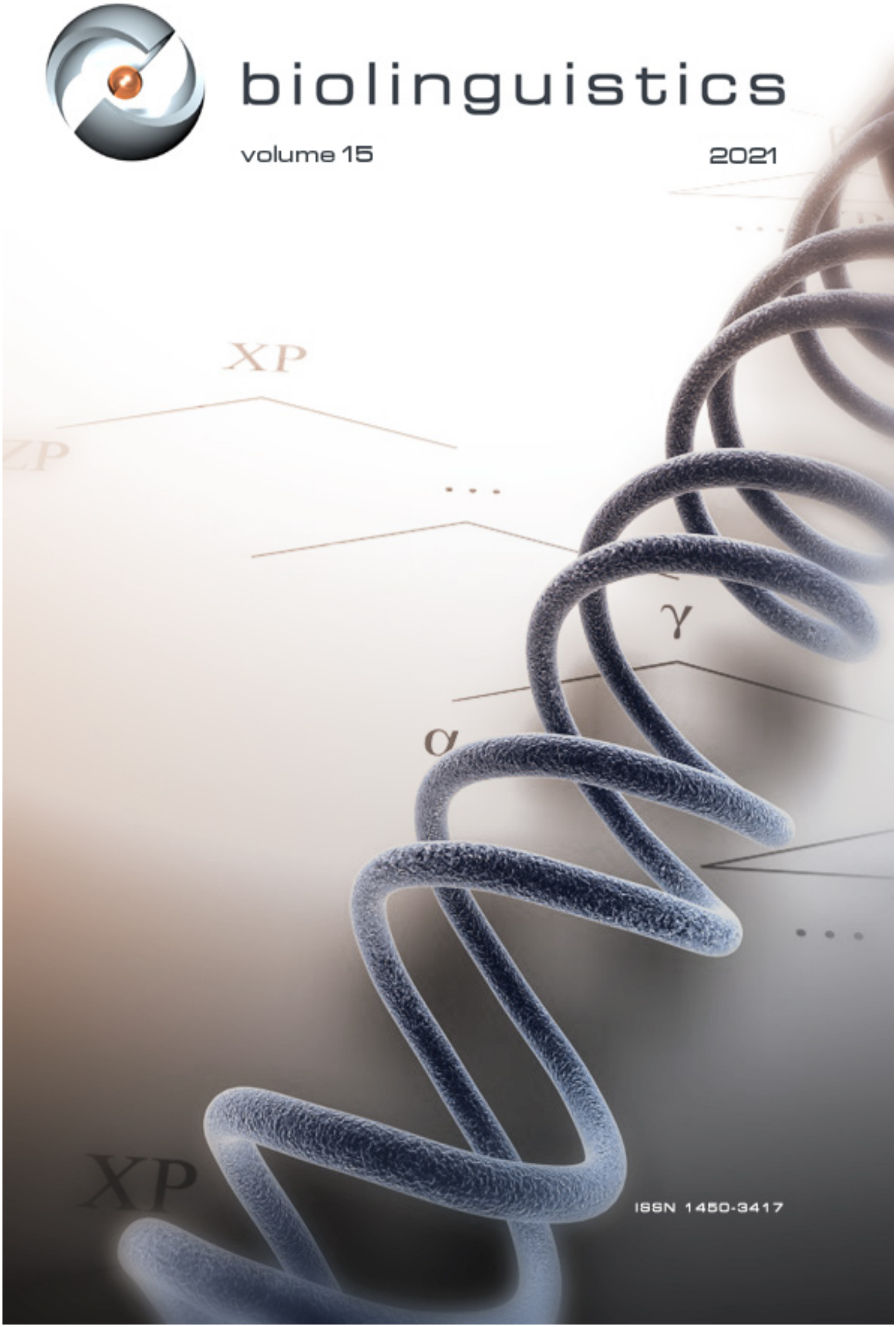
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Hypotheses and Definitions in Language Evolution Research: Reply to Mendívil-Giró (2020)

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1. Introduction

In his reply to our paper "*Language* in language evolution research" (Wacewicz et al. 2020), José-Luis Mendívil-Giró (2020) argues against one of the central points of our paper, namely that the definitions of the term *Faculty of Language in the Narrow Sense* (FLN) in Hauser, Chomsky, & Fitch (2002; HCF) and Fitch, Hauser, & Chomsky (2005; FHC) are incompatible. In addition, he argues that the terminology proposed by HCF could be fruitfully applied to the theoretical avenues surveyed in the remainder of our paper, and that the idea of the language-ready brain, which is discussed at length in our paper, shares many theoretical assumptions with HCF's approach to language.

Although we do not agree with the main points of Mendívil-Giró's critique, we want to start by emphasising that there are many good and valuable specific observations in his reply. Firstly, the "faculty of language" is, obviously, not the same as "language." We agree with Mendívil-Giró that "HCF is not about lang-

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uage in general, but about the human faculty of language (FLB), a property or state of the human brain that allows us to learn and use languages." The original authors were careful to explicitly note this difference (HCF 2002: 1570), as were we, by repeatedly using the formulation "(the faculty of) language." This should not however be used to question the centrality of the term "faculty of language" for language evolution and the fact that very frequently researchers (especially from the biolinguistic tradition) take it as a shorthand for "language" in this context. Consider, for example, the following quote from Fitch (2010: 22): "[R]esearchers (including ourselves) had been using the same word, 'language', to talk about two different things (FLB and FLN) for many years." Mendívil-Giró agrees on both of those points: "In fact, the object of study from the biolinguistic point of view adopted by Chomsky, Fitch an Hauser cannot be other than FLB (that is, FL)." Secondly, we seem to be in full agreement with Mendívil-Giró when he declares that:

> It would certainly be naive to think that we can have a "correct" definition of language. The same is true in any field of science: you do not need a universally accepted definition of life to study the origin of life, nor a universally accepted definition of natural species to study the origin of species (not to mention matter or energy). Yet shared assumptions about these objects are clearly needed if the sciences that study them are to be viable. (2020: 146)

In our target paper, we do emphasise this former point (no single "correct" definition of language). But we also agree with the latter point (shared assumptions are needed), while noting that the shared assumptions will form a family-resemblance pattern without however reaching a full overlap. In sum, here Mendívil-Giró's position seems to be fully compatible with ours, as we extensively elaborate it in the Discussion section of our paper. For example:

> We distinguish between definitions on two different and clearly separable levels. One is the level of more specific technical terms that function as building blocks of theories and especially of hypotheses, which require unambiguous formulations so as to meet the fundamental standards of non-triviality and falsifiability. This level is thus essential for science to make progress by conclusively resolving arguments with recourse to empirical data rather than getting stuck on conceptual differences. The other level, however, is the global level of macroscopic notions, which cannot (without further specification) function as building blocks of specific theories or hypotheses but have a different role, related instead to integrative and classificatory goals.
>
> (Wacewicz et al., 2020: 87–88)

In our paper, we have already pointed out that any theory-specific use of the term "language" will inevitably remain meronymous, in the sense of always relating only to parts of the complex phenomenon. We have therefore proposed to push definitions one level down: Our proposal was to leave language as an

unanalysable prime and instead provide rigorous definitions of particular components or aspects of language.

2. On the Definitions of FLN

One of our main points of contention with Mendívil-Giró's response is located in Section 2, as many of his claims are falsified by the quotes from HCF and FHC already provided in our target paper. He claims that our summary of the definetion of FLN in HCF is incorrect. In particular, while we show that in HCF "uniqueness to humans" is a *hypothesis* about FLN, Mendívil-Giró argues that it is part of the *definition* of FLN. Although the reading proposed by Mendívil-Giró is a good interpretation of the terminological distinction between FLN and FLB in the light of the 2005 paper by FHC, the actual published text of the 2002 paper (HCF) clearly contains the inconsistencies that we have pointed out and documented with quotations.[1]

In science, it is crucial to keep definitions and hypotheses apart. A major source of the inconsistencies that we have pointed out—and, by extension, the confusion that leads to the discussion we are engaging in here—lies in a conflation of hypotheses and definitions. In the remainder of this section, we will therefore aim at teasing these two aspects apart. Mendívil-Giró's stance that the FLN definition in HCF is extensional while the FHC one is intensional is, in our view, not convincing, although it can prove very useful in understanding how exactly the different conceptualisations of FLN conflate definitions and hypotheses. As we have already pointed out in our original paper, both HCF and FHC repeatedly make it clear that their definition is, first and foremost, intensional (to use Mendívil-Giró's term) and that its extension, i.e. what belongs to FLN, has to be empirically determined. Thus, any extensional claims that go beyond the provided definition are hypotheses, not definitions. The feature of being uniquely human is not part of the definition of FLN in HCF—instead, the assumption that FLN is uniquely human is explicitly framed as a hypothesis that could be wrong. In FHC, on the other hand, it becomes a major defining criterion for FLN, culminating in the possibility that FLN could be an empty set if no features that are uniquely human can be found.

To illustrate our point in more detail, let us tease apart the definitions and hypotheses that can be found in the original papers.

Definition.

Mendívil-Giró maintains that

> [HCF] propose that the FLN label should be reserved, by convention, for those components of the FL that (supposedly) are neither shared with other species (are specifically human) nor are part of other human cognitive domains (are language-specific); hence the use of the word narrow. (2020:147)

[1] A more extensive treatment of the inconsistencies was offered in Wacewicz (2012), which was the basis for our discussion in section 3 of our target paper.

However, he does not point us to a quote with the exact phrasing of this proposal. The likely reason for this is that there is no such quote in the entire published text of HCF (of course, we challenge the critical reader to find the relevant fragment of the HCF paper). Instead, HCF explicitly argue that FLN *may be* unique to humans, adding that "*this represents a tentative,* testable *hypothesis* in need of further empirical investigation" (HCF: 1576 [emphasis added]). In FHC, by contrast, species-specificity is part of the definition of FLN:

> The contents of FLN are to be empirically determined, and could possibly be empty, if empirical findings showed that none of the mechanisms involved are uniquely human or unique to language, and that only the way they are integrated is specific to human language. The distinction itself is intended as a terminological aid to interdisciplinary discussion and rapprochement, and *obviously does not constitute a testable hypothesis*. (2005: 180–181 [emphasis added])

At the beginning of Section 3 of our original paper, we list three quotes from HCF, where HCF define FLN as an abstract linguistic computational system, in particular when they first introduce that notion (p. 1571): "*Faculty of language–narrow sense* (FLN). FLN is the abstract linguistic computational system alone, independent of the other systems with which it interacts and interfaces" (HCF: 1571, italics in the original). Further, one of the HCF authors writes: "FLN … was defined by Hauser et al. (2002) as a computational process that is responsible for the generative and hierarchical properties of narrow syntax" (Tincoff & Hauser 2006; again, a quote already cited in our paper).

The fact that all these quotes were ignored by Mendívil-Giró indicates that his proposed interpretation seems to be only very loosely based on the actual content of HCF.

Hypothesis.

We show that HCF hypothesise that FLN is uniquely human (and uniquely linguistic), and provide the specific relevant wordings in three quotes in Section 3.1.2 of our original paper. Although Mendívil-Giró describes this as our "interpretation," this is the literal published text of HCF, for example "*Hypothesis 3: Only FLN is uniquely human*" (HCF: 1573, italics in the original). Note that species-specificity is explicitly framed as a hypothesis here, and nothing in the text indicates that it should be understood as a definition or a terminological proposal.[2] Quite to the contrary, HCF specifically assert that human uniqueness is not predicated definitionally of FLN, but as a hypothesis about it: "[A]lthough we have argued that most if not all of FLB is shared with other species, *whereas FLN may be*

[2] Mendívil-Giró does use one quote to illustrate his position here: "Their hypothesis in HCF is that the FLN label should be reserved only for the computational component: 'We propose in this hypothesis that FLN comprises only the core computational mechanisms of recursion as they appear in narrow syntax and the mapping to the interfaces'" (HCF 2002: 1573). This is a misinterpretation. As we show above, HCF first define FLN as "the abstract linguistic computational system" and then hypothesise in this quote that this abstract system "comprises only the core computational mechanisms of recursion".

unique to humans, *this represents a tentative, testable hypothesis* in need of further empirical investigation" (HCF: 1578, italics ours).

All in all, the definitions provided in HCF and FHC are only compatible if we conflate hypotheses and definitions (again, a point clearly demonstrated and documented with specific quotes in our target paper). This does not exclude the possibility that FLN is widely understood in the way proposed by Mendívil-Giró. But note that this fact, in turn, lends even more support to the point we made in Section 3, where we argued that "the specific wording of the top-down definitions of *language* was inconsequential to the research practice of the field."

3. The Language-Ready Brain

In Section 3 of his reply, Mendívil-Giró addresses the remaining theoretical frameworks discussed in our original paper:

(i) Language as a Multimodal Phenomenon (Kendon; McNeill; Zlatev)
(ii) Language as a Complex Adaptive System (Steels; Kirby)
(iii) Language as a Form of Social Interaction (Tomasello; Levinson)
(iv) Language in the Language-Ready Brain (Arbib; Bouchard; Boeckx & Benítez-Burraco)

Mendívil-Giró argues that the terminological distinction between FLN and FLB is compatible with each of these approaches, focusing on the fourth type of models. The first three lines of research are discussed surprisingly briefly in his reply. He devotes only one line to the view of language as a multimodal phenomenon (Section 4.1 of our paper) and writes that it identifies "language with speech and gesture, and would therefore be a central part of the study of the evolution of the sensorimotor component (SM) of FL." Thus, his analysis arguably oversimplifies both the multimodal conception of language and the FLN/FLB approach. The main tenet of the latter is that there is a set of formal properties of language that can be abstracted away from instances of linguistic usage. The views discussed by us in Section 4.1 of our paper vehemently oppose such a conceptualisation. As Kendon argues, language is only feasible in the context of other semiotic systems, most importantly gesture, together with which it is used—abstracted away from this rich semiotic context, language itself fades away (Kendon 2004, 2014). Further, it is difficult to agree with Mendívil-Giró that the multimodal approaches describe the evolution of the sensorimotor component of FL. This statement disregards the essentially cognitive nature of these proposals, for example, of McNeill's Growth Point (McNeill 1992, 2005), foundational to his conception of language and its evolution (McNeill 2012), or of mimesis, on which Zlatev (2008) builds the semiotic hierarchy to account for the emergence of language (Zlatev et al. 2020). On a more general level, Mendívil-Giró misses the point that for Kendon, McNeill, and Zlatev multimodality (or polysemioticity) does not represent an accidental property of language but its design property (cf. Vigliocco et al. 2014).

The language-as-a-Complex-Adaptive-System (CAS) view is dealt with equally briefly. Mendívil-Giró states that this view "is not particularly interested in FL as a biological object, nor, therefore, in the evolution of its components."

However, if this is meant to imply that proponents of a CAS view are not particularly interested in the biological and cognitive dimension of our ability to acquire and process language, this statement is misleading. In fact, the proposal that "language is shaped by the brain" (Christiansen & Chater 2008) highlights that the properties that shape language structure in order to make it learnable by humans are of fundamental importance to understanding language and its evolution. This importantly includes the complex interplay of biological properties as well as cultural and interactional processes (Beckner et al. 2009; Christiansen & Chater 2016). Importantly, these biological and neurological properties are seen as being domain-general in nature (Beckner et al. 2009; Pleyer & Hartmann 2019).

It is also somewhat misleading to state that the following view is increasingly popular (Mendívil-Giró): "[L]anguages somehow externally developed this complexity and motivated the adaptations that would lead to the language-ready brain." In fact, it is not the position of the complex adaptive system approach that languages "somehow" developed complexity. In fact, it is one of the main research avenues of this approach to investigate the factors that influence the emergence of structure and complexity in languages, with researchers having found a wealth of social, ecological, and other factors that influence the shape of languages in interaction with biological biases (e.g., Bentz 2018; Lupyan & Dale 2010, 2016; Raviv et al. 2020).

Mendívil-Giró is similarly laconic in his evaluation of the social-interactive perspective (Section 4.3 of our target paper). In this respect, he notes: "[Tomasello and Levinson] simply ignore the computational dimension of language (FLN)." In fact, the idea that there is some computational core of language is incompatible with their views. As we stress in in Section 4.3 of our paper, language for them is a form of social cognition and action, i.e. it is the implementation of socio-cognitive mechanisms, such as shared intentionality (Tomasello & Carpenter 2007) or the "interaction engine" (Levinson 2006), in the process of communication (Tomasello 2008; Levinson & Holler 2014). Beyond this characterisation, there is no language, which Mendívil-Giró apparently finds difficult to accept and proposes that their work "constitute[s] a part of the investigation of the evolution of the relation between the CI and SM components of FLB."

Tomasello and Levinson are committed to a deeply functionalist view, whereby language serves to achieve social goals—direct attention (Tomasello 2008), aid collaboration (Levinson 2006), or reasoning about each other's intentions (Tomasello 2008). There are of course properties, for example, codifycation (Tomasello), which distinguish language from other semiotic systems (e.g., gesture), which serve similar goals. However, the design of language to a large extent depends on the same general-purpose mechanisms as these other systems do (and accordingly, it entails a different sort of computations at the brain level), while its unique characteristics are the result of the interplay of these mechanisms with cultural-historical processes (Tomasello & Carpenter 2007; Tomasello 2008; Evans & Levinson 2009). In addition, it should be noted that Tomasello has written extensively on his usage-based, construction grammar approach to language acquisition (e.g., Tomasello 2003, 2011). This approach investigates how children use their abilities of intention-reading and pattern-recognition to build up networks of constructions of different degrees of abstractness and schematicity (e.g., Tomasello 2003; Diessel 2013). This also means that the cognitive dimension

of language acquisition and processing, which is extensively researched in constructionist approaches (e.g., Hoffmann & Trousdale 2013), is highly relevant for the social-interactive perspective. It is therefore misleading to say that the computational dimension of language is simply ignored in these approaches. Instead, it is reframed and viewed from a different perspective.

In his more extensive discussion of the language-ready brain hypothesis, Mendívil-Giró raises an interesting question when he points out that this term can be understood in two ways: "Either the brain first developed, through evolution […], or languages developed as complex cultural objects and then they served as an adaptive environment for the evolution of the language-ready brain from a 'language-unready' brain." This is a very relevant point as there are indeed multiple interpretations of the concept of language-readiness. From a gradualist point of view that is taken by most of the current approaches that we have reviewed in our paper, it would make sense to assume a co-evolutionary scenario. Accordingly, some brain innovations increasing language complexity might certainly result from biological changes (e.g., mutations in specific genes controlling brain development or neuron interconnection patterns).

Nonetheless, increasing evidence supports the view that specific language features can have a differential impact on selected cognitive abilities, such as working memory (Amici et al. 2019). This means that increasing language complexity resulting from cultural processes can eventually remodel our cognitive architecture, particularly if "cognitive gadgets" aimed to process language features more quickly and efficiently are implemented (Heyes 2018). Eventually, these changes can be fixed (and transmitted) via, for example, epigenetic marking. On this view, the statement that the language-ready brain precedes language is as problematic as the statement that language precedes the language-ready brain, and the answer to this particular chicken-and-egg question crucially depends on what we mean by "language."

However, this is also an area where a monolithic definition of *language* would not prove very useful—at best, it would allow for positing an arbitrary cut-off point in the sense that we speak of "language" as soon as a specific feature or set of features is available, but it would not help us understand the processes involved. Furthermore, this also implies that, as noted, even "language-readiness" can be seen as a gradual concept. A co-evolutionary scenario in fact allows for the possibility that there were further subtle biological changes since the first emergence of forms of language (Schoenemann 2009; Hurford 2012; Benítez-Burraco 2017), particularly, because our brain has been changing since our inception, reaching its present-day variation between about 100 and 35 kya (Neubauer et al. 2018).

4. Conclusion

While we do not agree with some of Mendívil-Giró's main points, we do agree with his general assessment that "the field of language evolution research is in good health," given fruitful debates about key concepts that are constitutive of our object of study—language. Just as Mendívil-Giró, we do not share Lewontin's (1998: 109) extremely negative attitude towards language evolution research, but

equally, we might agree with him that affirmations of "remarkable progress" are difficult to demonstrate—simply because a notion such as "remarkable progress" cannot be objectively measured and is very subjective. However, we also hold that the claim that we "know essentially nothing about the evolution of our cognitive capabilities" (Lewontin 1998: 111) is just as subjective and impossible to quantify and measure. Here we find that landmark publications such as Hurford (2007, 2012), Fitch (2010), Tallerman & Gibson (2012), or the launch of the CHIELD database (Roberts et al. 2020) are testament to the wealth of evidence, results, and knowledge that has been accumulated in language evolution research, which can hardly be said to amount to "essentially nothing."

As a case in point, this evidence can serve as useful constraints on hypotheses about the evolution of language (Johansson 2005). In addition, recent years have also seen a trend towards generating testable, falsifiable hypotheses about language evolution, as exemplified, for example, in Progovac's (2015) research programme (see also Progovac 2019). These developments can be seen as convincing indicators of scientific progress and we agree with Tamariz (2021: 513) that answers to "questions about language origins and evolution will come from the integration of knowledge from a variety of disciplines."

We started our original paper with several quotes—by Wescott (1991), Botha (2000), and others—to the effect that language evolution research must agree on a single definition of language as a *sine qua non* for progress. Such claims are very intuitive and *prima facie* very reasonable, but the essence of our original paper was to show that they are mistaken. This task was achieved by the lengthy section 4 of that paper, in which we demonstrated that several highly influential approaches to language evolution research—all of them undeniably central examples of latest research in this field—would not be able to agree on a single definition of language. In short, we again underscore that it is good to have explicit, clear and consistently applied definitions of your central terms—including language—*within* a paradigm or approach. This is as true in the field of language evolution as anywhere in science. However, at least at present, it does not appear to be possible to have a single top-down definition of language *across* all approaches and paradigms of language evolution. In particular, we would argue that our understanding of language should be based on the available scientific evidence, rather than accommodating the facts to one particular *a-priori* view of what language is and how it may have evolved.

This is why, if we ask the question of "what evolved"—which can be seen as the main question of language evolution research in its totality—the answer must simply be: language. This is despite the fact that such an answer would, in our view, be both undercomplex and overcomplex. It would be undercomplex because it would underestimate the complex set of cognitive prerequisites on which language builds, and it would be overcomplex for the same reason—unless we limit the scope of the term to specific aspects of language (such as FLN).

As for the definition(s) of FLN and FLB, we have defended our argument that the 2002 and 2005 definitions of FLN are incompatible. We have demonstrated that the 2005 definition includes elements that were stated as hypotheses before, and that the definitions are therefore only congruent if we conflate hypotheses and definitions. As our conceptualizations of language—both in

everyday discourse and in science—tend to include certain assumptions, it can be hard to keep hypotheses and definitions apart, but doing so is vital for enabling a fruitful exchange both between and within individual frameworks.

Author contributions

SH, MP, SW, ABB, and PZ jointly wrote the paper.

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Subcortical Contributions to the Uniqueness of Human Cognition: A Commentary on Laland & Seed (2021)

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1. Introduction

Laland and Seed (2021) address the issue of the evolution of human unique cognition. Having reviewed comparative evidence on five candidate traits—mental time travel, tool use, problem solving, social cognition, and communication—the authors conclude that no single trait could explain human superior cognition, and humans are probably cross-domain/modality/modular thinkers leading to a high-level intelligence which underlies human cognitive uniqueness. Such a comprehensively theoretical review attracts multidisciplinary readers, and the attempt to answer the question of whether human cognition is unique or not is highly significant in cognitive science. However, although the target paper provides numerous comparative data, we think that the continuous view of human cognition is not novel.

The solution the authors offer seems to be devoid of explanatory power. We agree that in general there is a continuum between nonhuman animal and human cognition, and it is not surprising that human cognition is superior because information from different modules interact. However, the authors fail to explain how different cognitive abilities interact and why there is still a gap between human language and animal communication systems. Take mental time travel as an example, various animals have been shown to have limited ability to recall the past

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and predict the future (Clayton & Dickinson 2010). However, the creative use of language enables humans to escape from current situations and produce meaningful utterances that refer to things and situations outside of the here and now. This is referred to as displacement (Hockett 1960, Bickerton 2009). It is worth noting that such displacement that we focus on in this paper is distinct from that repeatedly mentioned in the literature of generative grammar, where displacement is a property of linearized syntactic structure where phrases are interpreted in one place but pronounced in another.

We suggest that in order to achieve displacement, information from different cognitive domains needed to be encoded into lexical items. The combination of lexical items via syntax allows the generation of an in principle infinite number of different syntactic structures. Syntactic operations are assumed to be domain general, in the sense that they not only produce lexical items and sentences (Boeckx 2014) in the language domain but also extend to other domains like music (Shi & Zhang 2020) and movements (Pulvermüller 2014). In this commentary, we would like to focus on displacement, and suggest that domain general syntax serves as the underlying mechanism which enhances the combination of information from different cognitive domains. The importance of language in human unique cognition has been highlighted in the existing literature (e.g., Darwin 1871, Spelke 2009, Berwick & Chomsky 2016).

From a neurocognitive perspective, we would like to further argue that syntax beyond the language domain supported by the hippocampus and basal ganglia could play a key role. Moreover, we suggest that the interaction between these two subcortical structures in humans gives rise to the creative use of language that makes displacement possible which in turn lays the foundation for the uniqueness of human cognition. We are fully aware that neuroimaging and lesion studies point to the conclusion that syntactic operations are mainly supported by cortical areas and cortico-cortical connections (e.g., Friederici 2011). Nevertheless, subcortical regions have also been shown to be involved in syntactic operations (see Shi & Zhang 2020 for a review).

From an evolutionary perspective, subcortex is conserved across species, while neocortex is specific to mammals. Although it has been shown that neocortex is important for the evolution of high-level cognition, studies on birds who lack neocortices show that they exhibit high intelligence, like crows' tool use (Hunt 1996) and parrots' vocal imitation (Chakraborty et al. 2015), suggesting an important role of subcortex in cognition. On the other hand, if domain-general functions of the subcortical regions lay the foundation for domain-general syntax, it could be the case that in evolution cortical areas coordinate with subcortical ones to achieve a better efficiency of information transformation (Shi & Zhang 2021).

2. Displacement, Other Cognitive Abilities, and a Domain-General Syntactic Operation

Displacement is fulfilled through linguistic tools. For example, in the sentence *I played football yesterday*, it is clear that both the lexical item *yesterday* and the syntactic past tense *-ed* refer to an event that happened in the past. Both lexical items

and syntax are evolutionary novelties (Bickerton 2009). Syntax not only produces words but also puts words together into infinite combinations of phrases, clauses, and sentences which in turn are used to express our thought. As the carrier of displacement that makes mental time travel possible, syntax not only serves as the engine hub for lexical items and hierarchically structured sentences in the domain of language but can also be extended to other domains and cognitive abilities listed in Laland and Seed (2021). For example, syntactic operations have been suggested to be analogous to tool making and use (e.g., Stout & Chaminade 2012). Neuroimaging studies also suggest that syntactic operations and tool-making could share the same set of neural circuits (Hecht et al. 2014; Putt et al. 2017).

Besides, language has been assumed to play an essential role in problem solving (Baldo et al. 2005). Problem solving requires multi-facet abilities. For instance, inferential reasoning, as Völter and Cho (2017) noted, usually needs the transformations of mental representations to make predictions and the combination of spatiotemporally separate events. Hence, inferential reasoning is also closely linked to displacement, which enables humans to predict future events at the dimensions of both space and time.

In addition, as Laland and Seed state, language and language-related activities such as teaching play a crucial role in complex social cognition of humans. Social cooperation is closely related to displacement. For example, in the case of megafauna scavenging of ancient humans, when detecting a dead deinotherium, in order to persuade other members in the group to cooperate, the members must exchange information of where and when they found it, since only by themselves they cannot exploit it. This kind of high-end scavenging could have distinguished human ancestors from bone-crunching garhi and habilis.

Furthermore, Laland and Seed treat communication flexibility as the most obvious divide between humans and other animals. They highlight the syntactic properties "unbounded merge" (Chomsky 1995) and "recursion" (Hauser, Chomsky, & Fitch 2002) that underlie the creation of an infinite number of structures and the creativity use of language which in turn allows us to cope with different situations when we need to communicate with others. Hence, the syntactic operation serves as the prerequisite of human flexible communication.

Collectively, it seems that all five candidate traits reviewed by Laland and Seed are related to syntactic operations. This implicates that the domain-general thinking the authors assume could be realized with the advent of domain-general syntactic operation. We will focus on how the domain-general syntactic operation is supported by the subcortical regions in the following section.

3. Evidence from a Neurocognitive Perspective

At the brain level, high-level cognition could be derived from improved neural connectivity, diversification of cell types and general cortical enlargement in evolution (Striedter 2005). However, we would like to focus on how two subcortical regions, the basal ganglia and hippocampus, and their connectivity could have contributed to human unique cognition in the present commentary. Both areas exhibit domain-general cognitive functions. The basal ganglia have been assumed to be involved in motor planning and control (Wise et al. 1996), context-dependent

rule-based selection (Peigneux et al. 2000), and sequence learning (Chan 2007). The hippocampus serves as the hub for the interaction between semantic memory and episodic memory (Takashima et al. 2014). Moreover, the hippocampus is not only related to the storage of information from different cognitive domains (Tsao et al. 2018), but is also involved in the process of relational binding which is defined as

> rapidly, continuously, and obligatorily form associations among disparate elements across space and time, and further to enable the comparison of internal representations with current perceptual input.
> (Olsen et al. 2012)

Both areas are also implicated in language processing. It has been established that the cortical centered view of language network is insufficient to cover the updated data (Kensinger et al. 2001, Teichmann et al. 2015, Copland & Angwin 2019). For example, patients with impaired basal ganglia will have symptoms similar to non-fluent aphasia (Lieberman 2006). Further, if basal ganglia are affected along with cortical impairment, aphasic patients' probability to recover is lower (Crosson et al. 2005, Shi & Zhang 2020). Hence, the contributions of subcortical structures to language processing have received attention in cognitive and neurological research (see Shi & Zhang 2021 for a review). For example, the basal ganglia have been established to be related to syntactic processing of language (Kotz et al. 2003, Friederici & Kotz 2003, Progovac et al. 2018). Further, Boeckx, Martinez-Alvarez, and Leivada (2014) proposed that the basal ganglia are involved in the syntactic process of 'Linearization', the operation transferring hierarchical syntactic structures into temporal sequences. Shi and Zhang (2020) also provide more evidence for the functions of the basal ganglia in syntactic processing from a clinical perspective.

The hippocampus is involved in the process of lexicalization (Takashima et al. 2014) and lexical retrieval (Hamamé et al. 2014). Studies of developmental amnesia have shown that patients with atrophy of the hippocampus show difficulties of acquiring new semantic memory (Duff et al. 2020). Recent studies have also revealed that the hippocampus is involved in online syntactic processing (Piai et al. 2016). Further evidence suggests that the hippocampus seems to be the interface between language and memory (Shi & Zhang 2021). These functions imply that being the possible basis for displacement as well as lexical and syntactic operations, the hippocampus could play a crucial role when different cognitive abilities interact.

Since both the hippocampus and basal ganglia are highly conserved brain regions, some of their functions linked to displacement have also been found in nonhuman animals, but very limited when compared with humans (Shi & Zhang 2021). Shi and Zhang (2021) suggest that the reason why humans have superior displacement abilities can be partly due to the better coordination between the hippocampus and basal ganglia.

Furthermore, the functions of the hippocampus and basal ganglia are both domain-general, and the coordination between these two subcortical structures has been observed in learning and memory systems. For example, the hippocampus and the striatum (a subregion of the basal ganglia) were reported to be jointly

involved in episodic memory encoding (Sadeh et al. 2011). Increased functional connectivity between the hippocampus and striatum was also found in learning temporal associations (van de Ven et al. 2020). Their interactions also contribute to arbitrary associative learning (Mattfeld & Stark 2015). Moreover, the hippocampal-striatal interaction is evident in spatial navigation (Goodroe, Starnes, & Brown 2018). It has also been reported that the hippocampus and striatum both play crucial roles in decision-making (Johnson, van der Meer, & Redish 2007). The interactions between the nucleus accumbens (a subregion of the striatum) and hippocampus in rats were shown to be involved in decision-making about time tradeoff (Abela, Duan & Chudasama 2015). In the domain of language, Ullman's (2004) declarative/procedural model posits that declarative and procedural memory, supported by the hippocampus and basal ganglia respectively, interact with each other in first and second language learning.

Genetic studies also provide supporting evidence. Foxp2 was discovered as a gene affecting the coordination of speech production, together with problems in language production and comprehension in a family with fifteen relatives presenting verbal dyspraxia (the KE family; Lai et al. 2001). Two amino acid changes were detected in exon 7 of human FOXP2 when compared with the chimpanzee protein (Enard et al. 2002), suggesting that these two substitutions could have played a crucial role in human evolution. Such a humanized FOXP2 inserted in mice enhances the information transformation between procedural and declarative memory (Schreiweis et al. 2014), suggesting that the basal ganglia-hippocampal coordination could lead to better interaction among information from different cognitive domains, since both brain structures are involved in multiple cognitive domains. However, subsequent studies on FOXP2 revealed that the two mutations found in humans is shared with Neanderthals, thus the uniqueness of the human version FOXP2 become controversial (Fisher 2019). Nonetheless, it is less controversial that humans are the only species acquiring language and FOXP2 is in some way contributed to the evolution of human language.

By and large, since both subcortical regions are involved in syntactic processing, it is reasonable to propose that it is the domain-general syntactic operation that forms the basis for domain-general interaction.

4. Conclusion

All in all, we agree with Laland and Seed (2021) that human cognitive uniqueness arises from some combination of abilities, but we suggest that from the neurocognitive perspective, the domain-general functions of the hippocampus and basal ganglia play a key role. To be specific, we suggest that the enhanced coordination between the hippocampus and basal ganglia possibly support domain-general syntax which makes humans' cross-modular thinking possible. In the end, we would like to cite the following image:

> Another metaphor for the cognitive effect of human language would be the Swiss Army knife. Until language emerged, the minds of our ancestors were full of various tools, each tailored to specific needs. With language, all these tools were combined into a flexible all-in-one

> tool that makes available a variety of solutions (tools) whose effects can be combined spontaneously. (Boeckx 2010: 131)

Indeed, Laland and Seed (2021) also suggest that at the higher cognitive level, language could have enhanced the interaction between different cognitive domains, but when language is decomposed into subcomponents at the lower level, nothing seems to be unique to humans. This is consistent with the perspective of comparative biology that language *per se* is a very coarse term. In conclusion, we would like to propose that it is a domain-general syntax that could serve as the Swiss Army knife in human evolution and give rise to the uniqueness of human cognition.

Author Contributions

Both authors jointly conceptualized and wrote the paper.

Declaration of Interest Statement

The authors declare no competing interests.

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Corrigendum

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Correction for the Forum contribution by Shi and Zhang (2021) which was first published in *Biolinguistics* on 6 September 2021.

The authors note that the affiliation of Elizabeth Qing Zhang should list Sun Yat-Sen University in addition to Nicolaus Copernicus University, as well as the University Center of Excellence IMSErt – Interacting Minds, Societies, Environments in that particular order. Furthermore, the authors note that the corresponding author's e-mail address should read "zhangqing3@mail.sysu.edu.cn".

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Was Syntax Borrowed from Toolmaking?

Planer, Ronald J. & Kim Sterelny. 2021. *From Signal to Symbol: The Evolution of Language*. Cambridge, MA: MIT Press.

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1. Language vs. Cognition

The study of language evolution has gone from almost taboo to exuberant in just a few decades, with many new ideas and theories now proposed and debated. How language evolved is tightly linked to how human species evolved, as there is no more distinctive characteristic of our species than our language. Language is also the most tangible aspect of human cognition, the prism through which we get a glimpse into our other cognitive abilities. As put in Planer & Sterelny (2021, henceforth P&S), "since language is manifestly central to human life […] an account of the origins of human cognition and social life must include an account of the emergence of language" (p. xix). Consider this: When our otherwise immensely intricate, complex language abilities are just a bit off, just a mere 5%, we are judged as impaired, as having a disorder, or as not highly intelligent. We may be sent to therapy, and we are likely not to find the most desirable jobs or mates (both of relevance for natural/sexual selection). On the other hand, if we are not dexterous with using tools, if we are, say, 50% worse in our abilities than the typical person at, for example, assembling anything from Ikea or painting a straight line on the wall, we are still quite fine, certainly not considered as having a disorder. We can also be not musical at all, and not artistic at all, and we will not be considered as having a cognitive disorder. This is not to say—not at all—that we do not get attracted to good singing, or beautiful art and artefacts; this is just to say that these skills are precious (and relatively rare) additions to life, but not the

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necessities of life. Language, on the other hand, is a necessity of life, and it is not rare at all: It is everywhere where there are humans—all day, every day. This must be telling us something about our priorities, whether well-placed or not, which are certainly relevant for the considerations of genetic selection.

Nonetheless, interestingly, there is a tendency in current evolutionary theorizing to try to reduce language abilities to other cognitive abilities, and to argue that the genetic foundation or adaptation for specifically language is not there, or is not of any significance, and that language abilities (e.g., syntax), emerged culturally, simply by relying on whatever genetic predispositions or adaptations evolved for other abilities (e.g., stone toolmaking). As discussed in Progovac (2019a), this view (of no consequential genetic selection for language) seems to be characteristic of two completely opposing camps in the study of language evolution, the saltationist (one-mutation) camp (e.g., Berwick & Chomsky 2011, 2016), and the gradualist cultural evolution camp. As stated in Steels' (2011) overview, adherents to cultural evolution consider that language features *per se* do not originate through genetic evolution—and are therefore not linked to biological fitness. P&S's book falls into this latter category. It is an expertly written and intricately argued monograph, which aims to integrate a host of pieces and details from a variety of fields into a coherent story about human evolution. It is an erudite piece of scholarship, but still presented in a highly accessible style, able to appeal to a wider readership.

P&S put emphasis in this book on the evolution of cooperation and on social factors, and their strategy is to "nest an incremental view of the emergence of language within an equally incremental view of the evolution of human social life" (p. 25). In other words, the aim of this book is to elaborate on the lifeways of ancient hominins in such a detail that would allow them to deduce the cognitive and communicative capacities, including the capacity for language, needed to support these lifeways. P&S consider that the theory of mind, causal reasoning, hierarchical structure, and vocal control all had to be in place before language emerged, so that language could then emerge culturally. The recurring theme of their monograph is the gradual evolution of culture, language, and cognition more generally. The two main strengths of this monograph are (i) its plausible and detailed outline of a gradual evolution of human lifeways and culture, and (ii) its systematic attempts to correlate various lifestyle milestones with the cognitive and linguistic milestones. Regarding (i), P&S make a good case for language and cognitive evolution as a gradual, step by step process, where each stage needs to rely on the previous one, bringing some small but tangible advantages. This is not only the most feasible evolutionary pathway toward complexity, often extravagant in the case of language, but typological variation across languages is better captured within this view (see Section 5), in comparison to the view that syntax sprang into existence suddenly as a result of one single mutation, maintaining uniformity across all cultures (e.g., Berwick & Chomsky 2011, 2016). P&S's stated goal is to show that language evolution does not need to rely on a miracle, that is on an inexplicable jump in complexity (p. 212). I cannot agree more with their view that language (and cognitive) evolution was gradual, continuous with various precursors and pre-adaptations, and that it involved a multitude of factors acting simultaneously and synergistically upon one another. Where I believe modifications

are needed, as I outline below, is in the nature and the role assigned to the early stages of language and syntax in this gene–culture spiral that led to language, specifically the role of language itself in its own evolution, both cultural and genetic.

Regarding (ii), this book is rich in claims and details concerning possible causes or correlates of language evolution, advocating important roles of phenomena as diverse as: Obligate bipedal lifeways (which freed hands for gesturing),[1] hunting, tidal foraging, population size, singing, the control of fire, and especially stone toolmaking. P&S conclude by saying that their aim "has not been to provide an exhaustive survey of the options, still less to endorse one" (p. 211), and that instead they "strongly suspect that the *Sapiens* expansion and transformation of cooperation […] depended on the simultaneous effects of a number of factors" (pp. 211–12). However, there are two broad points I would like to make in this respect. First, among all these factors, there should certainly be room for one more player, and that is the contribution of early forms of language itself, with all its amazing detail. Secondly, in order to test any of these claims, which the authors themselves often characterize as speculative but plausible, and to move from plausibility to evidence and proof, one does need to converge on (or endorse, if you will) a specific (linguistic) proposal—specific enough that it can be rendered into specific hypotheses to be tested.

It is not my intention (nor ability) to respond to each of the claims P&S make in their monograph, which are many and various. Instead, I have selected a few that seem important and influential in the field of language evolution, and that are closest to my own expertise as a syntactician and linguist. One of them is the role that toolmaking might have played in the evolution of syntax.

2. Was Syntax Borrowed from Stone Toolmaking?

One of P&S's main arguments in the monograph is that the cognitive capacities for syntax, specifically for creating sentences, were borrowed from stone toolmaking (p. 119), in the sense that the genetic foundation for this ability was set in place by selection for the ability to make stone tools, and that, subsequently, the evolution of syntax itself simply advanced through cultural processes, needing no further genetic adaptations. According to them, the development of these tool technologies depended on the expansion of hierarchical control and were thus selected for computational capacities that "made it possible for hominins to fluently use hierarchically structured sentences" (p. 148). In other words, "the computational machinery underpinning hierarchical structure evolved in the service of technological skill, the production of sophisticated stone tools in particular" (p. xvii). Below I give three reasons why reducing syntax to stone toolmaking cannot be the right approach.[2]

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1 In this respect, P&S advocate a gesture-first hypothesis for language evolution.

2 Interestingly, P&S state that animal communication is about the immediate scene, but that human communication is not. Human communication is certainly not always about the immediate scene, but it can be, and often is, when we say, for example, 'There's a racoon right there!' or 'What a lovely sunset!' or 'Drop that gun!' or 'Eat your broccoli!' or 'You look worried.' I am not just saying this to point to an imprecision, but rather as an excuse to point out

There is no doubt, of course, that any new cultural invention that gets spread across a population relies on prior adaptations, and on the general pre-existing abilities. So, it makes perfect sense that tool use of the kind and magnitude manifested in humans has not been invented by say birch trees, for example, tools that would allow them to draw water from a nearby lake in case of droughts, no matter how adaptive this may be for them. So, stone toolmaking had its own many and various precursors and pre-adaptations, including bipedal mode. And if stone toolmaking had to rely on such pre-existing abilities, then, of course, it is expected that the evolution of human syntax, and human language more generally, also needed to be consistent with all sorts of predispositions, including those overlapping with toolmaking. To be completely clear here, and not be misunderstood, I am not saying that human syntax was some kind of evolutionary fluke that was brought about by a single random mutation, which may be the view of the saltationists cited above. In my own work, I have argued consistently against such a view, and, one more time, I have no doubt that pre-adaptations for syntax, and continuity more generally, are crucial. But that certainly does not mean that these predispositions or pre-adaptations are all there is to the evolution of syntax, in the genetic sense, and this also does not mean that stone toolmaking was what caused or enabled the evolution of syntax. Not at all. Here are three reasons, in addition to the reasons given in Section 1, why this line of reasoning does not follow.

First, it is perfectly plausible that both phenomena—that is, both toolmaking and syntax—rely on some more ancient shared predispositions that predate both, as P&S themselves suggest later in the book (p. 141).[3] Second, it is also possible that the early emergence and entrenchment of proto-grammar (perhaps of the kind described in Section 5) contributed to the evolution of Broca's area in a way that also helped develop more sophisticated means of toolmaking; the two could have certainly been engaged in a feedback loop, both contributing to the evolution of Broca's area. Third, the existence of a pre-adaptation for some ability does not at all preclude the possibility for further genetic selection and adaptation for the newly found function, meaning that it was entirely possible that each of these skills, toolmaking and the use of grammar, have been subject to genetic selection in their own right. I elaborate below.

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that language that is less displaced (more immediate) can often be shown to be grammatically less complex, as discussed with some specific small clauses (i.e. mini sentences) in, for example, Progovac (2015; see also Newmeyer 2005 for subordination). This is just one place where linguistic detail is important and useful in reconstructing the linguistic past with some precision, and which shows that syntax/language does not simply reduce to binary oppositions such as displaced vs. not; hierarchical vs. not, as further discussed below, as well as in Section 5.

3 P&S themselves point out that the origins of enhanced action planning and control must be deeper than hominin toolmaking, as their precursors are found in other primates (p. 141). So, for example, premotor cortex F5 in Old World monkeys is considered homologous with Broca's area in humans, as it, too, is recruited not only for motor behavior, but also for visual and auditory processing (p. 141). If so, then syntax, to the extent that it depends on these motor and multimodal abilities, did not have to wait for stone toolmaking to be perfected, or to rely on this development directly, as there was a previous, common foundation/pre-adaptation for both.

In other words, regardless of the ultimate precursors and pre-adaptations (which every newly emerging phenomenon will have), syntax (and language more generally) would have also, itself, been subject to natural/sexual selection forces, and would have thus contributed to the genetic make-up of humans, which in turn would have contributed to further enhancement of various other cognitive phenomena. As put in Pinker & Bloom (1990), it is impossible to make sense of highly complex phenomena that are especially well designed for a specific function, such as is the structure of the eye, without acknowledging that it (the eye) evolved for the purpose of seeing; evolution is the only physical process that can create an eye because it is the only physical process in which the criterion of being good at seeing can play a causal role. Just like being good at crafting tools would have improved that very ability through generations, including by genetic selection, so would, for sure, being good at using language and syntax, and the discussion below will touch upon just how immensely complex that ability is, and how reliably and effortlessly it is acquired. As P&S themselves state, citing West-Eberhard (2003), "genes are the followers, and not the leaders, in evolution […] selection will favor the genetic variants that acquire the behavior more rapidly or reliably […]" (p. 27). I cannot agree more, but then why should this not also apply to syntax, and language more generally? Why would the evolution of syntax follow some genes, set in place for something else? Isn't syntax acquired rapidly and reliably? Isn't it in fact acquired much more rapidly and reliably than toolmaking? Why would syntax or language abilities be exempt from these routine processes of evolution?

As a syntactician (although as somebody who knows very little about toolmaking), I do need to point out that there is so much more to syntax than what motor control, toolmaking, and syntax may have in common, and I am sure that there is also more to stone toolmaking than what it shares with syntax.[4] Contrary to what is often claimed by saltationists such as Berwick and Chomsky (see, e.g., Berwick & Chomsky 2011, 2016), syntax (or language more generally) does not reduce to the hierarchy-creating Merge, which basically allows one to combine and recombine, which may be what prompted this idea that syntax reduces to assembling tools. Human syntax is a composite of various hierarchical and flat constructions, seamlessly interwoven together, some systematic and some rather quirky, with a multitude of abstract grammatical categories whose existence typically arises through (gradual) grammaticalization processes, and which have their own predictable place in the tree of syntax (see Progovac 2015 for a detailed reconstruction and decomposition of various syntactic phenomena; also Progovac 2019b for Merge and Minimalism more generally in the light of evolution). And, above all, syntax is acquired effortlessly and reliably by almost all people (barring certain language disorders), and is processed also reliably, and with lightning speed, none of which is the case with toolmaking. Even though P&S state that their strategy is to "nest an incremental view of the emergence of language within an *equally* incremental view of the evolution of human social life" (p. 25), I see

4 Reducing syntax to toolmaking must have some consequences, and I would like to know what its predictions are and how they would be tested. Are people who are especially good at grammar also especially good at tool use or toolmaking? Do cultures that do not make stone tools not have the ability to process complex hierarchical language?

imbalance in their book: While one side of this equation has been presented with good detail, the social lifeways side, the incremental emergence of syntax or language has not been presented equally at all, as I further elaborate below, as well as in Section 5.

P&S's Section 5.6 goes in great detail over the stages through which tool technology passed. It expresses, and rightly so, admiration for the achievements in tool technology, specifically in the third stage, starting at around 800,000 years ago, concluding that "the degree of manual precision and foresight shown by these toolmakers is astounding" (p. 144). I completely agree. But I hasten to add that such tools, or any other tools for that matter, are no match for the sophistication of human syntax and language more generally, and for how fit our brains are specifically for processing them. We can just take a look at one chapter of P&S's book and consider how many words and sentence structures just this one chapter offers, and how many totally different sentences with different meanings. And just consider how much these sentences have been able to convey to the reader about a topic as amazing (and bizarre) from the point of view of nature as are the evolutionary developments in human and hominin lineages, describing the beings we have never seen (early hominins), and the times whose depth is hard to fathom. But it is all made quite accessible to us through language, as are all sorts of other topics. For syntax does not just assemble two dead pieces of stone or wood that are lying around; it assembles words and morphemes, each alive and bursting with meaning and connotations, and it can assemble sentences with a hundred such words, or more, placing each with incredible ease and precision in its designated hierarchical spot, thus constraining their meanings and functions to converge on a precise message. And for this one-hundred-word sentence that our syntax can so easily assemble, in a matter of seconds, it is not just that the words and morphemes are lying around like bricks waiting to be picked up and built into a house. Not at all. These words and morphemes themselves are creations of our linguistic mind, stored in our mental dictionaries, and we command on average tens of thousands of such words, and often even more. So, when we create this one-hundred-word sentence, each word must be carefully selected among many alternative possibilities, and these choices depend not only on the meaning, but also on the multitude of various syntactic particularities that words carry with them. But our syntax juggles all these words, thousands upon thousands of them, hardly ever dropping a ball. This cannot possibly be a trait that did not need any genetic adaptation beyond the abilities of motor control and tool assembly. I think that we often forget just how much work syntax does for us, and how much we take it for granted.[5]

3. What Kind of Feedback Loop?

Language is specifically discussed in P&S's Chapter 7 "From Protolanguage to Language." Here, the authors state that "if only late Pleistocene anatomically

5 P&S refer to "the easy problem of syntax" (p. 126), which "poses no deep mysteries" (p. 129). There is truly nothing easy or straightforward about reconstructing the evolutionary trajectory of syntax, once its details and variability across languages are taken into account.

modern humans (AMHs) had full language, we need at least a tentative explanation of that fact. We propose it was because only those humans cooperated so extensively" (p. 184), suggesting that cooperative lifestyles caused language to emerge. However, as will be elaborated below, cooperation cannot cause language to emerge, but it can facilitate its solidification, once language starts emerging, which means that some kind of feedback loop had to be in place to facilitate the evolution of both language and cooperation in humans. But what kind of feedback loop?

P&S invoke the self-domestication hypothesis of human origins, as a mechanism for decreasing reactive aggression in our species specifically (p. 205), by selecting against aggressive individuals. This is a promising line of inquiry because the propensity for reactive aggression, related to (lack of) cooperation, leaves a fossil signature, and has a genetic foundation. However, if we conclude that the reduction of reactive aggression somehow caused language, rather than that the concurrent emergence of early forms of language actually engaged in a feedback loop with self-domestication (see below for this type of proposal), then we are at square one again: because neither bonobos (a self-domesticated species), nor other super domesticated animals, such as cows or chicken, have developed language, certainly not of the kind that humans have. For such an immensely intricate, rich, and complex phenomenon as human language to emerge on an evolutionary scene, there had to be multiple factors acting simultaneously, and synergistically, one upon another, including selection for the emerging ability to use language. Language itself should certainly not be excluded from this synergy—why would it be?

In fact, it is fully expected that the cultural emergence of early forms of language was a crucial contributor to both language evolution and cooperation/self-domestication. A specific proposal advocating such a feedback loop, involving reduction in reactive aggression (via self-domestication), has been developed in Progovac & Benítez-Burraco (2019) and Benítez-Burraco & Progovac (2021). In this view, the ebbing and flowing of reactive (and proactive) aggression are seen as closely tied to the different stages of language evolution, with the emergence of verbal aggression/insult (associated with simplest syntax) constituting an important milestone in the gradual transition from physical fighting to verbal/cognitive contest.[6] P&S themselves conclude that self-domestication itself (with its reduction in reactive aggression) can only be a part of the story, as another factor is needed (p. 208). I completely agree. But the other factor, the elephant in the room, can certainly be the emergence of early forms of language, respectively, syntax.

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6 P&S state that early proto-language was used to facilitate cooperation and coordination, but that today we use language also for teasing each other, to tell a joke or a story (p. 47). However, there is no reason to exclude the relevance of joking and teasing (which can be continuous with insult/verbal aggression) from earliest proto-language, as they would have been especially adaptive at these early times, and are also more continuous with our ancestors' mindsets, providing more graceful evolutionary continuity (see e.g. Progovac 2015, 2016). Other primates seem quite capable of teasing and joking. To take just one example, as reported in Patterson & Gordon (1993), the gorilla Koko was not only capable of producing novel compounds (i.e. two-slot compositions), but also of using them for insult, playfulness, and humor.

Even though P&S's main argument is that language abilities are parasitic on other cognitive abilities, and on the genetic foundation put in place to support those other abilities, they seem to doubt themselves occasionally, and I find this promising. For example, they allow for the possibility that "natural selection has equipped us with many language-specific cognitive adaptations" (p. 112). That would mean that, after all, there was genetic selection for language features specifically. Then, later, P&S state that "this is not to say, however, that language has not exerted an evolutionary influence on the structure of this computational system. […] Indeed, we would be most surprised if the evolution of language made no difference to these control and recognition mechanisms" (p. 150). Indeed, and for that reason, one does need to enter the specifics of language into this feedback loop, for it is not enough to just mention the adaptiveness of language here and there, in generic terms. One does need to bite the bullet and hypothesize (and then test) just exactly what kind of language, and what kind of syntax, was subject to genetic selection, and how it interacted with, and contributed to, all the other evolutionary developments.

4. Who Needs Language and Who Doesn't?

Another line of reasoning that I do not find convincing are the arguments to the effect that hominins developed language because they 'needed' it. For example, P&S state that the hominins that lived after about the first third of the Pleistocene were "both capable of using a protolanguage and very likely needed a protolanguage, one with flexibility and displaced reference" (p. xvii).[7] But, in truth, who would not need language?[8] If somehow bonobos (who have been argued to be self-domesticated, too) were to manage to develop some (proto)language, would that not be highly adaptive, and would not that seem in retrospect as something that they needed? In fact, they seem to need it really badly for survival, as so many other species do. Nature does not just give a species or individuals what they need. Evolving something as useful and complex as language had to be partly a matter of chance or some "lucky break" as put in P&S (e.g., cultural emergence of simple forms of grammar), partly a matter of many other factors emerging and converging at the same time, in a positive feedback loop (e.g., reduction in reactive aggression; various environmental factors), and partly a matter of ruthless genetic selection for the phenotypes whose brains were just a bit better equipped to use and learn these newly emerging forms of language. For that reason, a strong, specific version of a gene-culture feedback loop involving the specifics of language is needed.

While P&S occasionally state that there was a coevolution of social and linguistic complexity (211), and that "advances in hominin communication systems […] fed back to further transform cooperation, creating a positive coevolutionary

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7 P&S also state that "mid-Pleistocene hominins needed to be able to add arbitrary signs to their lexicon, if their social capacities made that possible" (p. 216).

8 It could be that the use of the word 'need' in this respect was a shortcut for something else, but I mention it here because it may be reflective of P&S's unidirectional, deterministic view of language evolution, portraying it almost as an inevitable consequence of other evolutionary developments (that needed it).

feedback loop" (p. 181), they at the same time systematically advocate a unidirectional view especially when it comes to genetic evolution, where it is not clear what, if any, the return effect of early forms of language would have been. For example, on the same page, they say: "We have considered several factors that might explain the fact that sapiens found themselves on a trajectory toward hyper-prosociality, and hence full language," suggesting again that language was a result of human pro-sociality, and not a contributor to it. Similarly, on the final page of the monograph, P&S state that perhaps only sapiens developed "the complex forms of cooperation that required the resources of full language" (p. 222). P&S do acknowledge that their connections between language and social lifeways are imprecise and speculative, and that the specifics of language are not dealt with in the book (e.g., p. 214). The next section aims to show that the devil is in the detail…

5. Some Details of Proto-Syntax

P&S do consider some details of syntax, which is welcome, as without the detail one cannot start a meaningful discussion on this topic. For example, they consider that fixed word order (SVO or subject–verb–object) was the initial state of proto-language, and that some languages later developed other means of marking these distinctions, such as case and agreement (p. 118), leading to the subsequent loss of fixed word order in many languages. In this respect, their proposal is related to Jackendoff's (2002) Agent First proposal. P&S link this proposed stage of linear SVO syntax to archeological traces of enhanced forms of cooperation (p. 128). However, if proto-languages already converged on such a straightforward way of expressing who does what to whom with a simple SVO ordering, it is not clear at all why they would have lost this excellent solution. Why do many languages, as they point out, have free word order, and even more languages (certainly more than half) show other word order types, distinct from SVO? And why have so many languages developed all these complicated and often baroque agreement and case systems, whose main purpose is again to distinguish subjects from objects? At the very least, it seems safe to conclude that starting with a proto-language with the fixed SVO word order hardly provides that baseline for incremental evolution which P&S are seeking, the baseline which would lead to a next stage, and then a next stage, and where each stage brings some tangible advantages. This just highlights the difficulty of identifying such baseline for syntax without considering the details of syntax, as well as the details of syntactic variation across languages.

In my own reconstruction of proto-syntax, I have relied on some stable theoretical, as well as typological, postulates for syntax. This led me, surprisingly, to the earliest stage of syntax which does not in fact show any subject/object distinctions, and only operates with a two-slot syntactic mold initially, a small clause, with only a verb and one single argument, not differentiated for semantic/thematic role (see Progovac 2015, 2016, for many examples of such constructs across present-day languages, which can serve as proxies of this stage). It would have been only later, and based on this foundation/baseline, that transitivity in languages emerged, by adding one additional argument, but in diverging ways in

different cultures, yielding, for example, two main case-marking types: nominative-accusative and ergative-absolutive types. In addition, some cultures developed serial verb constructions, exhibiting sequences of two (or more) small clauses, the first one typically introducing the agent with a verb, and the second one introducing the patient with a verb. Starting with a two-slot small clause baseline (or common denominator), which operates with only one argument, goes a long way toward explaining this profound cross-linguistic variation in expressing transitivity, where each solution nonetheless involves only a small tweak of the baseline. This kind of approach not only lends itself to incremental evolution of syntactic complexity, but it is also specific enough to be testable.

Even by looking at this one specific syntactic example of transitivity across languages, one can see that humans across cultures struggled to just develop and grammaticalize subject/object distinctions, again something we take for granted, and one can also see that the reason why transitive grammars evolved can be directly linked to incremental communicative advantages, rather than being a mere consequence of a generic ability to assemble and reassemble things hierarchically, i.e. to merge and remerge. In other words, in this view, it was the struggle to develop and grammaticalize various linguistic distinctions, including subjecthood/objecthood, that led to the layered syntax (by tweaking the baseline), rather than hierarchy emerging for its own sake (or for the sake of stone toolmaking), and then these linguistic distinctions just populating these hierarchical layers, as some kind of ready-made products. There is a lot of discussion of this phenomenon in Progovac (2015, 2016; see also Progovac & Benítez-Burraco 2019), accompanied by an attempt to connect this specific proposal to the hominin timeline.[9] As do P&S, I also believe that specific interdisciplinary cross-fertilizations between linguistic and archeological and anthropological reconstructions are necessary, and I am sure that there is plenty of room for improvement in this respect on my end, just as I have suggested there is on the P&S's end. But that just means that more dialogue is necessary across different disciplines, as well as more testing, and that the details and reconstructions on both ends, both linguistic and archeological and anthropological, are absolutely necessary to get to the bottom of human evolution. In this endeavor, there is just no escape from the details of language, or from linguistics, for that matter.

Declaration of Interest Statement

The author declares no competing interests. A copy of the reviewed book was provided by MIT Press free of charge.

9 In a nutshell, in Progovac (2015, 2016), I have proposed, based on these profound differences in the expression of subjecthood, and transitivity more generally, that the hierarchical stage, and transitivity more specifically, did not emerge in all its complexity and in a uniform fashion only once (in Africa), but instead multiple times, and independently, either within Africa, or after the dispersion from Africa. This has specific implications for the timing of the emergence of hierarchical syntax, as well as for the hominin timeline.

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FORUM

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Notice

Biolinguistics Editorial Team*

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Another year is coming to an end, and so is another volume of *Biolinguistics*—volume 15. While we were only able to publish a small number of pieces in 2021, a fair number of submissions are still undergoing reviews or are being revised. On top of that, much of this year was spent preparing exciting changes to come in the new year and other outreach activities.

For example, I (Grohmann) just "returned" from Beijing, where the *5th International Conference on Biolinguistics* was held (ok, it was online: December 25–26). ICB 5 was jointly organized by two journals, *Cognitive Linguistic Studies* and us here at *Biolinguistics*, under the auspices of the School of Foreign Languages from Beijing Institute of Technology, co-sponsored by the *Journal of Beijing International Studies University*. We are certainly open to do more such collaborative event formats in the future (though it need not necessarily be over Christmas).

A large chunk of our time was also dedicated to further improvements, in parts following up on what we started last year already. With our involvement in the Free Journal Network (*Biolinguistics* Associate Editor Patrick C. Trettenbrein was elected in 2020 to the FJN Board of Directors), we carry on contributing to worldwide efforts to strengthen diamond/platinum open access journals. With continued funding, standards for FJN membership will steadily be increased, with two main points in the future being the provision of DOIs and a solid archiving policy. *Biolinguistics* will be poised for these eventualities thanks to our second major administrative development this year.

As it happens, the biggest and most tangible change for *Biolinguistics* will be a new hosting service for the journal. We will keep running on open-access journal software (OJS/PKP). But very soon, we will be integrated into PsychOpen GOLD, the European open-access publishing platform for psychology. Once this transition is completed, *Biolinguistics* will be run on a state-of-the-art architecture which includes DOIs, indexing, a modern journal system, and more. Not only will this meet all of FJN's requirements but it should also attract more submissions as DOIs will give the journal increased visibility and accessibility. We are grateful to PsychOpen GOLD that our journal was selected to be hosted, and thereby funded, in their growing repertoire. The transition will affect some of the database information, but we will announce details in due time to all users of *Biolinguistics*.

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To return to publication-related numbers, *Biolinguistics* received almost 50 *bona fide* submissions, not counting a large number of fake submissions which we hope to exclude more easily with the new platform. Still, most of these were completely irrelevant, dealing mostly with literary studies, and resulted in 42 desk rejections. Only counting (bio)linguistically oriented submissions, three papers were rejected after review, two submissions were published after review, and one was accepted after solicitation (a book review).

On average, it took 53 days to accept a submission for publication after sending it out for peer-review. For 2021, we are talking about a very small number of papers indeed, yet one major issue we are and have been facing at *Biolinguistics* concerns response time and responsibility of reviewers: In order to secure at least two reviewers for a given submission, we frequently have to approach five potential reviewers or more. And while many often take longer to provide the review (which is ok in pandemic times but is something we experienced before as well, of course), some reviewers never deliver despite agreeing to carry out this important task. This mirrors what was identified more generally in a recently published commentary (D'Arcy, Alexandra & Joseph Salmons. 2021. Peer review in linguistics journals: Best practices and emerging standards. *Language* 97.4: e383–e407).

Another relevant type of statistics concerns the number of downloads. Here, too, we experienced some technical difficulties due to the provider since the last migration three years ago. But we can say with some certainty that all published pieces are downloaded several hundred times, most even around the four-figure mark: Over the past 6 years alone (since volume 10), 20 pieces were downloaded over 1,000 times, two of which even more than 2,000 times—not including some counts that were lost in the last migration process. Given that only around 500 people are registered in the system, this is a great testimony to the success of true open-access publishing.

Finally, we are extremely grateful to our colleagues who agreed to review for *Biolinguistics* throughout 2021, the second pandemic year which impacts all of us. They are listed below by name in alphabetical order. For everything else, to close in customary fashion, we thank all our authors, readers, and supporters as well as the members of the *Biolinguistics* Advisory Board, the Editorial Board, and the Task Team for their participation and feedback, which is so crucial to the success of the journal. We appreciate your service to our scientific community.

Reviewers

Andreas Blümel
Barbara Citko
John Collins
Koji Fujita
Wolfram Hinzen
Hisatsugu Kitahara
Gereon Müller
Robert Scholz

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