The “grammar” in Universal Grammar: A biolinguistic clarification

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ABSTRACT This short piece addresses the confusion over terminology that has reigned, and partly still reigns, when it comes to the concept of Universal Grammar (UG). It is argued that whilst there might be changes in terminology and theory, conceptually UG cannot be eliminated. From a biolinguistic perspective, UG is not a hypothesis by any rational epistemological standard, but an axiom. Along these lines, the contemporary evolutionary perspective on the language faculty (FL) is briefly discussed to then argue that UG is necessarily part of FL in both a narrow and broad sense. Ultimately, regardless of terminology, UG is inevitably one of the factors determining the growth of FL.

Keywords: Universal Grammar (UG), Faculty of Language (FL), I-language, language universals, minimalism, evolution of language, biolinguistics, cognitive science

1 Introduction

Despite having been in active use by (mostly generative) linguists for a long time, the technical term Universal Grammar (UG) has led to ample confusion amongst linguists and non-linguists alike. As a consequence, Boeckx and Benítez Burraco (2014) have supplanted the label UG with talk about the “language-ready brain.” Yet, conceptually, nothing has changed: A language-ready brain is a brain shaped by UG. In turn, UG is defined as genetic endowment with regard to language (Chomsky, 2005) and is one of the factors influencing the initial growth and further development of the human language faculty (FL). This short piece addresses the confusion over the “grammar” in UG because both, terminology and the concept of UG, have been subject to a multitude of debates and misunderstanding.

2 Confusion over terminology

With regard to terminology, UG was labelled as such as an homage to the ideas expressed in the *Grammaire générale et raisonnée*, the Port-Royal grammarians, and the Western philosophical tradition associated with them (in this context see McGilvray, 2009, pp. 24–35). Originally, the term UG was used by (mainly generative) linguists to denote whatever it turns out to be that humans bring along to the process of language acquisition. It was understood that the properties of FL are ‘[...] determined by the nature of the mind’ (Chomsky, 1967, p. 9). Decades after the cognitive revolution (Miller, 2003), presumably due to the success of Government and Binding theory (Chomsky, 1981/1988), the term UG acquired a narrower and more specific meaning, denoting merely the genetic endowment enabling language growth (this change in terminology is critically evaluated by Boeckx and Leivada, 2014; for a more general summary of discussions on the past and present of UG also see Irurtzun, 2012, as well as the 2014 *Language Sciences* special issue).

Generally speaking, the label UG has lead to ample confusion. Non-linguists as well as researchers who oppose the “biolinguistic research agenda” (Hauser and Bever, 2008) that deems linguistics

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kind of a “theoretical biology” (Sklar, 1968) would often take the term *Universal Grammar* literally, thus interpreting it to imply that there in fact are aspects of “grammar” in all of the world’s natural languages that are universal and supposedly hard-wired in the genes of cognitively modern humans.¹ A prime example for this misinterpretation of UG that leads to a misconception of what the technical term actually denotes can be found in the early, and still in the recent and otherwise quite substantial work of developmental psychologist Michael Tomasello (e.g., 1990, 2004, 2005, 2008, 2009). Likewise, the work of some rather prominent linguists constitutes no exception (e.g., Evans and Levinson, 2009).

2.1 Does language exist?

Tomasello has repeatedly attacked the concept of UG because he thinks the existence of a genetically endowed FL to be unnecessary, implausible, or even impossible (2005). Along these lines, he has also argued that UG in fact constitutes an unfalsifiable hypothesis (Tomasello, 2004) and has recently pronounced UG “dead” (Tomasello, 2009). He himself favours accounts of language acquisition and learning that he labels as “usage-based approaches” (Tomasello, 2005, p. 191) which, according to him, involve only “one set of general cognitive processes” (Tomasello, 2005, p. 193) that are not domain-specific so that language development is guided only by semantic and pragmatic cues during acquisition. As Tomasello is a quite prominent scientist, his ideas on UG deserve consideration and simultaneously urge refutation (for a concise and to-the-point rejoinder also see Bolender, 2010a).

Tomasello is right insofar as to state that the idea that there is a UG can hardly be falsified when he deems UG a hypothesis in his 2004 commentary paper. Yet the reason for this is that UG is *not* a hypothesis by any rational epistemological standard. In point of fact, it is an axiom par excellence. In linguistics and related disciplines, this axiom named UG simply constitutes the technical term denoting the language-related properties of the human genome (by and large yet to be identified) which enable and aid the development of a fully grown and functional FL (in normally developing specimen); an actually self-evident claim as only humans are capable of (fully) acquiring human language. In this sense then, postulating the existence of UG almost amounts to stating a truism.

Consequently, questioning the existence of UG amounts to questioning the existence of human language altogether. What Tomasello and other critics then might rightly question and refute is the different accounts and theories of what is and is not part of UG that have been put forward, something that has been done repeatedly in the history of modern linguistics. Actually, in a recent lecture (2011a) and again in a subsequent paper (2013), Chomsky argues that Tomasello’s position ultimately amounts to exactly this, denial of the existence of language as an object of serious scientific investigation. Interestingly, in a commentary on a paper by Pinker and Bloom (1990), Tomasello indeed remarked that “[m]any scholars of language and language acquisition (myself included) would not agree [...] that there is such a thing as a language ‘faculty’ [...]” (1990, p. 760).

¹ Reviewer 1 asked whether the author thinks that (1) a biolinguistic approach always presupposes UG, as well as whether (2) a non-generative approach to biolinguistics at large would also be possible? As to (1), from the point of view that I am advocating here, a biolinguistic approach necessarily presupposes UG, though it might choose to employ a different term (e.g., “language-ready brain,” etc. pp.). If this were not the case, biolinguistics could not account for why only humans have language. Regarding (2), while I have taken generativism and biolinguistics to be synonymous here (as do many if not most researchers, something Martins and Boeckx, 2015 have criticised), I do not think that a biolinguistic approach necessarily presupposes generativism. However, as I consider the generative tradition to be the most advanced research program in contemporary linguistics that, by and large, actively seeks to make linguistics a branch of biology, I think that there is nothing wrong with employing the terms synonymously, as long as one sees work within generative linguistics as the basis for more interdisciplinary cooperation in the “strong sense” of biolinguistics of Boeckx and Grohmann (2007).

2.2 The contents of Universal Grammar

It appears as if Tomasello's dissatisfaction with UG and (generative) linguistic terminology arises from the fact that in his understanding, UG must contain statements of grammatical nature which are specific to language and can be found in all observable natural languages (i.e. E-languages; see Chomsky, 1986). Renowned examples of work that has employed such an E-language approach to language universals are Greenberg (1966) and Comrie (1981/1989), whereas the universals identified there actually reveal that linguistics has to "dig deeper." Piantadosi and Gibson (2014) have recently deciphered the limitations of typological statistics in establishing E-language universals, yielding akin implications. Thus, (generative) linguists have actually set out to "dig deeper" when they determined I-language(s) to be the object of investigation.

Accordingly, linguists generally distinguish Greenbergian (i.e. E-language) universals from Chomskyan universals, whereas only properties of the cognitive system (i.e. I-language) are truly universal. Now, even within this I-language approach we can still easily accommodate linguistic variation by introducing the notion of open parameters in UG principles (Chomsky, 1981/1988), as well as the idea of parameter setting during language acquisition (Ayoun, 2003/2005; Crain and Thornton, 2012). While the exact nature of parameters (binary, preset, etc.) is still subject to debate, so that the notion is used in different ways in the literature (Lasnik and Lohndal, 2010) and it remains unclear whether they are part of UG and thus "hard-wired," arise from experience, or possibly emerge during development; it arguably was due to the principles and parameters (P&P) approach that linguists could account for linguistic diversity for the very first time (Boeckx and Piattelli-Palmarini, 2005).

The idea that UG would contain hard-wired E-language properties is, of course, highly implausible if not absurd (especially in an evolutionary context, as I shall discuss below). In fact, if this were the case, modern linguistics could not account for language diversity. If an E-language property such as, for example, word order were part of UG, then there could be no variation in word order. However, within the P&P approach we can accommodate observable differences in word order and explain them by different parameter settings during growth of the I-language, one of the most famous examples being the head-order parameter that determines the positions of head and complement in a syntactic structure, thereby causing English and Japanese syntax to be mirror-inverted (Moro, 2008; Lasnik and Lohndal, 2010). Hence, the content of UG need not be overtly expressed in E-language(s), instead, UG principles factor in I-language(s) and allow for parametric variation.

From an evolutionary point of view, UG principles need not even be domain-specific in Tomasello's sense; in fact, UG principles quite likely have resulted from cognitive precursors that serve or have served a purpose other than language, so that they are actually being "reused" as part of FL. Though maybe unconvincing at first, this is by no means a self-contradicting notion: Quite generally, asking for the main function or purpose of a bodily organ in an evolutionary context seems inappropriate as "purpose" here in the majority, if not all cases is manifold and indeterminable. What, for example, would be the "purpose" of the human spine: Supporting the body or protecting the spinal cord?

In the neurosciences, this idea of "neural recycling," that is the reuse of neural circuitry that is already in place for a purpose other than their "original function" during phylogeny and ontogeny, is well-established (e.g., Dehaene and Cohen, 2007). As of today, input-systems assigning "some order to our experience" (Anderson and Lightfoot, 2000, p. 15), such as the visual system famously studied by Hubel and Wiesel (1962), are best understood by neuroscientists; but everything currently known about the way in which neurons encode and process information (Quian Quiroga et al., 2005; Sterling and Laughlin, 2015), as well as how even comparatively simple nervous systems implement cognitive functions (Gallistel, 2009), points to abstraction and abstract principles. On these grounds, it is not hard to see how abstract UG principles could have arisen by evolutionary "accident."

UG principles are then specific to language insofar only as they are part of FL, which leads to the question of how the human ability for language came into being in the first place. Whether there are UG principles that evolved for some other means and are now specific only to language is presently still unclear, but this question is of course, in the long run, subject to empirical research. But this research might not necessarily be classically linguistic in nature: “One should not overlook the possibility that the most appropriate terminology for describing the contents of UG may turn out to be terminology of neuroscience” (Bolender, 2010a, p. 2662).

3 An evolutionary take

The self-set focus of classic generativism and (large parts of) today’s biolinguistics has been on investigating the properties of UG, that becomes even more apparent when looking at work that takes an evolutionary perspective on the language faculty, such as Hauser, Chomsky, and Fitch (2002) and Fitch, Hauser, and Chomsky (2005), as well as Fitch (2010): In the main, a distinction between the Faculty of Language in a broad sense (FLB) and the Faculty of Language in a narrow sense (FLN) is made. Whereas FLB is (quite commonsensically) understood as including any mechanism involved in language in the mind/brain, FLN is what is specific to human language. Yet, the term FLN is not just “[. . .] a matter of definition, the specific mechanisms that belong in the FLN category is a topic for empirical research” (Fitch, 2010, p. 22).

Human language in the form of FLB is therefore just another example for an organ “[...] concocted from whatever biological structures are already available [...]” (Bolender, 2010b, p. 6). Jacob (1977) has famously dubbed this the “tinkering” *modus operandi* of evolution. Thus, even if it turned out that there are no language-specific cognitive principles but only originally non-linguistic principles that are reused (i.e. joined together) in FL around a generative mechanism, this would not refute the technical concept of UG. Instead, it would simply mean that there is only one language-specific mutation in UG, which is part of FLN, so that FLN is therefore minimal in the spirit of the Minimalist Program (MP; Chomsky, 1995). But UG figures in both FLN (language-specific properties) and FLB (“originally” non-linguistic cognitive properties reused in FL). Samuels, Hauser, and Boeckx (in press) endorse a similar view of UG with respect to phonology, the domain of language they judge most accessible to comparative study.

3.1 Minimalism, Darwin, & Wallace

Following the main idea of the MP, it is now in fact frequently assumed (e.g., Hauser, Chomsky, and Fitch, 2002) that FLN might be limited to the computational system that is central to human language, that is Merge as the process that allows for recursion. When discussing the MP, Ludlow makes the general remark that due to the ideas of the MP, FL—FLN to be precise—is conceived as being “not the product of messy evolutionary tinkering” (2011, p. 36), contrary to what has been said about factors that most likely determine the evolutionary origin of FLB above. Clearly, this claim requires further deliberation.

The underlying idea is that a recursive process (i.e. Merge in MP terminology), termed simply a “generative procedure” by Chomsky (2013), which supposedly is the sole component of FLN, enables a seemingly perfect matching of phonetic form (PF) and logical form (LF) at the interface level of the articulatory-perceptual system (A-P) and the interface level of the conceptual-intentional system (C-I), as first discussed by Chomsky (1995). This minimal or as streamlined as possible scope of the mechanisms that are part of FLN is, on the one hand, the product of theoretical linguistic research following the MP and, on the other hand, necessary in order to account for what part, or parts, of FL is uniquely human (Hauser, Chomsky, and Fitch, 2002; Fitch, Hauser, and Chomsky, 2005).

Otherwise put, a minimal account of FLN is the proposed answer to what Boeckx has termed “Darwin’s problem” (2009), and what Levinson has referred to as “Wallace’s problem” in a recent book review (2014). Darwin and Wallace were both puzzled by the question of how natural selection could possibly account for the seemingly excelling intellectual, linguistic, and technological abilities of humankind.—A small change with far-reaching consequences in the form of a minimal FLN could be the answer. An answer that, quite remarkably, implies that FLN might be non-adapted.

Of course, as Fitch is eager to point out, “it could transpire that no sub-component of the language faculty is truly unique” (2010, p. 22), meaning that one cannot distinguish FLN from FLB as FLN does not exist. Whilst this indeed is a possibility, Fitch considers it highly unlikely, (at least in this regard) on a par with Pinker and Bloom (1990), and, more recently, Pinker and Jackendoff (2005) who have criticised Hauser, Chomsky, and Fitch (2002) essentially by arguing for a UG that facilitates an account of FL with an extensive FLN (i.e. many language-specific evolutionary adaptations).

3.2 Findings in aligned disciplines

As Luef and Peltzer-Karpf (2013) delineate in their review of the current state of research in evolutionary anthropology, the communicative abilities (which include aspects of FLB) in humans and chimpanzees as well as other great apes by now have been found to be homologous traits in many respects (e.g., cerebral organization). Evidence for this supposition (especially in regard to Brodmann area (BA) 44—part of Broca’s area) is provided by various neuroimaging studies (e.g., Cantalupo and Hopkins, 2001; Petrides, Cadoret, and Mackey, 2005; Cantalupo et al., 2009; Kelly et al., 2010). Recently, Bornkessel-Schlesewsky et al. (2015a) proposed that the difference between human brains and those of non-human primates might even be only quantitative in nature, an argument that subsequently triggered a discussion between Skeide and Friederici (2015) who argued for a direct dorsal pathway unique to humans and Bornkessel-Schlesewsky et al. (2015b) who cited imaging evidence to the contrary.

Yet, language as *the* human speciality is neither necessarily and primarily a means of communication (frequently “language design” actually seems to hinder communication; see Chomsky, 2011b), nor should we expect it to be straightforwardly localisable in the brain (in accordance with other higher cognitive abilities; see Uttal, 2001). What is of interest for evolutionary studies is the particular cognitive phenotype that enables FL, that is a particular computational cognitive system (Bolhuis, Tattersall, Chomsky, and Berwick, et al., 2014). Thus, regardless of whether Skeide and Friederici (2015) or Bornkessel-Schlesewsky et al. (2015a, 2015b) are right about the connectivity profile of BA 44, this would be significant only if the computational implications of their claims were well-established; yet, the “atoms” of neural computation largely remain to be identified (Marcus, Marblestone, and Dean, 2014). Also, the development of linking hypotheses between linguistic computations and neural ones is a project that is still pending (Poeppel and Embick, 2005/2013). Homologies in brains of humans and non-human primates should therefore not be interpreted as evidence countering the claim that FL(N) enables a computational cognitive phenotype in humans that is absent in other (closely related) species.

Similarly, the fact that a multitude of cognitive functions has been localised to BA 44 does not imply that this area is not a crucial part of the syntactic network, uniquely involved in processing language (Fedorenko, Behr, and Kanwisher, 2011) and especially hierarchical structure (Musso et al., 2003), independent of other cognitive functions such as working memory (Makuuchi et al., 2009). Still, at the end, what actually happens in BA 44 is unclear (due to the limitations of contemporary neuroimaging techniques), and it might even turn out that BA 44 is just a “hub” routing information in the network (Moro, 2008). Recent work has emphasised the subcortical parts of language networks, as well as the role of the corpus callosum and globularity (Boeckx and

Benítez Burraco, 2014; Theofanopoulou, 2015). In any case, we should expect to find an extensive FLB and a minimal FLN, meaning that identifying how the particularly human cognitive phenotype manifests in the brain will not be a straightforward task (after all, it is not even clear whether the macro-level of neural connectivity and activity patterns is necessarily the “right” level to look for what is “uniquely human”).

Research on the genetic basis of language (reviewed in Fisher, Lai, and Monaco, 2003; Vargha-Khadem et al., 2005; Graham and Fisher, 2013) paints a similar picture of conservation that shows in sequence homology and possibly orthology (Krause et al., 2007). For example, it is at this time widely accepted that the by now infamous FOXP2 gene “[...] is arguably a necessary condition for language, [but] it almost certainly is not a sufficient one, by any stretch of the imagination” (Benítez-Burraco et al., 2008, p. 226). Generally, progress in identifying the genetic basis of FL(N) has been sparse because the direct linking of genotype and phenotype is a drastic simplification (Moro, 2008). Hence, Boeckx and Leivada (2014) have noted that the genocentric conception of UG might even turn out to hamper progress, and that UG will have to be accompanied by an “epiUG.”

4 Concluding remarks

Future research, presumably in close collaboration with neuroscience, will hopefully aid (bio-)linguists in determining the nature and properties of FLN/FLB and UG, as well as their role in evolution and development. In all likelihood, “Darwin’s and Wallace’s problem” cannot be solved in isolation from “Broca’s problem” (Boeckx, 2009), the question of how FL is implemented neurally. Similarly, the classic (generativist) focus on UG will have to make way for far more encompassing developmental accounts (Lorenzo and Longa, 2009) that attempt bridging the wide gap between genotype and phenotype.

Nevertheless, conceptually UG, or the “language-ready brain,” will remain unchallenged even when the terminology employed and theoretical accounts put forward continue to change. The reason for this is straightforward: If it actually transpired that there is no FLN this would practically undermine the very basis of modern (bio-)linguistics and result in an enormous explanatory gap just like the one that as of now can be found in usage-based approaches to language acquisition (Crain and Thornton, 2012; Chomsky, 2013) that have been briefly mentioned above.

So what to make of this? As Chomsky with much apropos remarks, as a cognitive scientist working on language one has to accept the axiom that human language (i.e. FL and UG) actually “[...] exists as an independent object of serious study and not as just some arbitrary collection of various phenomena and processes more or less like, say, today’s weather” (2011a). In other words, even though theories of UG will continue to change and will provide just one part of an encompassing “theory of language,” we must assume that something *is* special about human language (and thus genome and brain), at the very least the way in which it conciliated a multitude of cognitive functions.

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