

# Language evolution: Insisting on making it a mystery or turning it into a problem?\*

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## Abstract

Hauser et al. (2014) offer a rather negative view of the state of affairs in language evolution. The authors target some of the fields that have spawned the most activity and hypotheses in recent years (comparative animal behavior studies, archaeology, molecular biology and modelling), and then show what they have done wrong. We argue instead that it is the progress in these fields that accounts in large part for the revival of biolinguistic concerns.

In a recent, widely-read paper, Hauser et al. (2014) offer a rather negative view of the state of affairs in language evolution. More specifically, the authors believe that little to no progress has been made in the various relevant fields regarding the age-old questions of the origin and evolution of the human capacity for language. We beg to differ.

The authors' strategy is to target some of the fields that have spawned the most activity and hypotheses in recent years (comparative animal behavior studies, archaeology, molecular biology and modelling), and then show what they have done wrong. These fields, they say, have not advanced much more than speculation. Instead, we think that it is the progress in these fields that accounts in large part for the revival of biolinguistic concerns (Boeckx, 2013).

The intention of Hauser et al.'s paper is to point out the damage that has been done over the last decades, by calling attention to the dangers of jumping from simplistic, impoverished data and observations to full-fledged accounts. To some extent, we agree. But we find it curious that linguistics is not of the targets of the paper, even though the field is rife with speculative and untestable proposals and implications for how language evolved. The implicit but in our view obvious corollary is that—for the authors—linguistic theorizing plays at present a crucial role in advancing what we know about language evolution, or at the very least does not have much to be criticized (while other fields do have a lot to be criticized for, since they do not match what has been or could be

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accomplished by linguistic theorizing). We take this absence with a grain of salt, as we find it hard to explain how a paper on the status of language evolution studies does not even dabble in the shortcomings of what is in effect the field of expertise of half of its 8 co-authors.

While it is true that we do not know how language evolved—if we did, no one would be working on it anymore—, to diminish the work in that has been done recently on various disciplines to the point of irrelevancy is not only dubious (we feel it ends up throwing the baby with the bathwater) but, in the case of this group of authors, confusing (a close look at the literature will reveal that different combinations of the authors of Hauser et al. (2014) make arguments of the sort they take issue with, and rely on sources of information that in the paper are deemed unreliable). In what follows we will briefly touch on the different fields targeted by Hauser et al. (2014), and point out both incongruence and unjustified pessimism in their arguments. We will not offer here in-depth rebuttals or qualifications of the authors' positions, but instead provide a little glimpse into what we see as more heat than light.

In relation to the archaeological record, which in the abstract the authors say “does not inform our understanding of the computations and representations of our earliest ancestors, leaving details of origin and selective pressure unresolved”, Chomsky (2005, 3), on the basis of work by Tattersall, writes of the faculty of language as part of a “a complex of capacities that seem to have crystallized fairly recently, perhaps a little over 50,000 years ago, among a small breeding group of which we are all descendants—a complex that sets humans apart rather sharply from other animals, including other hominids, judging by traces they have left in the archaeological record.” In the very same page, Chomsky goes on to say that the great leap forward is “the result of some genetic event that rewired the brain, allowing for the origin of modern language with the rich syntax that provides a multitude of modes of expression of thought, a prerequisite for social development and the sharp changes of behavior that are revealed in the archaeological record [...]” The same ideas are echoed, for example, in Chomsky (2010). Yang (2010) claims that we cannot ask too much of Universal Grammar, because “[a] theory of Universal Grammar is a statement of human biology, and one needs to be mindful of the limited structural modification that would have been plausible under the extremely brief history of *Homo sapiens* evolution.” But how do we know this if, according to the same authors, language evolution has been a complete mystery for years? Speculation goes both ways, and one should not dismiss one and support the other. It seems that for Hauser et al. (2014) arguments of this sort were appropriate while the relevant archaeological record was thought to have been left by humans, and only now that this is not so clear (e.g. Zilhão, 2011), the authors claim we shouldn't try to derive inferences from archaeology. Nonetheless, one needs not look hard to find resort to archaeological evidence in support of a non-gradualist position as recently as in 2016 (e.g. Berwick and Chomsky, 2016, 37-38), leaving us all the more confused as to what their overall position regarding its reliability as a source of information really is.

Hauser et al. (2014) also take issue with comparative animal work, which

“provide[s] virtually no relevant parallels to human linguistic communication, and none of the underlying biological capacity.” The problem with this assessment is that it equates the testing of all-or-nothing hypotheses (animal  $X$  displays some form of language phenotype property  $P$ ) with everything that such endeavors might have to offer. We take it that not many people believe in “talking birds” and “signing apes” (if this was ever the case for serious scientists), but those studies and their scrutiny are important to determine what humans and non-human animals do, and the wealth of animal studies is becoming increasingly more important in the study of underlying mechanisms shared by different species and formulation of hypothesis concerning humans in particular. Berwick and Chomsky (2013) seem like they would agree, and Berwick et al. (2011), for example, draw a connection between birdsong syntax and underlying mechanisms of human speech, and state that “comparing the structure of human speech and birdsong can be a useful tool for the study of evolution of brain and behavior” (p. 120). This qualifies as Hauser et al’s (2002) FLN, which in the present paper the authors stress as referring not only to the mechanisms for discrete infinity but also to the “mappings to the interfaces with the conceptual-intentional and sensory-motor systems.” Hauser et al. (2014) are right to point out that some current techniques used in animal studies fail to capture the animals’ actual capacities, which they are more likely to display in their natural habitats, roaming free and devoid of extensive, goal-oriented training. But by doing so they are targeting the search for a human-like linguistic phenotype in other species, rather than the bottom-up comparative work of the kind advocated, for example, by de Waal and Ferrari (2010), to which we will return later.

As for molecular biology, Hauser et al. (2014) do not present a critique *per se*, but rather an overview of current work which shows that there is no clear path from genes to linguistic behavior. This is not surprising to molecular biologists, and in fact simplistic proposals of the sort they criticize—coming up with just-so stories out of thin air, or on the basis of impoverished observations—usually come from the field of linguistics.<sup>1</sup> It is for this reason that work in linguistics must provide information that can be used to creating linking hypotheses, which currently and for the most part it cannot. This difficulty in creating linking hypothesis between genes and linguistic behavior is amplified by this inadequacy of linguistics in providing primitives that other fields can work with.<sup>2</sup> A logical theory of the language faculty does not necessarily amount to a biologically plausible one, which is what we should be aiming for. This state of affairs alone would warrant a discussion of linguistics as a source of information in language evolution studies that is absent from Hauser et al. (2014). The way in which the authors present the linguistic phenotype—a novel recursion mechanism, unique to humans—is enough to stall or severely hinder the kind of linking hypotheses we all would like to see, and which Hauser et al. (2014) say we have no hopes of seeing any time soon. The reason for that is that we actually know that

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<sup>1</sup>See Boeckx (2016) for discussion of a recent example.

<sup>2</sup>For a discussion of this problem, see Poeppel and Embick (2005).

novelty doesn't simply "arise". While traits may on the surface seem novel, or *sui generis*,<sup>3</sup> their nature is "largely reorganizational, rather than the product of innovative genes" (West-Eberhard, 2005, p. 6547), that is, phenotypic novelties are the result of the combination of different, more generic mechanisms.

Hauser et al.'s case against current work in modelling is the most consistent with each author's practice, but their general disdain for the role of culture in evolution—"In this paper, we are interested in biological *as opposed* to cultural evolution" (p. 2, our emphasis)—overlooks important advancements in evolutionary biology which show that culture *and* environment might really be crucial. "Culture" is a taboo notion in most generative circles, perhaps because it is usually seen as detrimental to biology in a theory of language. We find this to happen only under a naïve view of biology, along with an axiomatic incompatibility with linguistic approaches that give pride of place to culture. Crucially, one should not ignore the role of environmental factors in the shaping of the genotype, and in turn the shaping of the phenotype. There is no reason to seek explanation of phenotypic variation only in environmental or genetic factors. Instead, one should incorporate the lessons from Evo-Devo, and pay attention to work on the genotype-environment interaction (West-Eberhard, 2003), which shows that the degree to which environmental choices affect the way the genetic blueprint is expressed depends on the specific genotype-environment interaction in each case.

In a somewhat more optimistic tone, Hauser et al. (2014) offer some suggestions of "paths forward", both interspersed throughout the paper and as a final comment. These suggestions, however, are very much confined, and suffer from the same problems that their negative assessment of the various fields does. In a nutshell, the authors insist on gauging the usefulness of theoretical and experimental work by whether or not it "speaks" to Merge, the recursive mechanism they place at the center of the linguistic phenotype. It is not surprising that the presupposition that Merge must be at the center of inquiry into language evolution drastically reduces what can be done in practice, but in doing so it pushes the *mystery* the authors speak of. That is not what parsimony is for. Language evolution thus becomes a *mystery* only to adherents of this presupposition, and a *problem*—like many others in the sciences—for those willing to explore further.

In the case of animal studies, the authors put their money on the development of new techniques that could allow the collection of neural data from free-ranging animals, thus revealing their capacities in the absence of reinforcement. We agree that such techniques would work wonders for the field, but what propels Hauser et al. (2014) is that we would then be able to devise a "set of stimuli that are generated from a recursive operation such as Merge (a recursive operation that combines two objects, such as two lexical items, to construct a new object, such as a phrase, in a process that can be iterated indefinitely), expose animals to a subset of these, and then test them on a wide range of alternatives that extend beyond the initial set in ways that can reveal substantial

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<sup>3</sup>For discussion, see Wagner and Müller (2002) and Moczek (2008), among others.

generalization, and thus comprehension of the underlying generative operation.” (p. 9–10) Presumably, these tests would reveal that animals either fail miserably or are able to generalize by relying on different, finite mechanisms, thus showing the uniqueness of Merge and supporting the discontinuity hypothesis. But there are myriad (other) ways in which animal studies can work in favor of a deeper knowledge about the biology of language. In this context, we find it appropriate to quote a passage by (de Waal and Ferrari 2010: 201):

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.

Indeed, looking for a full-fledged ability such as language or something that looks close enough to it is bound not to tell us much, but that’s not what we should be looking for. Instead, we should decompose it into more generic mechanisms, not unique to either the language domain nor the human species. This path will inevitably leads us to the study of abilities with little resemblance to language, and mechanisms at levels far deeper than the behavioral and the cognitive. But it’s these levels we need to get to in order to arrive at true linking hypotheses.

As for modeling, the authors say that “it must focus on the computations and representations of the core competence for language, recognize the distinction between these internal processes and their potential externalization in communication, and lay out models that can be empirically tested in our own and other species.” Again, it must speak to Merge (which is how we must interpret “the core competence for language” when reading Hauser et al. (2014)), and a host of other possible modeling work is not even considered. We don’t see how this would change the status of the field if all we are allowed to focus on is the core recursive mechanisms the authors equate with the linguistic phenotype (and perhaps the interfaces between and externalization systems, which are usually left vague in any case). Opening one’s mind to the role of the environment (or culture, which we find hard to tease apart from “environment” in a meaningful way) is likely to prove fruitful, and modeling work pays particular attention to the influence this aspect of the world might have. We agree with (Kirby, 2013, 473) that “the particular learning mechanisms that we bring to bear on the task of acquiring language are assuredly part of our biology. The key questions to ask about this aspect of the biology of language are: what is the nature of our

biological endowment that relates to language learning? and to what extent is this endowment specific to language? These questions essentially define the biolinguistics enterprise, and their answer depends on an understanding of the relationships between learning, cultural transmission, and biological evolution.”

In sum, Hauser et al. (2014) paint an ugly picture of language evolution that seems to have been caused by other, incautious scientists, while in reality the authors themselves have incurred in the same kind of arguments and assumptions—the kind they deem poor and speculative. This practice has not stopped with this paper: a quick read through the latest book by two of the authors (Berwick and Chomsky, 2016) will reveal discussion of topics that in Hauser et al. (2014) we are advised not to pay much attention to. This kind of incongruous back-and-forth is bound to cause more confusion than resolution. Furthermore, insisting on the idea that the evolution of language is mysterious—and not a problem we can look into right now, with its own difficulties and promising avenues—will deter only those who are stuck with a naïve view of biology that allows for such a simplistic perspective.

What is clear to us, and not so clear from reading Hauser et al. (2014), is that in order to make language evolution more of a problem and less of a mystery, everyone—linguists included—will have to make the mapping between mind to brain the focus of study. It is this intermediate level between genotype and phenotype that must be the target of intensive investigation. If the mind is what the brain does, it is imperative to understand how the brain came to do what it does. This will necessarily involve a reconsideration of the nature and fabric of the language faculty, for only those descriptions of linguistic knowledge that can be associated with concrete neural correlates will have a fighting chance of going beyond the limitations of the fossil record, and exploit findings in paleoneurology, paleogenetics, and comparative cognitive biology.

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