about how structures lose formal features (structural information), which must be deleted.

The issue is related to a diachronic question that an anonymous reviewer asks as follows: What will happen to the synchronic unmarked order asymmetry? Will all languages become <*SOV*> type, provided that it is the most efficient? Although the diachronic issue is beyond the reach of this paper, at this point, let us tentatively assume as follows. The diachronic change may be determined by the dynamic interaction between the two forces noted above: symmetry breaking and symmetry preservation (formation).

[13] An anonymous reviewer suggests that S-initiality is largely areal (geographical proximity of other S-initial languages) (Dryer 2012). If so, we should conclude that it is primarily the environmental factor that induces the unmarked word order asymmetry. Although the issue is beyond the scope of this paper, let us tentatively start with the view that all three factors (genetic, environmental, and physical) are involved in the asymmetry in question.

[14] I focus on the structure of a simple matrix transitive sentence (consisting of *S*, *O*, and *V*) that the relevant native speakers judge to be the unmarked (basic) word order (actually their C_{HL} reaction). C_{HL} is what motivates the universal base *v*P. I thank an anonymous reviewer for pointing out this unclarity. I call the universal base *v*P the base *v*P for simplicity.

[15] The definition of c-command is as follows (Uriagereka 2012: 121):

(i) *α* c-commands *β* if
(i) *α* does not dominate *β*, and
(ii) all nodes that dominate *α* also dominate *β*.

with the least effort, that is, only an external merge (the simplest possible structure-building operation) builds it. Every sentence structure starts with the base *v*P. If TRANSFER applies to the base *v*P, the phonological component Φ (sensorimotor interface) produces <*SOV*> as the unmarked order.[16]

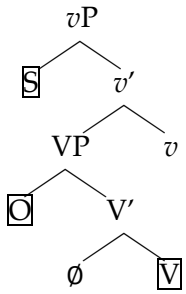

Figure 1: The universal base vP

Why is this structure the universal base *v*P?[17] First, it is the most cost-effective structure: the base *v*P is built by external merges only. If the cost is zero, the base *v*P corresponds to the identity (do-nothing) operation, which is the most cost-effective transformation. It is like the identity operation +0 under addition, which does not affect a number (for example, 3 + 0 = 3). Second, it is the most fundamental structure: every sentence structure contains the base *v*P at its deepest structure. Third, it gives us semantic universality: The base *v*P is the minimal domain where the V's inherent semantic information is assigned to *O* and *S*, and this holds universally. Fourth, there is *V*'s affinity for *O*: universally, *V* has an affinity for *O* rather than *S*.[18] Thus, C_{HL} disallows other possibilities.

C-command expresses a balance (equilibrium) between disconnection (i) and connection (ii) in a tree (Chomsky 1995: 339).

[16] TRANSFER (Spell Out) sends a halfway-built tree with sound information to Φ. The relevant derivation may involve movements in later steps. An anonymous reviewer asks an important question in this connection: Is it not the case that <*SOV*>, for example, is always re-derived many times or has many sources? I tentatively assume that the geometrical cost approach mapping a tree to an unmarked word order is compatible with the conception that an unmarked word order (output) derives from many source trees (input) because a function allows many-to-one correspondence (Stewart 1975). For unmarked <*SOV*> and <*SVO*>, let us assume that the c-command relation within the *v*P phase at the point of the first TRANSFER determines the unmarked order.

[17] I thank an anonymous reviewer for pointing out this crucial question. In an earlier draft, I adopted the view that *O* moves to Spec, *v*P for feature checking. The reviewer pointed out that such a *v*P competes in cost with the one in which *V* moves to *v*, that is, both structures have one internal merge. The reviewer's observation has improved the structure of the universal base *v*P; it is constructed by an external merge alone, which yields the simplest possible architecture for *S*, *O*, and *V*.

For phylogeny, the third factor (geometrically lowest cost) determines the six unmarked word orders. But for ontogeny, capitalizing on Yang (2002: 72), who argued that 'irregular'-verb formation is in fact 'regular' in that a child acquires 'irregular' verbs by applying 'regular'-class-forming rules, I propose that a child reliably associates an 'irregular' (minor) order (*OSV, VOS, OVS*) with its matching 'irregular'-formation rule, and reliably apply the rule over the default <*SOV*>. The ontogeny (acquisition) of 'irregular' (minor) unmarked orders parallels that of 'irregular' verbs. See section 3 for a detailed discussion.

[18] There is much evidence which indicates that *V* merges with *O*. *V* selects *O* (e.g., the *V say*

Let us demonstrate how the base *v*P is constructed. Given that each set includes the empty set by definition and that a syntactic object is a set, each syntactic object includes the empty set ∅ (an axiom). *V* externally merges with ∅.[19] *V′* and *O* merge, and *V* assigns Patient θ (a semantic role) to *O*.[20] The light verb *v* merges with VP. The *v′* merges with *S* and *v* assigns Agent θ to *S*. Thus, the base *v*P is the most inexpensive base for building the structure of {*S, O, V*} because it is formed by external merges only, given the Merge-over-Move hypothesis, and so every sentence starts with the base *v*P. Every final structure contains the base *v*P as a subset, and the base *v*P does not affect the usable c-command relations in the final structure. As noted above, the base *v*P is like the identity element 0 (zero) in addition. Probe features in *v* agrees with the goal features in *O*, the relevant structural features are valuated and deleted (Chomsky 2000).[21] The valued

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selects a *that* clause as *O* but the *V kill* does not), *V* forms idioms with *O* (e.g., *kick the bucket*), a transitive verbal noun N^V produces a compound word with *O* (e.g., *manslaughter*), and sequential voicing occurs between *V* and *O* (e.g., compound words in Japanese).

19 An anonymous reviewer points out that construing the empty set as a legitimate syntactic object is something new and that it should be justified. The reviewer points out that it poses a problem because in set theory, the empty set is a subset of every set, not an element of every set. I tentatively adopt the following definition of syntactic object in the bare phrase structure model (Chomsky 1995: 243, 262). I reintroduce the relevant definition stated in Uriagereka (2000: 497).

(i) *Syntactic object*
σ is a syntactic object if it is
a. a lexical item or the set of formal features of a lexical item, or
b. the set K = { γ, { α, β}} or K = {$\underline{<\gamma, \gamma>}$, { α, β}} such that α and β are syntactic objects and γ or $\underline{<\gamma, \gamma>}$ is the label of K.

If the *set* of formal features of a lexical item is a syntactic object as in (ia) and if the phonologically empty set lacking any member (phonological feature) is a legitimate phonological object, the syntactically empty set lacking any member (syntactic feature) may also be a legitimate syntactic object. As an alternative, the reviewer suggests 'Self-Merge' that allows vacuous projection, as in Guimarães (2000) and Kayne (2008). I leave open this fundamental problem for future research. See Barrie (2006: 99–100) for the solution adopted here, which avoids the initial-merge problem (or the "bottom of the phrase-marker" linearization problem; Uriagereka 2012: 141, fn. 23, citing Chomsky 1995: chap. 4). In fact, the structure-building space consists of empty set (∅) before *V* enters, i.e., "take only one thing, call it 'zero,' and you merge it; you get the set containing zero. You do it again, and you get the set containing the set containing zero; that's the successor function" (Chomsky & McGilvray 2012: 15). The operation also satisfies the restriction that "Merge cannot apply to a copy: a trace or an empty category that has moved covertly" (Chomsky 2004). The empty set ∅ is not a copy or an empty category that has moved covertly. Therefore, ∅ is allowed to merge with V. "The empty set is not 'nothing' nor does it fail to exist. It is just as much in existence as any other set. It is its *members* that do not exist. It must not be confused with the number 0: for 0 is a number, whereas ø is a set" (Stewart 1975: 48). "[T]he empty set ∅ is a subset of any set you care to name — by another piece of vacuous reasoning. If it were not a subset of a given set *S*, then there would have to be some element of ∅ which was not an element of *S*. In particular there would have to be an element of ∅. Since ∅ has no elements, this is impossible" (ibid.: 49). See also Fukui (2012b: 259) for the hypothesis that 1 is created by merging 0 with 1. If the natural numbers emerged from the abstraction from merge, the sentence-structure building must involve the empty set merging with V at the first step.

20 An intermediate projection such as *V′* is used for expository purposes.

21 The base *v*P is consistent with the Multiple Spell Out (MSO) hypothesis (which states that there is more than one point when a structure with sound features attached is sent to the PF (Φ) (Uriagereka 2012: 113, fn. 33). According to MSO, a domain, such as *S*, that is moved to

φ-feature is deleted because it is redundant: *O* contains the same *φ*-set in the first place. The valued structural Case is deleted as a reflex (side effect) of valued-*φ* deletion (ibid.: 122). If a formal feature is not deleted within C_{HL} and enters into the external performance systems (Φ and the thought system Σ), the external systems will freeze because such a structural feature is unknown to them.

The base *v*P is the most economical structure (involving the least effort) that satisfies the Linear Correspondence Axiom (LCA; originally proposed by Kayne 1994). LCA is a principle at the sound interface that maps two-dimensional structures to one-dimensional linear orders. LCA demands that a structurally higher term should be pronounced earlier. Let us adopt the following definition of LCA (Uriagereka 2012: 56).[22]

(4) *LCA*: When *x* asymmetrically c-commands *y*, *x* precedes *y*.

The base *v*P does not influence later structures. For example, suppose we arrived at $V \gg S \gg O$ as the final output structure of TRANSFER. In Φ, LCA notices only the boxed terms in Figure 2.[23] There, TRANSFER (Spell Out) sends the final CP structure to Φ, and LCA maps this structure to the linear unmarked order <*VSO*>.[24] Although the final CP structure contains the base *v*P whose syntactic relation is $S \gg O \gg V$, the final structure is not affected by the base *v*P (recall that the base *v*P is like the identity element 0 (zero) for addition).[25]

TP Spec and spelled out independently becomes opaque to subextraction. *O* in the base *v*P remains in situ and is not spelled out independently, and hence, no island effect is detected for *O*. Uriagereka cites Jurka (2010), who maintains that Kayne's (1994) hypothesis that <*SVO*> derives <*SOV*> is dubious: it incorrectly predicts that the moved *O* should exhibit the island effect. The universal base *v*P hypothesis rejects Kayne's (1994) hypothesis that structure building starts with the base VP in which *S* c-commands *V*, which c-commands *O*. See Fukui & Takano (1998) for arguments for our hypothesis.

[22] The original definition of LCA is as follows (Kayne 1994: 6). Given $d(X)$ = the set of terminals *T* that *X* dominates and *A* = the set of ordered pairs $<X_j, Y_j>$ such that for each *j*, X_j asymmetrically c-commands Y_j, where *X* asymmetrically c-commands *Y* iff *X* c-commands *Y* and *Y* does not c-command *X*, *LCA* = def. $d(A)$ is a linear ordering of *T*.

[23] With regard to the V-initial unmarked order, there is a debate on the derivation, i.e. remnant-VP movement vs. V-movement. For arguments for the former view, see Alexiadou & Anagnostopoulou (1998) and Massam (2000). I use a V-movement analysis for simplicity. The choice does not affect the discussion. See Carnie *et al*. (2005) for relevant discussions.

[24] If T contains EPP and attracts *S*, *V* must have reached *C* at the point of the final TRANSFER for the unmarked order <*VSO*> to be realized.

[25] The tree building in C_{HL} constitutes a group. It conforms to the four definitions of a group. First, it is closed: Merge applies to a tree and it creates a tree. Second, it has an identity element: the universal base *v*P is similar to 1 for multiplication; it does not affect the output. Third, it has inverse elements: there is always a set of remerge operations that returns some c-command relation to the base $S \gg O \gg V$ relation. Fourth, it obeys the associative law, (XY)Z = X(YZ), with respect to structure building (head projectionability); given the head-final property, both (XY)Z and X(YZ) produce a projection of Z. Alternatively, given the head-initial property, both (XY)Z and X(YZ) produce a projection of X. With regard to the fourth condition, Fukui & Zushi hold the view that C_{HL} disobeys the associative law for semantics, i.e., distinct hierarchical (binary) structures produce distinct meanings (Merge disobeying the associative law causes the hierarchical structures). See their comment on pages 19 and 322 of the Japanese translation of Chomsky (1982, 2002).

If Merge is the fundamental operation in C_{HL} and the concept of 'group' applies to any system with the possibility of combining two objects to yield another (Stewart 1975: 1), C_{HL} deserves a group-theoretical analysis. "Thus the concept 'group' has applications to

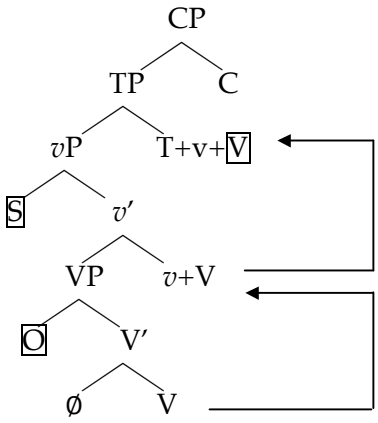

Figure 2: V ≫ S ≫ O c-command relation mapped to unmarked <VSO>

3. Word Order Asymmetry as Geometrical Cost Asymmetry

The symmetry structure of an equilateral triangle represents the group-theoretical structure of a cubic equation (Stewart 2007).[26] The permutation of three solutions corresponds to that of the three vertexes. Assume counterclockwise rotations, with a 0° rotation serving as the identity *I*.[27] Let us call the original triangle as the identity element or identity triangle.

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rigid motions in space, symmetries of geometrical figures, the additive structure of whole numbers, or the deformation of curves in a topological space. The common property is the possibility of combining two objects of a certain kind to yield another" (ibid.).

[26] I thank an anonymous reviewer for clarifying the issue. That is, the permutation group S_3 of three letters have only 4 isomorphism classes (or conjugacy classes) of subgroups, namely, {id} = *I*, C_2 (a cyclic group of order 2), C_3 (a cyclic group of order 3) and S_3. The reviewer criticizes that the observed broken symmetry corresponds most closely to C_2, amounts to a rather simple observation that *V* and *O* seem to remain symmetric whereas *S* is not symmetric with others. Here is the list of six subgroups of S_3. (23) stands for the permutation that switches 2 and 3, leaving 1 intact, as in (1, 2, 3) → (1, 3, 2). (132) stands for the permutation that changes 1 to 3, 3 to 2, and 2 to 1, as in (1, 2, 3) → (3, 1, 2). *I* is the identity permutation that keeps everything intact, as in (1, 2, 3) → (1, 2, 3). Assume 1 = *S*, 2 = *O*, 3 = *V*.

(i) a. {*I*, (23), (13), (12), (132), (123)} = S_3
b. {*I*, (132), (123)} = A_3
c. {*I*, (12)}
d. {*I*, (13)}
e. {*I*, (23)}
f. {*I*}

Every subgroup contains *I*, which is (*S*, *O*, *V*) → (*S*, *O*, *V*). This might partially express the C_{HL} fact that it is the highest probability that the identity transformation maps the universal base *v*P onto <*SOV*> unmarked order.

[27] A 0° and 360° cannot be distinguished group-theoretically, but they are distinct if we take the cost difference into consideration.

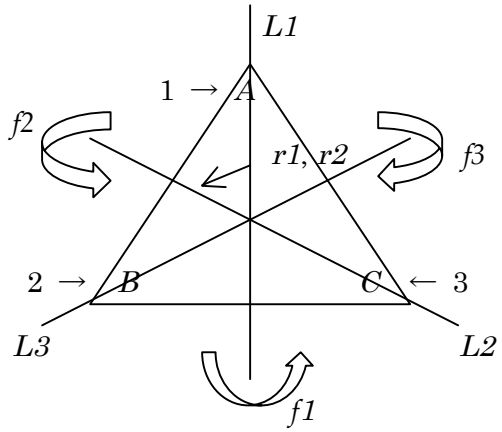

Figure 3: Symmetrical Operations of an Equilateral Triangle

An equilateral triangle has six symmetrical operations: rotations *r* (cyclic permutations) and reflections *f* (flips or non-cyclic permutations) indicated in (5).[28]

(5) a. *r0* = 0° = *I* (do-nothing rotation)
b. *r1* = 120° rotation
c. *r*2 = 240° rotation
d. *f1* = Flip around axis *L1*
e. *f*2 = Flip around axis *L2*
f. *f*3 = Flip around axis *L3*

The do-nothing operation *r0* changes <*ABC*> to <*ABC*>. The top apex corresponds to the first position, the lower left apex to the second, and the lower right apex to the third. The six transformations are as follows:

(6) a. *r0* changes <*ABC*> to <*ABC*>.
b. *r1* changes <*ABC*> to <*CAB*>.
c. *r*2 changes <*ABC*> to <*BCA*>.
d. *f1* changes <*ABC*> to <*ACB*>.
e. *f*2 changes <*ABC*> to <*BAC*>.
f. *f*3 changes <*ABC*> to <*CBA*>.

The transformation *r0* is the most cost-effective. Although Galois groups are indifferent to cost, geometrical operations do have cost differences, given an appropriate cost function. It is true that the structure of the symmetric group S_3 of order 3! (6) is too simple to imply anything. However, this simplicity is the very reason why I take operational costs into consideration.[29] All six symmetrical

[28] Rotations are linear transformations *T* (or function *f*) in R^2 (two-dimensional real-number space). Flips are *T* of R^2 subspace in R^3 (Strang 2009). *T* or *f* can be translated into a matrix *A*. If the unmarked order asymmetry can be expressed by *T*, we will be able to translate it into the matrix language, which we leave for future research.

[29] Algebraic cost means computing time (Strang 2003: 87). An anonymous reviewer offered the criticism that the structure of the symmetric group is too simple to imply anything. I thank

trans- formations can be expressed using only *r0*, *r1*, and *f1*, that is, *r2*, *f2*, and *f3* are derivable operations (Armstrong 1988).[30]

(7) a. *r0*
b. *r1*
c. *r2 = r1 + r1*
d. *f1*
e. *f2 = f1 + r1*
f. *f3 = r1 + f1*

Why do we select *r0*, *r1*, and *f1* as irreducible atoms for symmetrical transformations?[31] Recall that we started from an empirical (physical) fact about the human brain: C_{HL} produces a sentence structure with the base *v*P as its universal base, in which *S*, *O*, and *V* are externally merged such that *S* asymmetrically c-commands *O*, which in turn asymmetrically c-commands *V*. The base *v*P is the most cost-effective base with a cost of 0: it is built by external merges alone. Therefore, the base *v*P corresponds to *r0*, the identity operation (with a cost of 0). Since we use the cost differences between transformations, we have to rank transformations by their geometrical cost. After *r0*, the next most cost-effective operation is *f1*, which switches two (rather than three) positions, *O* and *V*. Because *f1* switches *O* and *V*, which have a strong bond, as stated earlier, and which form a natural class, *f1* is the most cost-effective transformation among flips (or reflections). Following *r0* (cost 0) and *f1* (cost 1), *r1* (with cost 2; it is a single-step rotation with three (rather than two) positions replaced) is the second most cost-effective transformation within the rotations.

Let us summarize cost calculation. Suppose that the identity operation *r0* has cost 0. The geometrical operation *r0* syntactically corresponds to doing nothing to the least costly base *v*P before spell-out, which in turn sent to Φ where LCA produces the linear order <*SOV*>. The more positions that a computation replaces, the more energy the computation uses.[32] This is the reason why *r1* is costlier than *f1*.[33] Furthermore, single-step operations are cheaper than two-step operations — mathematicians call this the 'length function' in symmetric groups.[34] Hence, *r0* is the cheapest of all, *f1* is the second cheapest, and *r1* is the third.[35] Assuming that *f1* has cost 1, *r1* has cost 2, and that addition is used for

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the reviewer for clarifying the crucial reason why I should consider geometrical cost, namely it sharpens the tool for observing the phenomena.

30 I stipulate that the vertical axis *L1* is the default (basis). An empirical reason is as follows. Given that the base *v*P corresponds to an equilateral triangle in which *S* is the top vertex, *O* is on the left, and *V* is on the right, the vertical axis *L1* switches *O* and *V*. There is considerable evidence that *V* has an affinity for *O*, rather than *S*. That is, given, *S*, *O*, and *V*, {*O, V*} constitutes a natural class excluding *S*, whereas {*S, V*} excluding *O* does not. The vertical axis *L1* switches elements in a natural class.

31 I thank an anonymous reviewer for pointing out the necessity of clarifying this reasoning.

32 "[I]n group theory it is the end result that matters, not the route taken to get there" (Stewart 2007: 121). However, the route matters for the geometrical cost approach: A longer route is more expensive.

33 I thank an anonymous reviewer for pointing out unclarity in an earlier draft.

34 I thank an anonymous reviewer for pointing this out.

35 This cost function is consistent with results under the Mobius function, according to which

cost accumulation, the costs for the six transformations are as follows:

(8) $r0 = 0$
$r1 = 2$
$r2 = r1 + r1 = 2 + 2 = 4$
$f1 = 1$
$f2 = f1 + r1 = 1 + 2 = 3$
$f3 = r1 + f1 = 2 + 1 = 3$

The identity operation *r0* is the cheapest (cost 0) followed by *f1* (cost 1) and *r1* (cost 2).[36] This is what we would expect if we replaced *A*, *B*, and *C* with *S*, *O*, and *V*, respectively.[37] The identity triangle looks like the following:

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the equation for flip is simpler than that for rotation.

[36] An anonymous reviewer asks a subtle and extremely important question: Exactly what are the relevant 'costs' to be minimized, provided the economy principle? I adopt the view that algebraic cost means computing time (Strang 2003: 87). The longer the root, the more time it takes. Therefore, the relevant 'cost' to be minimized is computing time. The high probability of unmarked <*SOV*> from phylogenetic point of view emerges from the fact that the identity (do-nothing) transformation is the fastest computation. Also, I thank an anonymous reviewer for pointing out a miscalculation in a previous draft and for clarifying the reason for selecting smaller values. The reasoning is as follows. For *f2*, there are three sets of operations that lead to the same result: $f2 = f1 + r1 = 1 + 2 = 3$, $f2 = r2 + f1 = 4 + 1 = 5$, and $f2 = r1 + f1 + r2 = 2 + 1 + 4 = 7$. For *f3*, there are two sets of operations that lead to the same result: $f3 = r1 + f1 = 2 + 1 = 3$, and $f3 = f1 + r2 = 1 + 4 = 5$. I select the lowest cost for each, assuming that C_{HL} obeys the Economy Principle. Therefore, $f2 = f1 + r1 = 3$, and $f3 = r1 + f1 = 3$.

[37] A reviewer points out that "[these] permutations on the SOV 'basic' string as the relevant group-theoretic action" is "the source of the most severe problems". However, what is 'basic' is not the SOV string itself. What is 'basic and universal' is the *v*P structure without internal merge (copy and remerge) at the point of TRANSFER (movements may occur later). The universal base *v*P *per se* is not the unmarked <*SOV*> order. TRANSFER applies to the universal base *v*P and Φ outputs <*SOV*> as a possible unmarked order for a simple matrix transitive sentence. The reviewer also has severe doubts on "the author's technique of considering string permutations rather than movement operations in the tree." However, I do not propose string permutations as a new technique to analyze sentence structures. Rather, I claim that movement operations in a tree (including no movement) can be expressed as the group-theoretical transformations of equilateral triangle. The movement operations and the geometrical transformations are compatible and translatable. If a certain structure (order) is not derivable due to a violation of the movement constraint, there is no geometrical expression for it. We consider how a per- mitted tree structure can be expressed algebraically and geometrically. The group-theoretic action acts on an equilateral triangle in a certain coordinates (which is a geometrical expression of a particular permutation of three solutions of a cubic equation). A triangle undergoes various linear transformations in R^2 (e.g., rotations in two-dimensional real-number space) and R^3 (e.g., reflections (flips) in three-dimensional space). However, I admit that the geometrical cost approach does rely on the universal base *v*P as the identity element. If that approach is untenable (as the reviewer points out), the geometrical cost approach collapses.

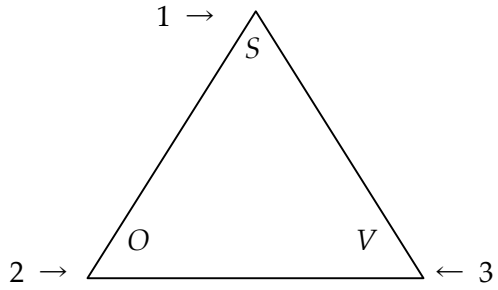

Figure 4: Identity Triangle Expresses the Universal Base v*P*

Internal merge operations including the lack thereof apply to the universal base *v*P, and the LCA produces various unmarked order types in Φ. This situation is geometrically expressed as symmetric transformations applied to the identity triangle, producing various permutations. Table 2 summarizes the transformations and costs.

Transformation	Cost	Input	Output	Ratio
r0	0	<*SOV*>	<*SOV*>	48.5%
r1	2	<*SOV*>	<*VSO*>	9.2%
r2	4	<*SOV*>	<*OVS*>	0.7%
f1	1	<*SOV*>	<*SVO*>	38.7%
f2	3	<*SOV*>	<*OSV*>	0.5%
f3	3	<*SOV*>	<*VOS*>	2.4%

Table 2: Transformations and Costs for {S, O, V}

Following Jenkins (2000, 2003), I speculate that the unmarked word order asymmetry is expressible as a group-theoretical factor (included in Chomsky's third factor): "[W]ord order types would be the (asymmetric) stable solutions of the symmetric still-to-be-discovered 'equations' governing word order distribution". The 'symmetric equation' is a linear transformation $f(x) = y$, where function f (or transformation T) is a set of merge operations that is expressed as a set of symmetric transformations of an equilateral triangle (or permutations of three solutions of a solvable cubic equation), x is the universal base *v*P input that is expressed as the identity triangle, and y is a mapped output tree that is expressed as an output triangle that preserves symmetry. The equation $f(x) = y$ can be translated into the matrix language: $Ax = y$, where A is a matrix that performs the transformation, x is a set of input vectors expressing the identity triangle (the universal base *v*P), and y is a set of output vectors expressing the transformed symmetrical triangle (the transformed tree).[38] The Galois theory and the Economy Principle (choose the cheaper operation) can express the current ratio of languages with the top three unmarked word orders:

[38] See Strang (2009) for the basic idea of linear transformations. The condition that a linear transformation must satisfy is as follows: $T(cv + dw) = cT(v) + dT(w)$, where T is a linear transformation, v and w are some vectors, and c and d are some constants. Projections and rotations are examples of linear transformations.

(9) a. *r0* (cost 0) produces <*SOV*> with a ratio of 48.5%.
b. *f1* (cost 1) produces <*SVO*> with a ratio of 38.7%.
c. *r1* (cost 2) produces <*VSO*> with a ratio of 9.2%.

Although the geometrical cost approach fails to predict the internal ranking among *f2*, *f3*, and *r2*, it does predict their relatively low probability:

(10) a. *f2* (cost 3) produces <*OSV*> with a ratio of 0.5%.
b. *f3* (cost 3) produces <*VOS*> with a ratio of 2.4%.
c. *r2* (cost 4) produces <*OVS*> with a ratio of 0.7%.

The geometrical cost approach predicts that <*OSV*> and <*VOS*> should emerge at the same rate, and that <*OVS*> should exhibit the lowest rate, which is not reflected in the actual statistics. We are not able to predict this difference. However, it is significant that the approach predicts the internal ranking of the major (top) three unmarked word orders and the division between the higher three and lower three with respect to unmarked word order in C_{HL}.[39]

What is symmetry? A state is symmetrical when an operation (or a transformation) does not affect (change) the properties of the state. However, some properties are preserved after transformation (symmetry is formed), whereas some properties are not preserved (symmetry is broken). What properties are preserved and not preserved here? The preserved property is the structure of the equilateral triangle itself located in particular coordinates (the entire shape looks the same after symmetrical transformations); information regarding the locations of *S*, *O*, and *V* is irrelevant. We observe the same-looking equilateral triangle after various symmetrical operations. The property not

[39] With regard to <*OSV*>, I tentatively propose that *O* raises and becomes the Spec, TP. The operation is very expensive because C_{HL} must find (and actually finds) a solution to circumvent a violation of the minimality principle; T has attracted *O*, which is more distant than *S*. With regard to <*OVS*>, V further raises to T. With regard to <*VOS*> (e.g., Austronesian languages such as Malagasy, Seediq, and Tzotzil), V further raises to C. However, the analysis wrongly predicts that the probability difference should be *OSV* > *OVS* > *VOS*. As an anonymous reviewer points out, the currently available difference is unexpectedly the opposite: *VOS* > *OVS* > *OSV*. Why should <*VOS*> be the most probable among the three? It may be that V-movement to C facilitates *O*-movement, as in Object Shift phenomena. As for the conditions on Object Shift, see Chomsky (2000). Alternatively, it may be related to the mathematical fact that "Inverses come in reverse order" (Strang 2003: 72). That is, $(SOV)^{-1} = V^{-1}O^{-1}S^{-1}$. In other words, <*VOS*> could be an inverse of <*SOV*>. Therefore, $(SOV)^{-1} \times (SOV) = (V^{-1}O^{-1}S^{-1}) \times (SOV) = I \times I \times I = I$. <*VOS*> shows relatively high probability because it is in inverse relation with the highest probable order, <*SOV*>. However, neither the exact nature of the derivation nor the linear algebraic reasoning is clear at this point.

Furthermore, a question arises as to why the unmarked word orders <*OSV*>, <*VOS*>, and <*OVS*> exist at all; i.e. why do they not show 0% if they are very expensive? From the perspective of phylogeny, I propose that these unmarked word orders are rare (minor) because they have higher geometrical cost. However, from the perspective of ontogeny, I propose, capitalizing on Yang (2002: 69–70), that they exist because they have higher weight (probability that is one or very close to one as a result of learning). These rare (minor) unmarked orders are like 'irregular' verbs: Every 'irregular'-forming rule, which applies to the verb class, is associated with a weight (probability). As a child acquires 'irregular' verbs by applying 'regular' class-forming rules, she acquires a 'minor' basic word order by applying 'regular' transformation (phrasal and head movement) rules.

preserved is the locational information of *S*, *O*, and *V* regarding where *S*, *O*, and *V* end up in the triangle after symmetrical transformations. We observe different arrangements of *S*, *O*, and *V* after various symmetrical operations. However, the identity (do-nothing) transformation is special in that it always preserves all properties after symmetrical operations.

A derivation of a sentence starts out with the universal base *v*P, in which *S* c-commands *O* and *O* c-commands *V*. If the base *v*P (without movement) is transferred to Φ, we obtain <*SOV*> as the unmarked order. This is geometrically expressed as the identity transformation where nothing is done. If *V* raises to *v* (one-step V-movement) before TRANSFER, we obtain <*SVO*> as the unmarked order. This is geometrically expressed as a flip (three-dimensional transformation) where we have *V* in the base-*O* position and *O* in the base-*V* position (*O* and *V* are switched). If *V* raises to *v* and then to *T* (two-step V-movement) before TRANSFER, we obtain <*VSO*> as the unmarked order. This is geometrically expressed as a 120° rotation, where we have *V* in the base-*S* position, S in the base-*O* position, and *O* in the base-*V* position. The structure-building cost corresponds to the geometrical cost. This causes the probability difference among the three major basic word order types from the phylogenetic viewpoint. Let us summarize the C_{HL} geometry correspondence in the following figures. The boxes in the trees are visible to Φ and to the LCA spelling out the unmarked word order.

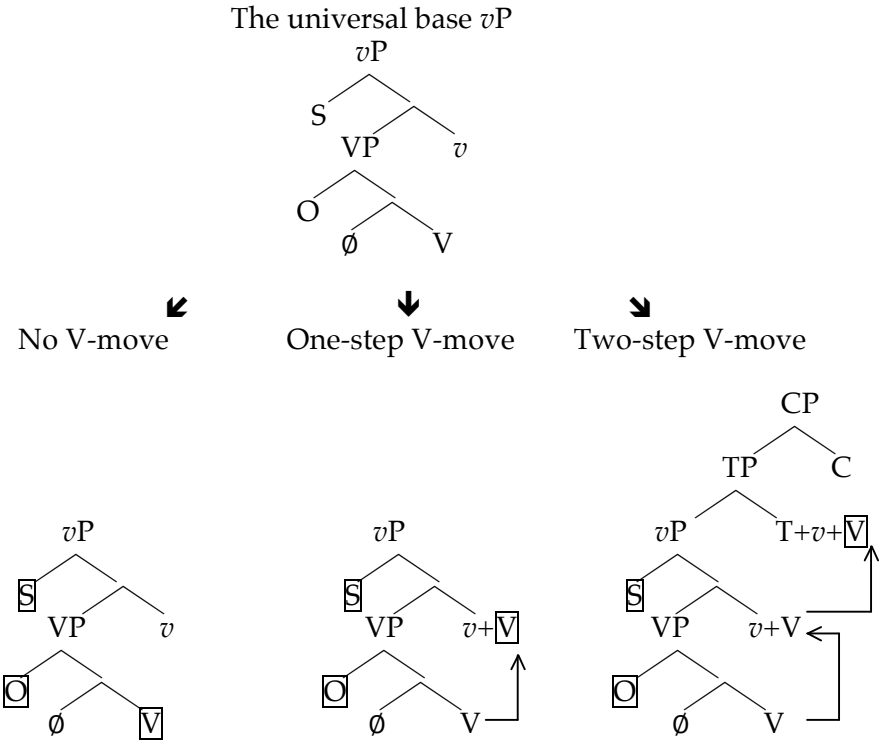

Figure 5: C_{HL} Transformation Deriving the Major Three Unmarked Orders

The above tree-building steps can respectively be expressed as (Galois-theoretic) geometrical transformations (rigid movements) as follows. These geometrical

transformations express various permutations of the solutions of the solvable cubic equation.[40]

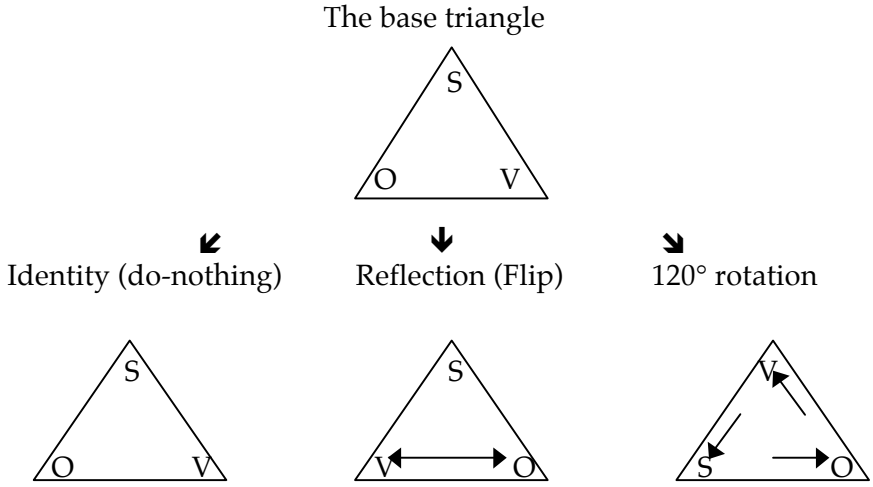

Figure 6: Geometrical Transformations Deriving the Major Three Unmarked Orders

Our analysis is consistent with the conception that "[o]ptimally, linearization should be restricted to the mapping of the object to the SM [sensorimotor] interface [Φ], where it is required for language-external reasons" (Chomsky 2005). The geometrical cost belongs to a mathematical or physical law that is language external. In Addition, our model supports the view that "order does not enter into the generation of the C-I [thought] interface," and that "syntactic determinants of order fall within the phonological component" (Chomsky 2008). In other words, the permutation among *S*, *O*, and *V* does not influence the meaning of the matrix simple transitive sentence in all languages in the thought system: the idea of "John loves Mary" is the same in all languages, whatever the unmarked order is; symmetry is maintained. On the other hand, with regard to the ordering that takes place in Φ, symmetry breaks in a manner that obeys a mathematical or physical law (except the do-nothing (identity) operation). Ordering is not accidental or random, contra Chomsky (2012).

However, it is also a fact that all six unmarked-order types behave alike in that they are all possible mother languages; each type is the most natural, frequent, and unmarked word order for the respective native speakers. The computational cost for basic order formation must be within the permissible level in all types; the relevant computation is equally efficient in all languages.

An anonymous reviewer asks a crucial question: Is it not the case that C_{HL} must produce the unmarked <*SOV*> only, provided that the unmarked <*SOV*> derives from the most efficient computation and that C_{HL} obeys the principle of efficient computation? Why does C_{HL} allow other unmarked orders that derive from less efficient computation? Why does the unmarked <*OVS*> for example exist at all, given that it derives from the least efficient computation? Is it not the case that the unmarked <*OVS*> cannot exist? Why does it exist at all?

[40] It is not clear how the relevant cubic equation looks like at this point.

A tentative answer is as follows. Suppose that the gross computational cost is 1.0 in all languages and that C_{HL} allows all possible patterns as long as the gross cost is 1.0.[41] If the basic (unmarked) word order is <*SOV*>, approximately cost 0.1 is used for the unmarked order building and the rest (cost 0.9) is used for other operations. If the basic word order is <*SVO*>, cost 0.2 is used for the unmarked order building and the rest (cost 0.8) is used for other operations. If the basic word order is <*VSO*>, cost 0.3 is used for the unmarked order building and the rest (cost 0.7) is used for other operations.[42] For example, the <*SOV*> type has the greatest cost 0.9 remaining for other operations. Thus, an <*SOV*>-type language such as Japanese tends to allow computationally more complex operations in other domains: this type allows (phonologically) null subjects, null expletives, null agreement morphologies, covert (phonologically null) wh-movement, covert extraction of argument-wh phrases out of islands, and scrambling (relatively free word ordering).[43] The C_{HL} needs more energy to locate where these silent entities are, how they are moving, and where they went because they are not heard (not pronounced); they are difficult to find and keep track of.[44] Therefore, our model predicts that the <*SVO*> type, unlike the <*SOV*> type, is less tolerant toward these phonetically null entities and word permutations. The prediction is borne out as comparative syntactic studies have observed: an <*SVO*>-type language such as English tends not to allow covert subjects, covert expletives, covert agreement morphologies, covert wh-movement, covert extraction of wh-phrases out of islands, and scrambling. In addition, our analysis predicts that the <*VSO*> type is furthermore less tolerant toward these phenomena.[45] We leave the detailed verification for future research. Let us summarize our point in Table 3.

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41 Notice that the number 1.0 is tentatively used here for maximum level of computational cost, not the probability 1.0 (it must happen).

42 The specific numbers expressing cost do not matter. What matters is the difference.

43 Covert extraction of adjunct-wh phrases out of islands is not allowed even in this type. The computational cost exceeds the threshold level (cost 1.0) at this point.

44 This idea is the opposite of the standard conception that covert entities and operations need less energy because the costly pronunciation is not necessary.

45 Unlike <*SVO*>-type languages such as English, <*VSO*>-type languages such as Irish (exclusively <*VSO*>) and Tagalog tend to show severer restrictions on covert elements and word permutations. For example, they require a phonetically realized question marker at the beginning (or the second position) of the question sentence; V-initial languages have pre-V particles (C?), C has a more elaborate system of phonetic realization with respect to feature combination of [±Q] and [±WH], which restricts cyclic wh-movement (Irish), *wh*-fronting is obligatory (Irish), the patient wh-phrase, but not the agent wh-phrase, is fronted in the matrix simple transitive question (Tagalog) (Aldridge 2002: 394), an argument movement to the left edge is strictly disallowed (Irish), and null subject is more strictly constrained; a pronoun must appear when V takes an analytic form (Irish), and ordering within nominals is more restricted (strictly head-initial), i.e., nouns must precede demonstratives, adjectives, or relative clauses; and inverted order is prohibited in questions. These observations indicate that the <*VSO*> type is much less tolerant toward covert elements and word permutations than the <*SVO*> type. See Carnie *et al*. (2005) for more information.

	<*SOV*>	<*SVO*>	<*VSO*>
Cost of basic order formation	0.1	0.2	0.3
Cost left for other operations	0.9	0.8	0.7
Gross cost used in C_{HL}	1.0	1.0	1.0

Table 3: Cost is balanced

Assume that the gross cost-level for C_{HL} operations is the same in all languages. In addition, assume that the number of parameters is the same in all languages, i.e., the cost for language acquisition is the same. With regard to <*SOV*>, less parameters are fixed for determining the unmarked order, and more parameters must be fixed for other operations. With regard to <*VSO*>, more parameters must be fixed for determining the unmarked order, and less parameters are fixed for other operations. However, the gross cost is the same in all languages. Our analysis is compatible with the conceptions that "[c]omplexities [expensive computation] in one domain of language are balanced by simplicity [inexpensive computation] in another domain", "[a]ll languages are necessarily equally complex [the gross cost is 1.0]", and that "[c]omplexity trades off between the subsystems of language."[46]

4. Conclusion

I am grateful to the anonymous reviewers for teaching me reality: My approach may be too simple, immature, groundless, and without promise, and my research has a long way to go even if it should turn out to be tenable. The reviewers pointed out several faults. First, S_3 is too simple to say anything about general patterns. Second, since one can superficially analyze any permutation phenomenon by means of the group theory, there is no substance to the argument that C_{HL} works group theoretically. Third, the classification based on S, O, and V may be too crude for samples. Fourth, it may be too simple to assume that the derivation of the unmarked <*SOV*>, for example, is done in only one way; there may be many ways to derive the unmarked <*SOV*>. The reviewers advised me to write this speculative paper without claiming to present any scientific findings, at least raise a set of good questions. I hope that this version manages to do that. I hope that my approach will lead to possible future research from the combined perspective of applied mathematics and biolinguistics.

Despite tons of difficulty, let us ask the following question. What would it mean for the geometrical cost approach to express the basic word order asymmetry in C_{HL}? What does it mean to say that the basic word order asymmetry can be expressed as solving a cubic (or complex quadratic, whatever) equation? What does it mean for the categories as *S*, *O*, and *V* to be described as the roots of an equation?[47] Following Noam Chomsky, I speculate that these

[46] Fenk & Fenk (2008), Nematzadeh (2013). See p. 329 in the Japanese translation of Chomsky (1982) for a possible hypothesis that every individual language shows the same cost level.

[47] I thank an anonymous reviewer for pointing out the necessity of asking these questions in order to provide the *raison d'être* for this project. According to the reviewer, in Jenkins' for-

questions lead us to a partial answer to the traditional question that troubled Alfred Russel Wallace (a British naturalist, explorer, geographer, anthropologist, and biologist; 1823–1913), co-author of the evolutionary theory of natural selection, 124 years ago. Chomsky (2005: 16, 2007: 7, 20, 2010: 53) quotes Wallace's puzzlement: The "gigantic development of the mathematical capacity is wholly unexplained by the theory of natural selection, and must be due to some altogether distinct cause," if only because it remained unused.[48] In favor of Leopold Kronecker (a German mathematician; 1823–1891), who said that God (Mother Nature) made integers; all else is the work of man (*Die ganzen Zahlen hat der liebe Gott gemacht, alles andere ist Menschenwerk*), Chomsky states that the theory of natural numbers may have derived from a successor function arising from Merge and that "speculations about the origin of the mathematical capacity as an abstraction from linguistic operations are not unfamiliar."[49] Considering Merge within the context of the evolutionary theory, Chomsky (2007: 7) proposed the following hypothesis:

(11) Mathematical capacity is derived from language.

If so, Wallace's puzzle is partially answered: "Some altogether distinct cause" is an operation in C_{HL}. I speculate the following hypothesis.

(12) A simple matrix transitive sentence consisting of *S*, *O*, and *V* can be expressed as a solvable equation with an algebraic-geometrical structure.

If this is true, we can study C_{HL} with Galois-theoretic tools.[50] As a Galois group characterizes the algebraic (or symmetry-related) structure of an equation, it can also characterize the algebraic (or symmetry-related) structure of a sentence at a relevant level.

Let us summarize the discussion through the key points listed in (13):

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mulation, the idea was that *the word orders themselves* (not the coarse individual categories *S*, *O*, or *V*) are the solutions to the equations governing syntactic structure and that the group theory could shed light on the algebraic properties of those equations. This paper attempts a very preliminary study to see how far we can proceed with permutations of these coarse but sufficiently simple categories.

48 Wallace's (1889: 467) statement is cited in Chomsky (2007: 7). "The significance of such phenomena, however, is far from clear." (Chomsky 2009: 26, 33). See Chomsky & McGilvray (2012: 16) for relevant discussion.

49 Chomsky has stricter view than Kronecker in that C_{HL} is the origin of nutural numbers, not integers. According to Chomsky (2005: 17), the "most restrictive case of Merge applies to a single object, forming a singleton set. Restriction to this case yields the successor function, from which the rest of the theory of natural numbers can be developed in familiar ways."

50 This is a huge 'if'. An anonymous reviewer asks: Could solving *algebraic* equations be such a fundamental logical operation as to explain whatever symmetry that is found in human brain? The reviewer is inclined to answer *no*. But at the same time, the reviewer states that "it is always worthwhile pointing out that every discrete structure in human language deserves group-theoretic analysis," and that "at least it must have value if it encourages future research in this direction."

(13) a. The cost hierarchy among the six geometrical operations that express the six unmarked word orders in C_{HL} is:

r0 < *f1* < *r1* < *f2* = *f3* < *r2*,

where *r0* corresponds to <*SOV*>, *f1* to <*SVO*>, *r1* to <*VSO*>, *f2* to <*OSV*>, *f3* to <*VOS*>, and *r2* to <*OVS*>. The geometrical cost approach predicts the current percentages of languages that have the top three word orders:

<*SOV*> (48.5%), <*SVO*> (38.7%), and <*VSO*> (9.2%).

b. Although this approach fails to predict the internal relative ranking of the lower three basic word orders, it nevertheless predicts a division between the higher three orders (<*SOV*>, <*SVO*>, and <*VSO*>) and the lower three orders (<*VOS*>, <*OSV*>, and <*OVS*>).

c. As Lyle Jenkins suggests, the unmarked word order asymmetry is expressible as a group-theoretical factor (included in Chomsky's third factor): "word order types would be the (asymmetric) stable solutions of the symmetric still-to-be-discovered 'equations' governing word order distribution." The "symmetric equation" is a linear function (transformation) $f(x) = y$, where the mapping function f consists of various internal merge operations that are expressed as various symmetric transformations (rigid movements) of an equilateral triangle, the input x is the universal base vP that is expressed as the identity triangle, and y is the respective output tree that is expressed as the output triangle that preserves symmetry after transformation.

d. The gross computational cost is the same in all languages. The more energy the system uses for the basic word order formation, the less energy is left for other operations (the law of conservation of energy). Our model predicts that the <*SOV*> type is the most tolerant toward phonetically null entities and operations in which C_{HL} needs more energy to locate them and keep track of the result, the <*SVO*> type less tolerant, and the <*VSO*> type still less tolerant.

e. The unmarked ordering asymmetry obeys a physical law that has algebraic and geometrical expressions. Ordering is not accidental, contra Chomsky (2012).

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Biolinguistics and Platonism: Contradictory or Consilient?

Jeffrey Watumull

It has been argued that language is a Platonic object, and therefore that a biolinguistic ontology is incoherent. In particular, the notion of language as a system of discrete infinity has been argued to be inconsistent with the assumption of a physical (finite) basis for language. These arguments are flawed. Here I demonstrate that biolinguistics and mathematical Platonism are not mutually exclusive and contradictory, but in fact mutually reinforcing and consilient in a coherent and compelling philosophy of language. This consilience is effected by Turing's proof of the coherency of a finitary procedure generative of infinite sets.

Keywords: biolinguistics; discrete infinity; ontology; Platonism; Turing machine

1 Introduction

In "The Incoherence of Chomsky's 'biolinguistic' Ontology" (Postal 2009), Postal attacks biolinguistics as "junk linguistics" (p. 121) with an "awful" (p. 114) ontology expounded in "gibberish" (p. 118), the "persuasive force of [which] has been achieved only via a mixture of intellectual and scholarly corruption" (p. 104), whereas writings espousing Postal's ontology "manifest substance and quality of argument at an incomparably higher intellectual level than [Chomsky's]" (p. 105). As a proponent of biolinguistics, I am tempted to reply in kind to such invective, but to do so would be bad form and bad science. A fallacy free and dispassionate — if disputatious — rebuttal is necessary and proper.

For Postal, language is a Platonic object, and therefore he concludes that the biolinguistic assumption of a physical basis for language is "*absurd*" (p. 104). To the contrary, I shall show Postal's conclusion to be a *non sequitur*.

By engaging in this argument, I fully expect Postal to accuse me of having "chosen to defend something [i.e., biolinguistics] its own author [i.e., Chomsky] is unwilling to" (p. 105), from which two conclusions necessarily follow *in Postal's mind*: (i) I am a living testament to Chomsky's "intellectual and scholarly corruption" of the youth; and (ii) "By exercising his undeniable right of silence here, Chomsky leaves unimpeded the inference that he has not attempted a refutation because he cannot" (p. 105). It goes without saying that I reject these conclusions and the premise from which they do *not* follow. (Incidentally, (i) corrupting the

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young has noble precedents (e.g., a case from 399 BCE comes to mind) and (ii) the argumentum a silentio is a classic(al) fallacy.)

This is not an apologia for Chomsky. Biolinguistics has no single author: It is a research program pursued by numerous individually-thinking scientists subordinate to no individual however foundational, august, and influential. Moreover, the theoretical and empirical contributions of the diverse subprograms in which these scientists work are so numerous and important that none can be "dominant" (Postal 2009: 104): for example, in the intersection of cognitive science, linguistics, and the formal sciences, the formal properties and functional architecture of linguistic cognition are being specified; evolutionary biology is investigating possible homologues/analogues of language in nonhuman animals; genetics is discovering some of the genes active in the development and operation of the language faculty; neuroscience is mapping the physical substrate of linguistic processing; and this is but a sampling of the biolinguistics program to "reinstate the concept of the biological basis of language capacities" (Lenneberg 1967: viii).

The subprogram I work in, call it Mathematical Biolinguistics, is so theoretically and empirically eclectic that I am naturally interested in its ontology. It therefore cannot be "odd for [Postal's] opposite in the present exchange to be anyone other than Chomsky" (Postal 2009: 105).

In the next section, I very briefly and very informally define the biolinguistics Postal impugns. The third section is a rehearsal of Postal's arguments for linguistic Platonism and *ipso facto* (so he assumes) against biolinguistics; in particular, it is argued that the notion of language as a system of discrete infinity is inconsistent with an ontological commitment to language as a neurobiological (finite) system of cognitive computation. I proceed in the fourth section to analyze some of the flaws in these arguments, demonstrating that the ontologies of Platonism and biolinguistics — properly defined — are not mutually exclusive and contradictory, but in fact mutually reinforcing and consilient in a coherent and compelling philosophy of language. This consilience is effected by Turing's proof of the coherency of a finitary procedure generative of infinite sets.

I must add that my work and the ontology it assumes are not representative of all biolinguistic research. Many would accept my thesis that, just as engineers have encoded abstract software into concrete hardware, evolution has encoded into the neurobiology of *Homo sapiens sapiens* a formal system (computable functions) generative of an infinite set of linguistic expressions, modulo my understanding of the (un-encoded) formal system as a Platonic object. Nor is mine the only coherent interpretation of biolinguistics. So it must not be thought that someone with my philosophy is the only possible opposite [to Postal] in the present exchange.

2 Biolinguistics

Let the ontology of some research program be defined as 'biolinguistic' if it assumes, investigates, and is informed by the biological basis of language — a definition subsuming many productive programs of research in the formal and natural sciences. But so general a definition cannot adjudicate the case with Postal. At issue here is the particular definition of biolinguistics that identifies language as *I-language* — i.e., a computational system (a function in *intension*) *internal* to the

cognitive-neurobiological architecture of an *individual* of the species *Homo sapiens sapiens* — the properties of which are determined by the three factors that enter into the design of any biological system: genetics, external stimuli, and laws of nature.

That Chomsky invented the term *I-language* and has expatiated on the three factors does not render him the "author" (Postal 2009: 105) of biolinguistics — that would be a category error analogous to attributing "authorship" of evolutionary biology to Darwin given his invention of the term *natural selection* and expatiation on the factors entering into common descent with modification. Biolinguistics and evolutionary biology are research programs to investigate objects and processes of nature. Thus the only author of I-language is nature. And thus anyone is free to recognize the ontology of biolinguistics as here defined.

3 Platonist Ontology

The incoherence of the biolinguistic ontology is claimed to derive from the fact that "*there cannot be such a thing*" (Postal 2009: 105) as biolinguistics, which assumes that "a mentally represented grammar and [the language-specific genetic endowment] UG are real objects, part of the physical world, where we understand mental states and representations to be physically encoded in some manner [in the brain]. Statements about particular grammars or about UG are true or false statements about steady states attained or the initial state (assumed fixed for the species), each of which is a definite real-world object, situated in space-time and entering into causal relations" (Chomsky 1983: 156–157). To Postal, this ontology is as "*absurd*" as a "biomathematics" or a "biologic," for "[w]ere mathematics biological, brain research might resolve such questions as whether *Goldbach's Conjecture* is true. Were logic biological, one might seek grants to study the biological basis of the validity of *Modus Ponens*. The ludicrous character of such potential research is a measure of the folly of the idea that these fields study biological things" (Postal 2009: 104, 105).

By analogy, Postal argues that the objects of linguistic inquiry are not physical (*a fortiori* not biological), but rather "like numbers, propositions, etc. are abstract objects, hence things not located in space and time, indeed not located *anywhere*. They are also things which cannot be created or destroyed, which cannot cause or be caused. [Natural languages] are collections of other abstract objects normally called *sentences*, each of which is a set" (Postal 2009: 105).

In the paper under consideration, Postal does not expound this ontology; a "brief exposition of its essence" (Postal 2009: 106) suffices for his and my purposes. Essential to the ontology — a form of linguistic Platonism — are the type/token distinction and discrete infinity.

3.1 Types/Tokens

> ES IST DER GEIST DER SICH DEN KRPER BAUT: [S]uch is the nine word inscription on a Harvard museum. The count is nine because we count *der* both times; we are counting concrete physical objects, nine in a row. When on the other hand statistics are compiled regarding students' vocabularies, a firm line is drawn at repetitions; no cheating. Such are

> two contrasting senses in which we use the word *word*. A word in the second sense is not a physical object, not a dribble of ink or an incision in granite, but an abstract object. In the second sense of the word *word* it is not two words *der* that turn up in the inscription, but one word *der* that gets inscribed twice. Words in the first sense have come to be called tokens; words in the second sense are called types.
>
> (Quine 1987: 216–217)

The distinction applies to sentences: For instance, in the classic story by Dr. Seuss, there exist (by my quick count) six tokens of the one type *I do not like green eggs and ham*. Postal defines sentence tokens and types as the objects of inquiry for biolinguistics and linguistic Platonism, respectively. For biolinguistics, *as Postal understands it*, a sentence is nothing more than a "*brain-internal token*" (Postal 2009: 107) — a mental representation. Such an object is defined by spatiotemporal (neurobiological) coordinates with causes (cognitive, chemical, etc.) and effects (e.g., in reasoning and communication). For linguistic Platonism, *as Postal understands it*, this physical object is (if anything) a token of an abstract type, with only the latter being 'really' real. Empirically, "island constraints, conditions on parasitic gaps, binding issues, negative polarity items, etc." obtain not of physical objects *per se*, but of abstractions: "Where is the French sentence *Ça signifie quoi?* — is it in France, the French Consulate in New York, President Sarkozy's brain? When did it begin, when will it end? What is it made of physically? What is its mass, its atomic structure? Is it subject to gravity? Such questions are nonsensical because they advance the false presumption that sentences are physical objects" (Postal 2009: 107). For Postal, this nonsense is nonfinite.

3.2 Discrete Infinity

"[T]he most elementary property of language — and an unusual one in the biological world — is that it is a system of discrete infinity consisting of hierarchically organized objects" (Chomsky 2008: 137). "Any such system is based on a primitive operation that takes n objects already constructed, and constructs from them a new object: in the simplest case, the set of these n objects" (Chomsky 2005: 11). "Call [the operation] *Merge*. Operating without bounds, Merge yields a discrete infinity of structured expressions" (Chomsky 2007: 5). Postal invokes the type/token distinction in his critique of this biolinguistic conception of discrete infinity. He assumes that any object constructed by a physical system must be physical: "Consider a liver and its production of bile, a heart and its production of pulses of blood; all physical and obviously finite. And so it must be with any cerebral physical production" (Postal 2009: 109). Thus if language is a physical (neurobiological) system, then its productions (sentences) must be physical (neurobiological tokens). But physical objects are by definition bounded by the finiteness of spatiotemporal and operational resources: "There is for Chomsky thus no coherent interpretation of the collection of brain-based expressions being infinite, since each would take time and energy to construct, [...] store, process, or whatever[...]; they have to be some kind of *tokens*" (Postal 2009: 109, 111).

More abstractly, a discretely (denumerably) infinite set is one with expressions (members) that can be related one-to-one with the expressions of one of its

subsets (and with the natural numbers). But if language is a neurobiological system, hence finite, then obviously it cannot contain or construct a set that can be related to the (countable) infinity of natural numbers: "[E]very physical production takes time, energy, etc. and an infinite number of them requires that the physical universe be infinite and, internal to Chomsky's assumptions, that the brain be" (Postal 2009: 111). *Reductio ad absurdum*, supposedly.

If biolinguistics implies that expressions are bounded by the spatiotemporal and operational resources of neurobiology, then the (infinite) majority of expressions contained in the discrete infinity are generable only in principle: there exist infinitely many more possible sentences than can ever be generated in the physical universe. So for the biolinguistic system to be defined as discretely infinite, it must be defined as an idealization: a system abstracted away from the spatiotemporal and operational resources of neurobiology. In other words, the biolinguistic system is discretely infinite only if abstracted from biology. And this, Postal concludes, is the fundamental fallacy:

> [If] the biological [Merge function] 'ideally' generates an infinite collection, most of the 'expressions' in the collection cannot be physical objects, not even ones in some future, and the [natural language] cannot be one either. [A]lmost all sentences are too complex and too numerous [...] to be given a physical interpretation[...]. In effect, a distinction is made between real sentences and merely 'possible' ones, although this 'possibility' is unactualizable *ever* in the physical universe. According to the biological view, [...] the supposedly 'possible' sentences are, absurdly, actually biologically impossible. Thus internal to this 'defense' of Chomsky's biolinguistic view, the overwhelming majority of sentences *cannot be assigned any reality whatever internal to the supposed governing ontology*. This means the ontology can only claim [natural language] is infinite because, incoherently, it is counting things the ontology cannot recognize as real. (Postal 2009: 111)

If, however, tokens as physical objects can implement abstract types, then presumably a recursive rule — a finite type — could be tokenized as a procedure in the mind/brain. This Postal concedes: Although "nothing physical is a rule or recursive," because recursive rules are Platonic, a "physical structure can encode rules" (Postal 2009: 110). Presumably, therefore, Merge — the mentally-represented/neurobiologically-implemented recursive procedure posited in biolinguistics to generate discrete infinity — is a legitimate posit. Postal dissents: "[A]n interpretation of physical things as representing particular abstractions [is] something Chomsky's explicit brain ontology has no place for" (Postal 2009: 110). Furthermore, Merge generates sets, and sets are Platonic abstractions, but as "an aspect of the spatiotemporal world, [Merge] cannot 'generate' an abstract object like a set" (Postal 2009: 114). So Merge is either biological — not mathematical and hence incapable of generating a set (let alone an infinite one) — or it is mathematical — hence non-biological but capable of generating discrete infinity. In sum, language is either physical *or* it is Platonic, and only under the latter definition can it be predicated of that "most elementary property," discrete infinity — or so Postal maintains.

4 Mathematical Biolinguistic Ontology

Let me affirm at the outset my commitment to *mathematical* Platonism, which informs my biolinguistic ontology in ways to be discussed. More strongly than Chomsky, who does grant mathematical Platonism "a certain initial plausibility," I am convinced of the existence of "a Platonic heaven [of] arithmetic and [...] set theory," *inter alia*, that "the truths of arithmetic are what they are, independent of any facts of individual psychology, and we seem to discover these truths somewhat in the way that we discover facts about the physical world" (Chomsky 1986: 33). It follows from this position that I must be committed to *linguistic* Platonism for any linguistic objects reducible to or properly characterized as mathematical objects. And indeed in my theory of natural language, the quiddities that define a system as linguistic are ultimately mathematical in nature. (The 'essence' of language, if you will, is mathematical — a proposition I shall not defend here, assuming it to be essentially correct, for at issue in this discussion is not whether the proposition is true, but whether it is consistent with a biolinguistic ontology if true.)

4.1 Overlapping Magisteria

It has been convincingly argued (see, e.g., Hauser, Chomsky, Fitch 2002; Watumull, Hauser, Berwick 2013) that a recursive function generative of structured sets of expressions is central to natural language; this function is defined in intension as internal to the mind/brain of an individual of the species *Homo sapiens sapiens*. So conceived, I- language has mathematical *and* biological aspects.

Of course to Postal this is nonsense: The ontologies of mathematics and biology are non-overlapping *magisteria*. Assuming mathematical Platonism, I concur that a mathematical object *per se* such as a recursive function (the type) is not physical. However, even Postal (2009: 110) concedes that such an object can be physically encoded (as a token). The rules of arithmetic for instance are *multiply realizable*, from the analog abacus to the digital computer to the brain; *mutatis mutandis* for other functions, sets, etc. And *mutatis mutandis* for abstract objects definable as mathematical at the proper level of analysis, such as a computer program:

> You know that if your computer beats you at chess, it is really the *program* that has beaten you, not the silicon atoms or the computer as such. The abstract program is instantiated physically as a high-level behaviour of vast numbers of atoms, but the *explanation* of why it has beaten you cannot be expressed without also referring to the program in its own right. That program has also been instantiated, unchanged, in a long chain of different physical substrates, including neurons in the brains of the programmers and radio waves when you downloaded the program via wireless networking, and finally as states of long- and short-term memory banks in your computer. The specifics of that chain of instantiations may be relevant to explaining how the program reached you, but it is irrelevant to why it beat you: there, the content of the knowledge (in it, and in you) is the whole story. That story is an explanation that refers ineluctably to abstractions; and therefore those abstractions exist, and really do affect physical objects in the way required

by the explanation.

(Deutsch 2011: 114–115)

(Though I shall not rehearse the argument here, I am convinced by Gold (2006) that "mathematical objects may be abstract, but they're NOT [necessarily] acausal" because they can be essential to — ineliminable from — causal explanations. The potential implications of this thesis for linguistic Platonism are not uninteresting.)

I take the multiple realizability of a computer program to evidence the reality of abstractions as well as anything can (and I assume Postal would agree): Something "substrate neutral" (Dennett 1995) is held constant across multiple media. That something I submit is a Turing machine (computable function): the mathematical object representing the formal properties and functions definitional of — and hence universal to — computational systems.

4.2 The Linguistic Turing Machine

Within mathematical biolinguistics, it has been argued that I-language is a form of Turing machine (see Watumull 2012; Watumull, *et al.* 2013), even by those Postal diagnoses as allergic to such abstractions:

> [E]ven though we have a finite brain, that brain is really more like the control unit for an infinite computer. That is, a finite automaton is limited strictly to its own memory capacity, and we are not. We are like a Turing machine in the sense that although we have a finite control unit for a brain, nevertheless we can use indefinite amounts of memory that are given to us externally[, say on a "tape,"] to perform more and more complex computations[...]. We do not have to learn anything new to extend our capacities in this way.
>
> (Chomsky 2004: 41–42)

As Postal would observe, this "involves an interpretation of physical things as representing particular abstractions," which he concedes is coherent in general because "physical structure can encode rules" and other abstract objects (e.g., recursive functions) (Postal 2009: 110) — computer programs, I should say, are a case in point. However he does not accept the interpretation in this particular case.

4.3 Idealization

Postal (2012: 18) has dismissed discussion of a linguistic Turing machine as "confus[ing] an ideal machine[...], an abstract object, with a machine, the human brain, every aspect of which is physical." I-language qua Turing machine is obviously an idealization, with its unbounded running "time" (i.e., number of steps) and access to unbounded memory, enabling unbounded computation. And obviously "[unboundedness] denotes something physically counterfactual as far as brains and computers are concerned. Similarly, the claim 'we can go on indefinitely' [...] is subordinated to the counterfactual 'if we just have more and more time.' Alas, we do not, so we can't go on indefinitely" (Postal 2012: 18). Alas, it is Postal who is confused.

4.3.1 *Indefinite Computation*

Postal's first confusion is particular to the idealization of indefinite computation. Consider arithmetic. My brain (and presumably Postal's) and my computer can encode a program (call it ADD) that determines functions of the form $f_{\text{ADD}}(X+Y) = Z$ (but not W) over an infinite range. Analogously, my brain (and Postal's) but not (yet) my computer encodes a program (call it MERGE) that determines functions of the form $f_{\text{MERGE}}(\alpha, \beta) = \{\alpha, \beta\}$ — with the syntactic structure $\{\alpha, \beta\}$ assigned determinate conceptual-intentional and sensory-motor representations — over an infinite range. These programs are of course limited in performance by spatiotemporal and operational resources, but the programs themselves — the functions in intension — retain their deterministic form even as physical constraints vary (e.g., ADD determines that $2 + 2 = 4$ independent of performance resources).

Assuming a mathematical biolinguistic ontology, I-language is a cognitive-neurobiological token of an abstract type; it "generates" sets in the way axioms "generate" theorems. As the mathematician Gregory Chaitin observes, "theorems are *compressed* into the axioms" so that "I think of axioms as a computer program for generating all theorems" (Chaitin 2005: 65). Consider how a computer program explicitly representing the Euclidean axioms encodes only a finite number of bits; it does not — indeed cannot — encode the infinite number of bits that could be derived from the postulates, but it would be obtuse to deny that such an infinity is implicit (compressed) in the explicit axioms. Likewise, $z_{n+1} = z_n^2 + c$ defines the Mandelbrot set (as I-language defines the set of linguistic expressions) so that the infinite complexity of the latter really is implicitly represented in the explicitly finite simplicity of the former.

So while it is true that physically we cannot perform indefinite computation, we are endowed physically with a competence that *does* generate a set that *could* be produced by indefinite computation. (A subtle spin on the notion of competence perhaps more acceptable to Postal defines it as "the ability to handle arbitrary new cases when they arise" such that "infinite knowledge" defines an "open-ended response capability" (Tabor 2009: 162).) Postal must concede the mathematical truth that linguistic competence, formalized as a function in intension, *does* indeed generate an infinite set. However, he could contest my *could* as introducing a hypothetical that guts biolinguistics of any biological substance, but that would be unwise.

Language is a complex phenomenon: we can investigate its computational (mathematical) properties independent of its biological aspects just as legitimately as we can investigate its biological properties independent of its social aspects (with no pretense to be carving language at its ontological joints). In each domain, laws — or, at minimum, robust generalizations — license counterfactuals (as is well understood in the philosophy of science). In discussing indefinite computation, counterfactuals are licensed by the laws expounded in computability theory:

> [T]he question whether a function is effectively computable hinges solely on the behavior of that function in neighborhoods of infinity[...]. The class of effectively computable functions is obtained in the ideal case where all of the practical restrictions on running time and memory space are removed. Thus the class is a theoretical upper bound on what can ever in any century be considered computable.

(Enderton 1977: 530)

A theory of linguistic competence establishes an "upper bound," or rather delineates the boundary conditions, on what can ever be considered a linguistic pattern (e.g., a grammatical sentence). Some of those patterns extend into "neighborhoods of infinity" by the iteration of the recursive Merge function. Tautologically, those neighborhoods are physically inaccessible, but that is irrelevant. What is important is *the mathematical induction from finite to infinite*: Merge applies to any two objects to form a set containing those two objects such that its application can be bounded only by stipulation. In fact a recursive function such as Merge characterizes the "iterative conception of a set," with sets of discrete objects "recursively generated at each stage," such that "the way sets are inductively generated" is formally equivalent to "the way the natural numbers [...] are inductively generated" (Boolos 1971: 223).

The natural numbers are subsumed in the computable numbers, "the real numbers whose expressions as a decimal are calculable by finite means" (Turing 1936: 230). (The phrase "finite means" should strike a chord with many language scientists.) It was by defining the computable numbers as those determinable by his mathematical machine that *Turing proved the coherency of a finitary procedure generative of an infinite set*.

> For instance, there would be a machine to calculate the decimal expansion of π[...]. π being an infinite decimal, the work of the machine would never end, and it would need an unlimited amount of working space on its 'tape'. But it would arrive at every decimal place in *some* finite time, having used only a finite quantity of tape. And everything about the process could be defined by a finite table[...]. This meant that [Turing] had a way of representing a number like π, an infinite decimal, by a *finite* table. The same would be true of the square root of three, or the logarithm of seven — or any other number defined by some rule.
>
> (Hodges 1983: 100)

Though they have not been sufficiently explicitly acknowledged as such, Turing's concepts are foundational to the biolinguistic program. I-language is "a way of representing [an infinite set] by a finite table" (a function). With the set of linguistic expressions being infinite, "the work of the machine would never end," but Postal must concede that nevertheless I-language "would arrive at every [sentence] in some finite time, having used only a finite quantity of tape. And everything about the process could be defined by a finite table," and thereby programmable into a physical mechanism. (This gives a rigorous sense to the rationalist-romantic intuition of language as the "infinite use of finite means.")

5 Generation and Explanation

But for all the foregoing, the finitude/infinitude distinction is not so fundamental given the fact that "[a] formal system can simply be defined to be any mechanical procedure for producing formulas" (Gödel 1934: 370). The infinitude of the set of

expressions generated is not as fundamental as the finitude of I-language (the generative function) for the following reason: it is only because the function is finite that it can enumerate the elements of the set (infinite or not); and such a compact function could be — and *ex hypothesi* is — neurobiologically encoded. Even assuming Postal's ontology, in which "[natural languages] are collections of [...] abstract objects" (Postal 2009: 105), membership in these collections is granted (and thereby constrained) by the finitary procedure, for not just any (abstract) object qualifies. In order for an object to be classified as linguistic, it must be generated by I-language; in other words, to be a linguistic object is to be generated by I-language. And thus I-language *explains* why a given natural language contains as members the expressions it does.

This notion of *I-language as explanation* generalizes to the notion of *formal system as scientific theory*:

> I think of a scientific theory as a binary computer program for calculating observations, which are also written in binary. And you have a law of nature if there is compression, if the experimental data is compressed into a computer program that has a smaller number of bits than are in the data that it explains. The greater the degree of compression, the better the law, the more you understand the data. But if the experimental data cannot be compressed, if the smallest program for calculating it is just as large as it is [...], then the data is lawless, unstructured, patternless, not amenable to scientific study, incomprehensible. In a word, random, irreducible.
>
> (Chaitin 2005: 64)

This notion is particularly important, as Turing (1954: 592) observed, "[w]hen the number is infinite, or in some way not yet completed [...]," as with the discrete infinity (unboundedness) of language, "a list of answers will not suffice. Some kind of rule or systematic procedure must be given." Otherwise the list is arbitrary and unconstrained. So for linguistics, in reply to the question "Why does the infinite natural language **L** contain the expressions it does?" we answer "Because it is generated by the finite I- language f." Thus I-language can be conceived of as the theory explicative of linguistic data because it is the mechanism (Turing machine) generative thereof.

Second, with respect to idealization generally, for mathematical biolinguistics to have defined I-language as a Turing machine is not to have confused the physical with the abstract, but rather to have abstracted away from the contingencies of the physical, and thereby discovered the mathematical constants that (on my theory) must of necessity be implemented for any system — here biological — to be linguistic. This abstraction from the physical is part and parcel of the methodology and, more importantly, *the metaphysics* of normal science, which proceeds by the "making of abstract mathematical models of the universe to which at least the physicists give a higher degree of reality than they accord the ordinary world of sensation" (Weinberg 1976: 28). The idealization is the way things *really* are. Consider Euclidean objects (e.g., dimensionless points, breadthless lines, perfect circles, and the like). These objects do not exist in the physical world. The points,

lines, and circles drawn by geometers are but imperfect approximations of abstract Forms — the objects *in themselves* — which constitute the ontology of geometry. For instance, the theorem that a tangent to a circle intersects the circle at a single point is true only of the idealized objects; in any concrete representation, the intersection of the line with the circle cannot be a point in the technical sense as "that which has no part," for there will always be some overlap. As Plato understood (*Republic* VI: 510d), physical reality is an intransparent and inconstant surface deep beneath which exist the pellucid and perfect constants of reality, formal in nature:

> [A]lthough [geometers] use visible figures and make claims about them, their thought isn't directed to them but to the originals of which these figures are images. They make their claims for the sake of the Square itself and the Diagonal itself, not the particular square and diagonal they draw; and so on in all cases. These figures that they make and draw, of which shadows and reflections in water are images, they now in turn use as images, in seeking to behold those realities — the things in themselves — that one cannot comprehend except by means of thought.

Analogously, any particular I-language (implemented in a particular mind / brain) is an imperfect representation of a form (or Form) of Turing machine. *But*, Postal would object, the linguistic Turing machine is Platonic, hence non-biological, and hence *bio*-linguistics is contradictory. *But*, I should rebut, this objection is a *non sequitur*.

I am assuming that fundamentally a system is linguistic in virtue of mathematical (non-biological) aspects. Nevertheless, in our universe the only implementations of these mathematical aspects yet discovered (or devised) are biological; indeed the existence of these mathematical systems is known to us only by their biological manifestations — *i.e., in our linguistic brains and behaviors* — which is reason enough to pursue *bio*-linguistics. To borrow some rhetorical equipment, biology is the ladder we climb to the "Platonic heaven" of linguistic Forms, though it would be scientific suicide to throw the ladder away once up it. That chance and necessity — biological evolution and mathematical Form — have converged to form I-language is an astonishing fact in need of scientific explanation. It is a fact that one biological system (i.e., the human brain) has encoded within it and/or has access to Platonic objects. (Postal must assume that our finite brains can access an infinite set of Platonic sentences. The ontological status of the latter is not obvious to me, but obviously I am committed to the existence of the encoding within the former of a finite Platonic function for unbounded computation.) Surely a research program formulated to investigate this encoding/access is not perforce incoherent.

I do, however, deny any implication here that such complex cognition, "in some most mysterious manner, springs *only* from the organic chemistry, or perhaps the quantum mechanics, of processes that take place in carbon-based biological brains. [I] have no patience with this parochial, bio-chauvinistic view[:] the key is not the *stuff* out of which brains are made, but the *patterns* that can come to exist inside the stuff of a brain" (Hofstadter 1999 [1979]: P-4, P-3). Thus, as with chess patterns in "stuff" of a computer, it is not by necessity that linguistic patterns spring from the stuff of the brain; but the fact remains that they can and do. And thus

linguistics is just as much a biological science as it is a formal science. (Likewise, computer science is as grounded in engineering as it is in mathematics.)

To reiterate, at present there exists no procedure other than human intuition to *decide* the set of linguistic patterns. The neurobiology cannot answer the question whether some pattern is linguistic (e.g., whether some sentence is grammatical), but it encodes the procedure that enables the human to intuit the answer to such a question. Analogously, neurobiological research would not establish the truth of Goldbach's conjecture or the validity of reasoning by modus ponens, but rather would be unified with research in cognitive science to establish (discover) the rules and representations encoded neurobiologically that enable cognitive conjecture and reasoning.

Moreover, the biological/linguistic distinction is arguably ill-formed if we assume as it is reasonable to do that some linguistic theories *just are* biological theories. Examples abound:

> [Sapir] was looking at the phonetic data from a certain American Indian language and was able to show that, if he assumed a certain abstract phonological structure with rules of various kinds, he could account for properties of these data. He could explain some of the facts of the language. That investigation in itself was an investigation of psychological reality in the only meaningful sense of the term. That is, he was showing that if we take his phonological theory to be a theory about the mind — that is, if we adopt the standard 'realist' assumptions of the natural sciences — then we conclude that in proposing this phonological theory he was saying something about the mental organization of the speakers of the language, namely that their knowledge and use of their language involved certain types of mental representations and not others — ultimately, certain physical structures and processes and not others differently characterized. (Chomsky 1983b: 44)

Postal believes that an "explicit brain ontology" as assumed in biolinguistics "has no place for" the encoding of an abstract object such as a Turing machine in a physical system such as the brain — but I see *no grounds whatsoever* for this belief. Not only is this belief contradicted by Chomsky's (2004: 41–42) Turing machine analogy, but Postal himself quotes Chomsky discussing how in biolinguistics "we understand mental states and representations to be physically encoded in some manner" (1983: 156–157); and to physically encode something presumes a non-physical something to be so encoded. For this reason "it is the mentalistic studies that will ultimately be of greatest value for the investigation of neurophysiological mechanisms, since they alone are concerned with determining abstractly the properties that such mechanisms must exhibit and the functions they must perform" (Chomsky 1965: 193).

It is in this sense of neurobiology encoding mathematical properties and functions that, "astonishingly" (Postal 2012: 23), we observe the most trivially obvious of facts:

> We don't have sets in our heads. So you have to know that when we develop a theory about our thinking, about our computation, internal processing and so on in terms of sets, that it's going to have to be translated

> into some terms that are neurologically realizable. [Y]ou talk about a generative grammar as being based on an operation of Merge that forms sets, and so on and so forth. That's something metaphorical, and the metaphor has to be spelled out someday.
>
> (Chomsky 2012: 91)

In other words, while the formal aspects of a Turing machine (e.g., Merge, sets, etc.) are, *ex hypothesi*, realized neurologically, it would be absurd ("astonishing") to expect physical representation of our arbitrary notations (e.g., $f_{\mathrm{MERGE}}(X, Y) = \{X, Y\}$). As Turing observed, in researching the similarities of minds and machines, "we should look [...] for mathematical analogies of function" (1950: 439) — similarities in software, not hardware.

Of course an ontological commitment to abstract properties and functions is not necessarily a commitment to Platonism (as Aristotle demonstrated and many in the biolinguistics program would argue), yet it is certainly the default setting. So it can be argued that I-language is just like Deutsch's chess program: a multiply realizable computable function (or system of computable functions). Indeed given my understanding of a Turing machine as a mathematical object, *I-language qua Turing machine is necessarily and properly defined as a physically (neurobiologically) encoded Platonic object.*

6 Conclusion

I have argued that mathematical biolinguistics is based on the *perfectly coherent* concept of computation — as formulated by Turing — unifying mathematical Platonism and biolinguistics: Evolution has encoded within the neurobiology of *Homo sapiens sapiens* a formal system (computable function(s)) generative of an infinite set of linguistic expressions (just as engineers have encoded within the hardware of computers finite functions generative of infinite output). This thesis, I submit, is or would be accepted by the majority of researchers in biolinguistics, perhaps modulo the Platonism, for indeed it is not necessary to accept the reality of mathematical objects to accept the reality of physical computation. However, I am a mathematical Platonist, and thus do recognize the reality of mathematical objects, and thus do argue I-language to be a concretization (an 'embodiment' in the technical sense) of a mathematical abstraction (a Turing machine), which to my mind best explains the design of language.

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★ FORUM ★

Biolinguistics: Fact, Fiction, and Forecast

Cedric Boeckx

1 Introduction

Biolinguistics, as I understand it, refers to a branch of the cognitive sciences that seeks to uncover the biological underpinnings of the human capacity to support language acquisition (the development of an I-language, where 'I-' is meant as 'internal', 'individual', and 'intensional', following Chomsky 1986). That language acquisition requires a (possibly complex and multi-faceted) biological foundation cannot be seriously put into doubt, and biolinguistics, in the wake of early works by Chomsky and Lenneberg, takes that fundamental facet of human biology as its subject matter.

In his 'discussion note', in which he reviews *The Biolinguistic Enterprise*, which I co-edited with Anna Maria Di Sciullo (Di Sciullo and Boeckx 2011), Jackendoff (2011) goes through a series of important issues conceding the field, and makes several points worth highlighting, but he also commits several errors worth pointing out. This is the object of the present piece. Specifically, my aim in the pages that follow is to tease apart the real issues ('fact'), from the rhetoric ('fiction') and from the different bets various researchers make concerning the future ('forecast').

Ray Jackendoff is eminently well placed to speak about biolinguistics, since he has made seminal contributions to the field. Indeed, he is among the most committed theoretical linguists I know when it comes to establishing interdisciplinary bridges (a necessary step towards a productive biolinguistics), and has been for many years before other biolinguists joined forces (witness Jackendoff 1983, 1987, 2007). Given his stature in the field, Jackendoff's opinion cannot be ignored. Inaccuracies, if any, should be corrected, lest beginning students of the field receive a distorted picture of the enterprise.

As the title of his paper indicates, Jackendoff contrasts two views of the language faculty. As he puts it in the abstract, his aim is to "compare the theoretical stance of biolinguistics" with a constraint-based Parallel Architecture of the sort he has been advocating for decades (see the pieces collected in Jackendoff 2010, and especially Jackendoff 1997, 2002, Culicover and Jackendoff 2005). As we will see shortly, however, the contrast between his approach and "biolinguistics" conflates 'biolinguistics', 'minimalism' and 'Chomsky's specific proposals within minimalism and biolinguistics', which are related, but nonetheless distinct targets.[1] This I

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[1] To be clear, 'biolinguistics', 'minimalism', and 'Chomsky' are related in the sense that Chomsky is at the heart of the cognitive revolution that provides the source for biolinguistics, and, among many other things, he is responsible for initiating and articulating the minimalist pro-

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take to be one of the major problems of Jackendoff's paper. The other one concerns the fact that a careful reading of Jackendoff (2011) reveals points of convergence that are far more significant than the contrasts that appear to dominate the paper.

Characteristically, Jackendoff's article touches on a wide range of issues (lexical redundancy, recursion, phonology, semantics, thought in other species, language-vision interfaces, evolutionary scenarios, etc.), all of which, according to him, provide evidence for the superiority of his approach over alternatives that he variously attributes to Chomsky, the minimalist program, and biolinguistics. Jackendoff also touches on the nature of linguistic inquiry more generally, highlighting the continuity with the Chomskyan program broadly construed, viz. a research paradigm that focuses on linguistic competence, its development in the individual and its emergence in the species. Jackendoff usefully discusses points of agreement concerning these general issues (in passing offering cogent responses to UG-critics, such as Evans and Levinson (2009); see Jackendoff's footnote 2, p. 587. In this part of his paper, Jackendoff points out that "of course, there are good reasons to want to minimize UG": "a leaner UG gives us a better chance of succeeding" in figuring out how UG is implemented in the brain and evolved in the species. (Jackendoff talks a lot about UG being encoded "somehow on the genome", but I think we should keep an open mind about the options nativism has to offer: there are many sources for innate ideas, the genome being only one of them; see Cherniak (2005), Longa and Lorenzo (2012), Chomsky (2005), and Lewontin (1993, 2000) for relevant discussion).

In this context Jackendoff provides one of the best descriptions I've come across of current biolinguistics: a field "...where linguistics makes contact with biology, taking the study of language beyond just the description of languages and language universals". But even in this early part of the paper, Jackendoff stresses a few biases characteristic of his general position, such as (i) the idea that language evolution is intimately tied to communication (see already Pinker and Jackendoff 2005, Jackendoff and Pinker 2005): "this is a problem ...not unique to the evolution of language. It arises in trying to account for any communicative capacity in any organism;" (ii) the idea that "explaining the origins of the language faculty depends of course on what one thinks the human language faculty *is*" ; and (iii) the idea that "the evidence from biological development is so far removed from the details of language structure that I find it hard to give these sorts of evidence priority over far more clearly established facts of modern linguistic structure in deciding which theory of linguistic knowledge to pursue".

I will come back to each of these points in the course of this paper. For now, let me concentrate on Jackendoff's description of biolinguistics (section 2 of Jackendoff 2011), where we begin to move from facts to fiction. (In what follows, I will

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gram; and minimalism certainly feeds on biolinguistic concerns, but biolinguistics itself is not a theoretical framework (on a par with, say, Government-and-Binding or minimalism). Unfortunately, Jackendoff is not the only one to err in this context. Consider, for example, the following passage from the Linguistic Society of America Special Interest Group [SIG] on Biolinguistics, founded in 2009, which "seeks to help the field of biolinguistics define itself by, as stated in the SIG description, "helping to identify what makes biolinguistics 'bio' (and 'linguistic'), initiate discussions on how it differs from previous models of generative grammar (and how it doesnt), debate whether generative grammar is actually a prerequisite [...] and so on." Asking how biolinguistics differs from previous models of generative grammar is, in my view, a category mistake.

back up my arguments by quoting extensively from Jackendoff's paper to help the reader identify what I am after, so I ask the reader's indulgence if at times, both a quote, and my paraphrase of it appear side by side.)

2 "Biolinguistics"

Jackendoff begins his section on "biolinguistics" with the following:

> In recognition of the goal of interfacing linguistic theory with biology, practitioners of the Minimalist Program have begun calling the enterprise "biolinguistics" (e.g., Jenkins 2000, Larson, Déprez, and Yamakido 2010, Di Sciullo and Boeckx 2011, and the online journal Biolinguistics [www.biolinguistics.eu])

He then goes on to say that "biolinguists have for the most part focused on three issues. The first is the genetic basis of the language faculty, including extensive discussion of the possible role of the FOXP2 gene". Here Jackendoff points out that "[t]his issue has been part of the literature for decades (even if much of the evidence is new) and is not specific to the biolinguists". "The second issue is what principles of evolutionary and developmental biology could have led to a language faculty", where Jackendoff adds that "here, it seems to me, the distance between the nature of linguistic structure and what is known about the genetic basis of biological development is so great that the best that can be attempted at the moment is informed speculation, and I will have nothing to say about this issue here." Finally, "The third issue is the "first-principles" question posed by Chomsky (1995): "How perfect is language?", where perfection is defined in terms of elegance, lack of redundancy, and computational efficiency. Of the three, this issue has been the most central to biolinguistic inquiry into the actual form of the language faculty."

With this passage, Jackendoff's critique begins: "in stressing the deep questions of genetics and optimal design, the biolinguists have bypassed an important biological and psychological issue". Specifically,

> Evolution probably did not come up with radically new kinds of neurons; and if special kinds of neurons had been discovered in the language areas of the brain, we would have heard about them. Moreover, although it is important to bear in mind how little we know rather little about how assemblies of neurons encode and compute information, we might still be inclined to guess that evolution did not come up with radically new kinds of mental computation, mental processes, and mental architecture in inventing a language faculty. (See Gallistel and King 2009 for extensive discussion of the conservatism of neural mechanisms.) So to the extent that a theory of language permits a graceful integration with a plausible picture of the structure and function of the rest of the mind/brain, it places fewer demands on the genome, and therefore it is a better theory. I would contend that this too should be considered a criterion on linguistic theory from a biolinguistic perspective. (p. 590)

The passages just quoted provides enough material to make a first series of comment. First, Jackendoff errs in saying that practitioners of the minimalist program

have begin calling the enterprise biolinguistics. As has been pointed out on numerous occasions (including in the introduction of Di Sciullo and Boeckx (2011), which Jackendoff takes as a point of reference in his article), the term 'biolinguistics' (in its current sense) goes back to the mid-1970s. It is true that minimalist practitioners are using the term, because many (including Jackendoff, as pointed above) have seen points of contact between a minimal UG (the focus of minimalism, Chomsky 2007) and the attempt to uncover the biological (i.e. neural, genomic, etc.) basis of the language faculty. But as the manifesto of the online journal Biolinguistics (cited by Jackendoff) makes clear (Boeckx and Grohmann 2007: 3): "It is important for us to stress that biolinguistics is independent of the minimalist program. As Lenneberg's work makes clear, biolinguistic questions can be fruitfully addressed outside of a minimalist context."

It is therefore wrong to say that "[inquiry into the genetic basis of the language faculty] has been part of the literature for decades (even if much of the evidence is new) and is not specific to the biolinguists". It *is* specific to biolinguistics (as opposed to, say, sociolinguistics), but not, of course, specific to minimalism, or any other theoretical framework.

At times, Jackendoff appears to realize that biolinguistics and minimalism are not the same thing, since he writes (in a passage quoted above): "So to the extent that a theory of language permits a graceful integration with a plausible picture of the structure and function of the rest of the mind/brain, it places fewer demands on the genome, and therefore it is a better theory. I would contend that this too should be considered a criterion on linguistic theory from a biolinguistic perspective." (p. 590)[2]

The issue of graceful integration is indeed central to biolinguistics, but Jackendoff is not the only one to realize this. Numerous authors (including those of a minimalist background) have written about this in biolinguistic venues, as the frequent references to David Poeppel's reflections on this matter (Poeppel 2005, Poeppel and Embick 2005, Poeppel 2011, 2012) attest (see Hornstein 2009, Boeckx To appear, Samuels 2011, Di Sciullo and Boeckx 2011). It is simply not true that "biolinguists have bypassed an important biological and psychological issue", for this

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[2] See also the following passage:

> Although these points all violate the preconceptions of "efficient computation" that gave rise to the notion of Merge, each of them is supported both within the theory of language and elsewhere in the mind/brain. Thus, they should be seen as welcome advances in pursuing a biolinguistic agenda, at once accounting for a wider range of phenomena in language and bringing the language faculty closer to graceful integration with the rest of the mind/brain. (p. 603)

And the concluding paragraph of his review (p. 617):

> In each case the proposed answer is also motivated on grounds internal to language, and in each case it differs from the proposals of the Minimalist Program and biolinguistics, which are based on criteria of perfection, optimal design, and efficient computation. I conclude that a constraint-based and Unification-based Parallel Architecture leads to more satisfactory accounts of the linguistic phenomena in question, incorporating the insights of many other constraint- based frameworks. At the same time it provides a far more promising approach to the criterion of graceful integration, offering a quite different direction for biolinguistic research.

is what defines the field. True, Jackendoff is entitled to express skepticism about the results obtained so far, but the same could be said of his own approach. In fact, one could say that Jackendoff 'bypasses' important evo-devo issues when he writes "it seems to me, the distance between the nature of linguistic structure and what is known about the genetic basis of biological development is so great that the best that can be attempted at the moment is informed speculation, and I will have nothing to say about this issue here". Are we so sure that the distance between the nature of linguistic structure and what is known about the brain, or between the nature of linguistic structure and what is known about other cognitive systems, is less great?

While I personally agree with Jackendoff that we should try to assimilate what we can learn from other cognitive systems, I also think Jackendoff is right in stressing that we know far more about the structural properties of language than any other system. So how can we be sure in which way integration will proceed? As Newport (2010282) has written in her thoughtful review of modularity issues, "The generative tradition in language has given us an elegant and detailed articulation of how these principles work themselves out in language; whether the same principles apply in detail to any other domain remains to be seen, since few comparably sophisticated analyses have ever been done of other complex cognitive domains."

At this point, it seems fair to say that we know so little about so many things that appear necessary for a truly integrated biolinguistics that favoring some directions over others is a matter of placing one's bets (a necessary part in any scientific inquiry)—more a matter of gut feelings than anything else ('forecast' rather than 'fact'). But as Yogi Berra told us, it's hard to make predictions, especially about the future. Certainly, one cannot at present argue in favor of the superiority of one bet over another. Jackendoff may be more attracted towards an alignment with other cognitive systems because he has worked extensively on the relation between linguistic cognition and visual cognition, but I may be more attracted towards development because of results like Gunz et al. (2010, 2012), Neubauer et al. (2010) that suggest that species-specific patterns of brain configuration that may well give rise to a language-ready brain arise early in development (hence the relevance of evo-devo considerations). Wouldn't ignoring that amount to bypassing issues concerning the human brain?

The last comment I'd like to make in the context of the passages quoted at the beginning of this section concerns what Jackendoff takes to be central issues of "biolinguistics". Specifically, I think he is wrong about "perfection" being so important. The issue of optimal design was central at the beginning of the minimalist program, but numerous authors have pointed out that this issue has lost its centrality in later implementations of the program. My own feeling is that it has played a less direct role in the revival of biolinguistic concerns than other factors, such as results from comparative psychology or genomic studies (the term 'perfection' is not even part of the index of Di Sciullo and Boeckx (2011), as none of the chapters of the book elaborate on this issue). Moreover, Jackendoff is wrong in keeping separate the 'second' central issue in "biolinguistics" (which I take to be evo-devo concerns) and the 'third' issue ('perfection', or perhaps more generally, 'third factor principles of the sort discussed in Chomsky 2005), because they are clearly related,

as is evident from the evo-devo literature.[3] So, one cannot set one aside ("I will have nothing to say about this issue here") and try to focus on the other.

3 Architectural concerns

When it comes to offering arguments in favor of his own framework, Jackendoff offers very little new. Most of the themes, and specific examples used as illustrations, are to be found in his previous publications. Accordingly, I will not go through each and every one of them, as I have already done so elsewhere (Boeckx and Piattelli-Palmarini 2007, Boeckx et al. 2010).[4] Instead, I will focus on a few new points made by Jackendoff, and show that in each case they miss their targets.

As is well-known, Jackendoff is very critical of what he calls "syntactocentric" models, which place emphasis on the role of (narrow) syntax in structuring the modern language faculty, and favors his (and others') "parallel" models, where every component of the grammar is said to contribute equally to the nature of the language faculty. It is therefore no surprise to find in Jackendoff (2011) a series of attacks on the centrality of Merge, and recursion in language (familiar from Pinker and Jackendoff 2005 and Jackendoff and Pinker 2005), and a favorable treatment of "unification" as the core mechanism in language, an emphasis on words and on the redundancy of the lexicon, a demand for attention to phonology and semantics, and a discussion of possible evolutionary stages where syntax was missing (protolanguage).

Let me stress that all of these are important themes, and that in some cases Jackendoff is right to criticize some particular research directions (e.g., the position in Hauser et al. 2002; for my own critical remarks on the latter, see Boeckx (2013)), but wrong to say that these are representative of biolinguistics as a whole. But I cannot fail to find it ironic that in several places Jackendoff is led to conclude (expressing surprise!) that his position converges with the one he is attacking:

> "In a curious way, one might see Marantz 1998 as arriving at the same conclusion, though with a quite different twist" (note 22, p. 610),
> "in a curious way this story is compatible with Chomsky's speculations" (p. 616),
> "So we might want to say that the digital property of phonology comes by virtue of "natural law," in (I think) precisely the sense Chomsky intends" (p. 604)

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[3] Design considerations, including issues like 'perfection' or 'optimality' are related to 'third factors' considerations in the following sense: third factor principles *on their own* lead to systems that are optimized systems because they ultimately instantiate the workings of laws. But biological systems are not the result of third factors alone. When I say that 'perfection' considerations no longer dominate minimalist writings, while third factors do, I mean that one can choose to study the contributions of third factors without elaborating on the qualitative ('optimization') consequences they have.

[4] I cannot resist mentioning that, quite apart from what he wants to say about biolinguistics, if he wants to criticize minimalism (which I recommend he do in separate pieces), Jackendoff should update his target. For example, he insists on the important of a "numeration" (p. 599) in the context of minimalist derivations, but numerations have long been left out of minimalist inquiries. In general, I think that one should keep the big picture from the details separate.

Such passages are in fact very frequent in Jackendoff's article, more so than in previous critiques by him, which makes Jackendoff (2011) particularly interesting. Thus, concerning the issue of redundancy (an issue that in fairness Chomsky and many others have rarely addressed), Jackendoff writes (p. 591), "[i]t is true that, as Chomsky reminds us, "the guiding intuition that redundancy in computational structure is a hint of error has proven to be productive and often verified." This intuition lies behind good scientific practice, and too easy an acceptance of redundancy can be a source of theoretical complacency. However, good science also ought to be able to consider redundancy as a solution when the facts patently point that way." In other words, it's an open issue.[5]

More eloquently still, when discussing the superiority of "unification"-based approaches over Merge-based approaches, Jackendoff addresses the obvious point: how do the elements to be unified get constructed in the first place? (In other words, what constructs the constructions?) To his credit, Jackendoff states the obvious (other construction grammarians are, unfortunately, far less explicit): "I should make clear that Unification alone cannot create constituent structure: it only creates a Boolean combination of pre-existing features and structures." (p. 602) Surprisingly, Jackendoff goes on to state "In order to build structure, one needs a skeletal constituent structure that can be unified with two or more items. Such a skeleton is of course already richly present in cognition: the part-whole schema. One formal realization of this schema is a set {x, y} with variable elements x and y as parts." But this is Merge, and Jackendoff knows it: "This can be unified with specific elements A and B to form the set {A, B}, in effect the output of Merge." "One might say then that these schemas are nothing but constraint-based counterparts of Merge, and this would be partly correct." Jackendoff quickly adds that "However, Merge per se is too limited". But he has just pointed out that unification alone is equally limited (it needs something like Merge). Accordingly, it strikes me as incorrect to oppose Merge and Unification. (I should point out that proponents of Merge-based systems would readily agree that Merge is not enough to capture all the facts in natural language grammars. All they are claiming is that in order to kick-start all the other operations, (all) you need (is) Merge.)

The ultimate irony is to be found on p. 616, where Jackendoff writes (in the context of language evolution), "perhaps beyond timing, the difference [between his and Chomsky's approaches–CB] is predominantly one of terminology". So much for "two views" about the human language faculty.[6]

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[5] If Gallistel and King (2009) are right (and they strike me as such), we have no idea about what the brain basis of memory is, so imagine how little we know to talk about redundancy ....

[6] Incidentally, Jackendoff's insistence that his parallel model leads to evolutionary scenarios that are quite different from those to which Merge-based approaches lead is incorrect. As Clark (2013) shows in detail, there is not a simple dependency between syntactic theory and views on syntactic evolution. Such a conclusion should come as nor surprise, given the range of evolutionary scenarios compatible with Merge-based approaches, including those that Chomsky approves of, such as Lebeaux (1988), or those published in the *Biolinguistics* journal (works by Progovac and others, referred to in Jackendoff 2011).

In the context of evolutionary scenarios, I cannot fail to express my surprise about Jackendoff's vision. According to him (p. 615–6), "conceptual structure evolved first, long before language, in fact long before hominids. Then came phonology and its links to meaning, yielding protolanguage, and last came syntax and its links to both." But if semantics and phonology were already in place, and linked to one another, why was syntax needed at all?

Arguably the most unfair part of Jackendoff's overview concerns his (cursory) remarks about phonology and semantics. Jackendoff writes (p. 611):

> Some practitioners of the Minimalist Program (e.g., Phillips and Lau 2004, Marantz 2005) have commented that a theory with only one "generative engine" is preferable on first principles. This is taken to be sufficient to dismiss the Parallel Architecture, which has three generative engines. But the Minimalist Program addresses only syntax: it has no theory of phonology, no (generally accepted) theory of semantics, and no (generally accepted) theory of the lexicon. So it should be no surprise that it needs only one "generative engine."

But Jackendoff ignores that there are treatments of phonology within a minimalist-oriented biolinguistic approach, beginning with the rich tradition within Distributed Morphology (for the most comprehensive overview, see Samuels 2011, based on Samuels 2009, but see also Embick 2010, Marvin 2002, and Newell 2008). Although these may not constitute a "generally accepted" theory of (morpho-) phonology (has there been any since *Sound Patterns of English*?), they show how syntactocentric models can provide a solid foundation for models of phonology. True, Chomsky himself has not endorsed such a model (though some scattered remarks in his recent writings make me think he would favor it), but remember that Chomsky is not (or should not be) the target of Jackendoff's focus, if the latter is 'biolinguistics' at large. Within a field in the making like biolinguistics, there is no generally accepted theory of anything. Would Jackendoff's own writings on semantics or the lexicon, as rich as they may be, count as "generally accepted"? I don't think so. Theories, to count as such, should not only enable one to describe a rich set of facts, they should place constraints on what is possible, and here I think that Jackendoff's parallel architecture is too unconstrained. As I pointed out on other occasions (Boeckx and Piattelli-Palmarini 2007, Boeckx et al. 2010), it's hard to find the components of Jackendoff's vision that rule out structures that are known to be illicit.

When it comes to semantics,[7] Jackendoff rightly stresses the existence of 'thought' (conceptual structure) in other species, but I think he seriously minimizes the impact of syntax on these conceptual structures. Much like there is massive evidence for thought in other species, there is massive evidence that language affects conceptual combinatoriality. This is a message that comes from numerous domains (comparative psychology (Hauser 2009), archeology (Mithen 1996), developmental psychology (Spelke 2003)), and it is a finding that biolinguists are trying to capture (including in the book that Jackendoff takes as his target; witness my contribution to Di Sciullo and Boeckx (2011), but see also Pietroski 2007, Ott 2009). In essence, the finding amounts to the emergence, thanks to language, of robust cross-modular

[7] Jackendoff completely misrepresents Hinzen's contribution to Di Sciullo and Boeckx (2011). He writes that Hinzen "proposes that in fact combinatorial syntax directly generates conceptual structure, so there is no need to map to the semantic interface; he has in effect reinvented Generative Semantics." Nothing could be further from the truth. Hinzen claims, along with—though perhaps more explicitly than—others, that syntax gives rise to meaningful representations that would be unavailable in the absence of language. Being syntactocentric, Hinzen's model is not Generative Semantics, but Generative Syntax. If anything, Jackendoff's separate generative semantic component is much closer to Generative Semantics, as Boeckx, Hornstein, and Nunes (2010) point out.

thinking (evidence for which is absent in other species). This is, in fact, a finding that is hard to capture in Jackendoff's framework, where conceptual structure in all its richness (and generatively) pre-dates the modern language faculty.

Jackendoff, of course, may disagree with this finding, but he cannot simply ignore it (doing so would be "bypass[ing] an important biological and psychological issue"). In fact, if Jackendoff had paid attention to that literature, he would have seen that many authors (myself included) attribute to words (or lexical items) an importance that he claims words have and that minimalists (he says) ignore (p. 599), "recursive structures cannot exist without units to combine"). He would have seen that as far as that literature is concerned, Merge is a generic combinatorial mechanism (coming "off the shelf, from [the] F[aculty of] L[anguage] B[road]").[8] He would in fact see (see Boeckx 2009, 2011, and especially Boeckx To appear) that some minimalists trying to contribute to biolinguistics blame standard (minimalist) models of being too "lexicocentric". In short, Jackendoff would have seen that Hauser et al. (2002) is not the only possibility on the table.

As I have written elsewhere (Boeckx Submitted), I agree with Jackendoff that Hauser et al. (2002) is problematic in many respects, but I also think that Jackendoff's argument in favor of recursion, to which a good portion of Jackendoff (2011) is devoted, in other cognitive domains is far from compelling. The crux of the matter is to be found on p. 591:

> Evaluating whether a particular domain of behavior or cognition is recursive requires a theory of mental representations in that domain. And unfortunately, outside of linguistics, there are virtually no theories of mental representation (with some exceptions that I will mention in a moment). In the absence of such theories, it is premature to assert that only language requires recursion.

This is correct, but by the same reasoning, is it not premature to assert that other cognitive systems require recursion? Here it seems to me Jackendoff minimizes the risks of making claims in the absence of comparably sophisticated analyses in other complex cognitive domains. Once again, I find it hard to argue for the superiority of one approach over the other.

4 Concluding remarks

Although the goals and ambitions of biolinguistics were first formulated over 50 years ago, too many pieces were missing to even guess what the puzzle would look like: we knew too little about the genome, about the cognitive profile of other

[8] Jackendoff appears to be aware of this, as he points out in note 16, "Hornstein and Boeckx 2009 (89) make such an argument: "... it is reasonable to suppose that an operation like Merge, one that puts two elements together (by joining them or concatenating them or comprehending them in a common set), is not an operation unique to FL. It is a general cognitive operation, which when applied to linguistic objects, we dub 'Merge'." In other words, they acknowledge that recursion may be found elsewhere in cognition, but they do not call it Merge. Thus they reduce the notion that Merge is unique to language from an empirical hypothesis to a tautology." Jackendoff's comment is unfortunate, as he ascribes a claim derived from Hauser et al. (2002) "Merge is unique to language" to us. But the passage makes clear that we don't hold this position, so we are not putting forth any tautology.

species, about how evolution works, and also about what language is. Although we still know very little, we are now in a better position to seek integration, hence the rise (or revival) of biolinguistics. Being so young, the discipline is still in an exploratory phase, and I feel it would be seriously wrong to be dogmatic about anything. There are, of course, many views about the specifics of the language faculty, but I don't think there are two views about biolinguistics. It's the same program for everybody. And one should welcome theoretical pluralism.

Jackendoff is right about many things, but wrong to stress disagreements (especially superficial, "terminological" ones) at the expense of common goals and common hypotheses. When the rhetoric (fiction) is carefully separated from the facts, everyone turns out to be in the very same boat (like it or not, that's the boat Chomsky and Lenneberg constructed). We all "aspire to biological plausibility" (p. 617), but we all should recognize that we all face Poeppel's granularity mismatch problem. We also suffer from the absence of comparably sophisticated analyses outside of the language domain. So, it's very risky to claim that some of us are bypassing important biological and psychological issues. Being in the same boat, we all make slightly different bets regarding how to reach the land. These bets arise as a result of personal biases.

What I have tried to show in this paper is that Jackendoff is wrong to take his biases as a solid foundation to argue in favor of his own articulation of "biolinguistics". We all have to start somewhere, but is the essence of language really communication (consider how problematic this is: Balari and Lorenzo 2013)? Are we so sure that different theoretical articulations really make radically different evolutionary predictions, given the richness of an extended synthesis in biology (Pigliucci and Müller 2010)? Of course, as Jackendoff likes to say, one's theory of language evolution depends on one's theory of language. But one's theory of language evolution depends equally on one's theory of evolution. And are we so sure that evo-devo issues are too removed from current linguistics that we can set them aside?

In the end, Jackendoff points out (p. 615) that "All that could be argued is that it is plausible (though I can imagine that someday genetic evidence might be telling)". We could all imagine this regarding our pet theories. But imagination belongs to the realm of forecast, not fact. Pretending otherwise is entering the realm of fiction.

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