There is no place for markedness in biologically-informed phonology*

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1 Introduction

In the study of language from a biological perspective, doing away with traditional notions in linguistics has become a necessary procedure. This is a not a goal, but rather a realization: when one starts gathering information from different disciplines, it becomes apparent that many linguistic primitives and constraints just won't do, and consequently must be abandoned for the purposes of the study of language as a biological *capacity*. This is not a completely foreign process in the sciences, and it applies to any approach to language or to anything: theories do away with previous notions very frequently, and that is a way in which a theory might depart from a previous one and become, in effect, a new and desirably better one. The degree to which this practice manifests itself—which in the case of a biological theory of language might seem extreme to linguists in general—is usually a function of how much new information is brought into the fold, and the importance conferred to it. By information I mean insights that *inform* a theory; not necessarily extra information encoded in the theory itself.

Indeed, the more one tries to learn about and incorporate insights from other fields that pertain to an object of study, the more likely one is to reframe long-held assumptions, which tend to last for a long time when kept away from outside

influence. Any serious biolinguist[1] will thus find this to be a very natural process. The immediate effect of this approach is that what is considered a good theory of language actually becomes less predictive of particular linguistic phenomena, and therefore less interesting for those who strive for all-encompassing formalization of linguistic structure. This is very likely a reason for some resistance to biolinguistics, perceived as a move that leads to weaker or not very linguistic-looking theories. However, for those who strive for an understanding of the biological and evolutionary nature of the human capacity for language or phonology in particular, and not interested in *data-fitting* (on this notion, see Hale & Reiss, 2008; Blaho & Rice, 2014) this is not a weakness and is in fact desirable.[2] In reality, and to put things more practically, that "weak" character is only apparent or worth discussing when looking at a biological theory of language with behavioral outcomes in mind, in the same sense that from a social studies perspective a physics theory is weak in predicting social interactions, which fall outside of its scope even if down the line they rely on physics.

2 Markedness

Markedness is one of those notions that seem unwarranted in a biological theory of language in general and phonology in particular, the topic of this volume. In fact, even in linguistic circles not particularly known for discussing the biological properties of language, markedness has triggered suspicion regarding its status as a valid or useful concept.

Over 10 years ago, Martin Haspelmath (2006) wrote a paper titled "Against Markedness, and What to Replace it With". Haspelmath offers quite an exhaustive list of senses of markedness found in the literature, which he groups into four

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[1] By "serious biolinguist", what is meant here and throughout the paper is anyone who explicitly makes the biology of language their research focus. Similarly, a biolinguistic approach here means an approach to language explicitly rooted in biology and open to integration between different fields of the life sciences. The term is also—perhaps most often, unfortunately—used as an updated label for generative linguist, which can cause confusion, as it often does. For a discussion on different senses of "biolinguistics" (and, by extension, "biolinguist"), and the motivations behind them, see Martins & Boeckx (2016b).

[2] On the topic of "weak" theories, it is important to make a distinction between theories that are adjusted so as to rule out unattested patterns (for example, imposing a fixed ranking of at least some constraints in Optimality Theory (McCarthy, 2002)), and those that are weak in the sense that they do not try to predict patterns that fall outside of what is considered to be their ontological scope (for example, Substance Free Phonology (Hale & Reiss, 2000, 2008)). A good biological theory of language is "weak" only in the latter sense, since it does not try to encode as biological properties of language ways of ruling out particular linguistic patterns that do not occur (nor ways of guaranteeing frequent—or even seemingly universal—patterns, for that matter).

main kinds: markedness as (i)complexity, (ii) difficulty, (iii) abnormality, (iv) multidimensional correlation. Haspelmath's rationale, which finds some relatively recent antecedents in Gurevich (2001), Hume (2004) and Blevins (2004), is that all of the senses in which markedness is alluded to can be explained by less ambiguous terms. In other words, instead of an abstract notion of markedness, one can always find an independent, often simpler explanation for all phenomena that linguists have tried to explain by alluding to a notion of markedness. This passage from (Samuels, 2011, 18) summarizes the issue nicely:

> [Haspelmath] makes the important point that, no matter which sense of the term one uses, markedness always demands rather than provides explanation and it is explainable by other principles in all cases. For example, markedness as phonetic difficulty demands explanation in terms of the human articulatory and/or perceptual systems; markedness as structural complexity demands explanation in terms of the added demands that complexity makes on linguistic computation (such as taxing a limited memory capacity). So when we speak of markedness, we are really using shorthand for a number of deeper factors that are in large part extralinguistic.

Haspelmath's paper has been cited many times, and as far as I was able to determine there have been no rebuttals of it: there seem to be no convincing arguments that we should continue alluding to markedness now that we understand that it is a confusing label for a number of things, all of which have their own explanations.[3] The fact of the matter is that, despite this, resort to markedness does not seem to have slowed down, and it's still very much a central concept in most theories of language and of phonology in particular. A case in point would be Optimality Theory (Prince & Smolensky, 1993 [2004]), whose constraint set (CON) is based on notions of markedness and wellformedness, and the constraints therein are taken to be universal.

A notable exception that tries to counter this tendency is Substance Free Phonology (SFP) Hale & Reiss (2000, 2008), which attempts to describe phonological *competence*, or that which is the pure phonological component, regardless of *performance*. In other words, the aim of SFP is to describe the computational mechanism(s) of phonology alone, while avoiding allusion to or incorporation of the accidental manifestations of what it generates into its model.

Reiss (to appear) introduces SFP by asking the reader to "Imagine a theory of phonology that makes no reference to well-formedness, repair, contrast, typology, variation, language change, markedness, 'child phonology', faithfulness, constraints, phonotactics, articulatory or acoustic phonetics, or speech perception." I

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[3]This doesn't mean that we know exactly how all of those other things operate exactly, but it definitely doesn't mean that markedness-based explanations should be pursued instead.

think this is indeed something worth imagining and achieving. Even if one does not particularly care for the nature of phonological computation, wondering whether or not to keep these notions in a theory forces one to (re)think of their nature. All of these notions—notice how many of these actually fit inside the "markedness" label themselves—are based on substance: extra-phonological content and factors that influence or happen to be the physical shape of the patterns we attest in the world's languages. If they are extra-phonological, and if they can be explained in other terms, very often in completely independent terms (like the morphology of anatomical structures involved in articulation and perception, for example), encoding them in a theory of phonology is in effect needlessly duplicating knowledge and unwarrantedly loading the phonological component of language with complex machinery. Reiss (to appear) provides the very telling example of Boyle's law—which completely independently of language explains the relationship between the pressure of a gas and the volume of its container (thus providing some principled explanation for the way our breathing system works)—which has been appealed to in an Optimality-theoretical constraint (McCarthy & Prince, 1995). Encoding a gas law into an innate phonological constraint shows either an as yet unjustified ontological commitment to clearly extra-linguistic phenomena, or an unexplained indifference to the content of linguistic constraints, as long as they work. This is done for the sole purpose of accurately accounting for the patterns we attest when this task should be secondary to the problem of understanding what *phonology* is.[4] The take home message of SFP, if a conflation of all work under this label is allowed (for a overview of some of the different ways in which the SFP has been used, see Blaho, 2008), is that i) whatever phonology is, it must be possible to describe it in terms that are independent of its observable results, and ii) phonological representations have no reason to look like what they represent.

Again, extra-phonological factors definitely play a role, but there is no reason to expect the computation machinery of phonology to specifically account for the results of this complex interaction. It is then not surprising that theories of phonology which abuse substance, namely by incorporating markedness in some form in the arsenal of fundamental constraints that make up the phonological component of human language, are probably destined to not be relevant in discovering important properties of phonology as part of a biological system, even if formal coverage of attested patterns reaches unprecedented heights.

This conclusion that SFP has reached, which in a way is proof that what Haspelmath (2006) and others have pointed out makes sense, has been arrived at

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[4]I still believe that for many generative and in particular OT phonologists the task of covering all that is attested—no more, no less—is, deep down, secondary; what they want to achieve is a theory of the phonological component. But they still seem to be seduced by the idea of doing this by building not only the possibility space but also the possibilities themselves onto the phonological component.

within the field of linguistics. A an approach that is consonant with this idea is Evolutionary Phonology (Blevins, 2004), which defends and focuses on the non-phonological nature of diachronic sound change.

This is a very important point: some phonologists, most likely influenced by ideas from other fields, recognize that the way in which phonology has been traditionally conceived of is not the right one (and not by just a little), and this rings particularly true if we assume that our object of study is biological. The concept of markedness in particular, though probably very advantageous in an utilitarian sense to many linguists, doesn't seem to do much for the understanding of the phonological component when we start wondering about its place in biology and evolution, even if we don't pursue those curiosities much further. But if we do, the concept of markedness becomes even less likely to be relevant. What I intend to do in what follows is offer an additional reason to reject markedness by offering some insights from biology.

3   Biology

The notion (or role) of markedness that is worth exploring here for the purposes of this exposition is one that Haspelmath (2006, 42) describes in the passage below. All of the other ways in which markedness has been used by linguists don't really trigger a response from biolinguists, as they all seem to have been shown to be irrelevant or redundant even in for biologically-agnostic linguistics.

> *Markedness is available in the cognitive code (= Universal Grammar).* In Chomsky & Halle (I968), the idea was proposed that markedness values are not just present in language-particular mental grammars, but are in some way defined at the level of the innate cognitive code for language ('Universal Grammar'). This position lives on in Optimality Theory in the widespread claim that markedness constraints (as well as the other constraints) are innate and part of UG.[. . .]

I understand this sense of markedness to be the one that appeals to contemporary generative phonologists the most. It is a claim about a real property of our "language component".

If one starts to seriously think about phonology from a biological perspective, that is, if one tries to come up with *bio*logical (and not just logical) arguments and experiments with the aim of understanding what that part of language might look like biologically, markedness becomes indeed a strange concept.

A possible way to entertain this exercise is to think about the role of markedness in terms of its uniqueness. If markedness is something which is really part of the language component, one could consider one of two ideas: it is either unique to

language (and therefore humans), or part of a larger set of components not unique to language and maybe not even to humans. Putting things in these terms might ring some bells, namely the FLN (Faculty of Language in the narrow sense)/FLB (Faculty of Language in the broad sense) distinction put forward by Hauser et al. (2002).

3.1   If markedness were unique

Ever since the distinction was introduced, linguists have focused on FLN (that which is unique to language and humans) at the expense of FLB (that which is shared with other domains other than langauge and other species). This was most likely fostered by the fact Hauser et al. (2002) proposed that FLN would contain "recursion and its mapping to the sensorimotor and conceptual interfaces". These concepts, regardless of how well- or ill-defined they might be, are familiar to linguists; they are of the kind that linguists appeal to. Had the content of FLN been left completely up for grabs, perhaps FLB would have gotten more attention from linguists. But the problem here is not that markedness was not included in FLN. Even if Hauser et al. (2002) had given no hints as to what FLN contains, there is a much bigger issue: the very distinction is not fruitful in biological terms. By proposing a set that is supposed to contain biological properties that are *sui generis*, unique to language, and which are not shared or related to anything else in human species or in others, one is effectively hermetically sealing the problem of the evolution of language, in that research forward becomes in practice impossible. It becomes a mystery, as some—in part including the authors of the FLN/FLB distinction—have claimed (Hauser et al., 2014).[5]

This is not a problem of the FLN/FLB distinction *per se*, but rather of the reasoning behind it. Anything which is put in FLN is suddenly at odds with current understanding and practice in biology. Anything that is *unique* is evolutionarily *novel*: [...] neither homologous to any structure in the ancestral species nor homonomous to any other structure of the same organism"(Muller & Wagner, 1991, 243). But the consensus in biology is that virtually nothing is truly novel (Moczek, 2008), and an extremely specific cognitive trait like markedness would seem to have an even harder time qualifying for the prize. Moreover, whatever is in a set of unique and novel traits is not amenable to investigation in biology. The most consensual view is that "phenotypic novelty is largely reorganizational rather than a product of innovative genes" (West-Eberhard, 2003). The biological makeup of language is built upon different parts, reorganized and to new uses.

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[5]Those who use this mystery-inducing state of affairs as a way of highlighting the uniqueness of language (language cannot be studied biologically because there's nothing else like it) will be stuck in this hindering predicament; others will explore ways forward (for a discussion, see Martins & Boeckx, 2016a).

Its study requires a comparative method, the only method perhaps that is really in line with a Darwinian view of biology, and the one which also cognition in particular seems to be the most fruitful. As De Waal & Ferrari (2010, 201) put it:

> Over the last few decades, comparative cognitive research has focused on the pinnacles of mental evolution, asking all-or-nothing questions such as which animals (if any) possess a theory of mind, culture, linguistic abilities, future planning, and so on. Research programs adopting this top-down perspective have often pitted one taxon against another, resulting in sharp dividing lines. Insight into the underlying mechanisms has lagged behind. A dramatic change in focus now seems to be under way, however, with increased appreciation that the basic building blocks of cognition might be shared across a wide range of species. We argue that this bottom-up perspective, which focuses on the constituent capacities underlying larger cognitive phenomena, is more in line with both neuroscience and evolutionary biology.

So if, for the sake of argument, we say markedness is *unique* to language, there doesn't seem to be either plausibility for such a claim or ways of investigating it. To uphold markedness as a meaningful biological property unique to language, one would have to explain how it got there, and contra current knowledge in biology and genetics explain the one event that put this extremely specific cognitive property in place. This is no fault of markedness. Any particular putative linguistic entity posited to be biologically unique is bound to be met with the same limitations from the start. In fact, linguistic properties at large, even those that we can define in a much more satisfactory manner than markedness, stand in the way of creating linking hypotheses between linguistic behavior and biology and neuroscience. Their ontology and granularity are simply not compatible with the kinds of primitives the other fields can work with (Poeppel & Embick, 2005). One simply cannot find direct links from specific genes to putative linguistic properties, or find a specific brain structure that is responsible for them (Poeppel, 2012). The fact that many linguistic properties posited and used in theories of language "just work" in linguistics is not a sign of their strength, but instead a sign of the circumscribed nature of linguistics as a science, which regardless of its own success, when confronted with other sciences finds more obstacles than commonalities.

3.2   If markedness were not unique

If we say the opposite, for the sake of argument, that markedness is not a unique but instead a shared trait, and follow the Darwinian lessons in West-Eberhard (2003) and De Waal & Ferrari (2010) (and much evolutionary biology), we have to look for markedness elsewhere. In other words, we have to decompose the notion

of markedness into other, more generic notions, which we can both try to trace back in evolution and study in other domains and species. What we will arrive at is a list of properties that will start to look a lot like the independent explanations for these other senses of markedness. This effectively renders the innate, UG-encoded, ontologically essential notion of markedness vacuous; we have to concede markedness is something else, the extra-linguistic factors that the myriad senses of the term redundantly denote.

After realizing this, we will find ourselves delving into methodologies and questions that bear little resemblance to traditional linguistic theorizing, and we should welcome it. Samuels (2011, ch.3) already highlighted this *modus operandi.* So we may find ourselves looking at the genetics and neuroanatomy of vocal-learning birds and find parallels between their computational abilities and those that underlie human phonology (for a good, recent review, see Samuels, 2015), and how an eminently environmental factor such as domestication actually influences their song learning (Okanoya, 2012), and then how a similar process in human self-domestication might have had a comparable effect in the shaping of human language (Benítez-Burraco et al., 2016). We may also turn to our primate ancestry and find that monkeys have the vocal tract for speech (Fitch et al., 2016), but before assuming they're a lost cause because they don't have the brain, we might find that some species get around the limitations of their vocal tract by vollitionally controlling their calls through other means, like the orangutans who place leaves or their hands in front of their mouth when vocalizing, to exaggerate size and trick predators (Lameira et al., 2012). Or we might find it curious that when lip-smacking, the lips, tongue and hyoid of monkeys move at a frequency that is consistent with the rhythm of human speech, as is the configuration of different parts of the vocal tract (Ghazanfar et al., 2012), and wonder about what is behind it at the brain level. These are just a few examples of what one would deal with when looking for ways of understanding different factors that contributed to how both the shape and computation of the phonological side of language happen to be the way they are.

But even just conceptually, the notion of markedness is odd in a biological context. Whatever may be perceived as deviating or staying true to "what's normal" in biology is a result of various factors; they are not to be explained by any one notion whose sole purpose is to set them as such. There is no ontologically special constraint whose violation will determine if an organism will have x or y limbs, or if mammals will have a stronger or a weaker sense of smell. No biologist in their right mind will attempt to explain seemingly ubiquitous or absent characteristics of organisms by appealing to a notion of (un)markedness and call it a day. They will know the interaction between genes, development, and environment is so complex that it would be unreasonable to put all of these factors aside and instead use

in their theory a notion that just *is*. If we take the well-known, widely repeated mantra of Chomskyan linguistics that we should look at the language faculty as any other characteristic of organisms—popular examples are the digestive and visual systems—, or that if it is true that we should study the mental the same way we study the physical, the reasons for appealing to an abstract and ontologically real notion of markedness seem to diminish to the point of oblivion.

4   A note on other notions, like Merge

An anonymous reviewer brought to my consideration the status of other well-established notions in linguistics such as Merge, which does not make much biological or neurological sense, as pointed out by Chomsky in his book of interviews with James McGilvray (Chomsky, 2012, 91), and yet few syntacticians have come to think that the problem is with syntax and not biology, and have found little motivation to reframe the notion. This legitimacy of this state of affairs is supported by an analogy drawn in the beginning of the same book (Chomsky, 2012, 19) between current linguistics and chemistry before the 20th century. Atoms made no sense in classical physics, and it was physics that had to radically change to accommodate them, leaving chemistry virtually unchanged. The analogy goes that perhaps biology will need to change significantly if we want its unification with linguistics to take place (Chomsky, 2012, 150).

This analogy would make sense *all things being equal*, but in actuality there's no reason to believe—just based on the history of physics and chemistry—that biology needs significant change at the moment specifically to accommodate the study of notions like Merge. What's at stake in this case is very different from what was at stake in the unification of physics and chemistry. To be fair, evolutionary biology is actually undergoing a major paradigm shift (from a modern to an extended synthesis) and provides a vast space of possibilities for learning more about the connection between end products (such as language) and their biological underpinnings. This connection is a complex and multi-dimensional one (Boeckx, 2017). As far as biology is concerned, now we are actually at a much better time to study the evolution of language than ever before. Perhaps now is the time to act on what Chomsky also says in the book when continuing to talk about Merge (Chomsky, 2012, 91):

> we don't have sets in our heads. So you have to know that when we develop a theory about our thinking, about our computation, internal processes and so on in terms of sets, that's to going to have to be translated into some terms that are neurologically realizable. [...] There are a lot of promissory notes there when you talk about a generative

> grammar as being based on an operation of Merge that forms sets, and so on and so forth. That's something metaphorical, and the metaphor has to be spelled out someday.

I think the last part of the passage above is very worth focusing on. The fact that few linguists would question the ontology or atomicity of a notion like Merge does not mean its ontological status is well defined and supported. It's true that the biological investigation of language is a small niche in linguistics; one would not expect a great number of syntacticians to explore the biological underpinnings of the notions they posit very frequently. But the translation work is necessary; the metaphors need to be spelled out.

As I mentioned before, on the side of linguistics, the most authoritative position at the moment seems to be that the evolution of language—or, specifically, Merge—is a mystery (Hauser et al., 2014). But there has been nothing in this position that offers even hints at better unification; it only hinders any possibilities for exploring the topic. Evolutionary biology—which the brand of biolinguistics I argue as deserving of the name (see footnote 1, and reference therein) takes to be fundamental—has much more to offer than this. While linguistics hasn't done much to accommodate any modern insights from biology—biological arguments put forth by linguists have remained pretty much the same over the course of 60 years—evolutionary biology keeps expanding the wealth of fields and information it needs to draw from in order to arrive at a better picture of how evolution works (Pigliucci & Müller, 2010). The resulting picture is messier, but also richer.

Having said this, a distinction between markedness and Merge in terms of their status in language studies needs to be made. What they have in common is that they both need to be reassessed if we are to better understand the nature and evolution of language, and biology can—must—help us in that direction. What distinguishes the two notions in this context is the stage of reassessment they are in. Markedness has already been shown even within the field of linguistics to be a problematic notion, for it seems to be nothing more than a label for other things, all with their own independent explanations. In other words, markedness seems to be the result of a conceptual mistake; it doesn't really exist *per se*. It can safely be put aside. Merge, on the other hand, seems to exist. It's the recursive ability or operation that allows us to "construct a digitally infinite array of hierarchically structured expressions with determinate interpretations at the interfaces with other organic systems" (Berwick & Chomsky, 2016, 110). The problem with Merge is that most linguists take it to be atomic, and its nature is defined in such a way that it is not amenable to evolutionary study (Hauser et al., 2014). There are no reasons to think from a biological point of view to that Merge is irreducible, even if from a (linguistic) theoretical point of view it is very elegantly so. There are ways to argue for it, as Berwick & Chomsky (2016, and several other publications)

have tried to do, but all of the important details are left vague or inscrutable (how exactly it could suddenly have arisen as is, what the interfaces really are, what is the influence of these other, necessary organic systems is in its evolution and operation, etc.).

To sum up the point brought up by mentioning Merge, the study of language evolution—which concomitantly leads us to more knowledge about its biological nature—would profit from a change in perspective from linguists in regards to irreducible notions that do not fit a modern biological picture. Perhaps there is more under the hood that will give us answers, which biology will help us find.

5   Conclusion

A conclusion that could be derived is that any theory of language that makes use of a notion of markedness is not adequate at the biological level. Optimality Theory, for example, is eminently inadequate if it is to have biological plausibility aspirations. I find its form elegant—some factors are stronger than others, but not always—and OT captures that spirit. But it does so at the expense of objective and stable criteria and it fails to recognize the multiple dimensions in which that interaction happens.

Whatever explanation is sought by using markedness can only lead to more things to be explained, and thus its use is, at the very least, counter-productive. If the goal is to describe linguistic phenomena in a way that fits the data, the only thing being brought to the table with sophisticated theories or markedness is a sophisticated notational system.

Trying to fit markedness into a biological picture—to really imagine what it would be like if someone actually set out to study markedness biologically—might seem like a straw-man to some. But it shouldn't be taken that way: if a notion in a theory of language stops making sense when put into a biological context, the problem is very likely not one of biology. It's more reasonable to think that simply there is no place for markedness in biologically-informed phonology.

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