

Review of R. Larson, V. Deprez, and H. Yamakido (2010), *The Evolution of Human Language: Biolinguistic Perspectives*

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This collection of papers grew from a 2005 conference exploring themes from Hauser, Chomsky, and Fitch (2002, henceforth HCF). Part I (almost half the book) is almost exclusively concerned with that paper: it contains an introduction which explains the significance of HCF; a reproduction of the original paper; and further papers by the three individual authors, with one other paper, from Jackendoff. Jackendoff aside, these papers clarify the original intent of HCF (Chomsky's contribution develops the argument of HCF, while Hauser and Fitch each aim to remove some of the unclarity which dogged that paper). The remainder of the book introduces the reader to a range of alternative theories of the evolution of language. But HCF dominates the volume: it is hard not to see the other papers as a series of explicit or implicit endorsements of, complements to, and disagreements with that paper.

Little is completely new in this volume. Many contributions summarize research which is developed in greater detail elsewhere. But there is still real interest in the juxtaposition of such divergent papers. The synthesis is largely left to the reader, but an active reader will feel that the whole is greater than the sum of its parts. The book is a snapshot of the state of the art in certain research traditions (although by no means all, or even a representative sample) addressing topics in the evolution of language.

Among the papers not by Hauser, Chomsky, or Fitch, three classes can be discerned. Some papers are broadly compatible with HCF, or focus on areas that were treated somewhat dismissively by HCF. This class contains Piatelli-Palmarini, who recapitulates certain standard Chomskyan themes, but also Stromswold (who presents a wealth of new data from a large-scale twin study on heritability and genetic correlation of many broad linguistic and nonlinguistic measures, and arrives at a satisfyingly pluralistic picture of the genetic underpinnings of language, which doesn't exactly support HCF, but is at least largely compatible with it), and Sperber & Origgi (who neatly demonstrate the absolute centrality of an ostensive-inferential model of communication to a theory of the evolutionary growth of a communicative system, a model which relies on a capacity for 'naïve psychology', or Theory of Mind), among others.

Taking these papers (together with HCF) seriously, one arrives at a pluralistic conception of the evolution of language, in which no single major innovation led to the emergence of language. The remarkable and apparently species-specific properties of language include the following: human language users recursively generate hierarchical structures, including a syntactic structure built on highly abstract primitive categories; the "narrow" language faculty, in HCF's terms, interfaces with a symbolic, intensional, compositional semantic system, and an anatomically modified phonetic system; linguistic structures are built from a lexicon, an exceptionally large array of stored associative structures, some aspects of which are probably language-specific; successful linguistic communication requires an advanced theory of mind; and so on. Different papers focus on different areas, and there is a regrettable tendency for researchers (e.g. Lieberman on vocal anatomy and the neural basis of motor control, Bingham on collective ostracism of free riders, and Dor & Jablonka on cultural evolution preceding biological evolution) to assume, in the face of massive evidence to the contrary, that "their" speciality is *the* important innovation, but by reading between the lines, we can derive a compelling argument that anything recognizable as modern human language is the product of several distinct innovations, over millions of years.

Moreover, the adaptive value of all of these innovations is clear, and it is also clear that many of them are not just adaptive for their effect on language, or communication. This raises a central question of which innovations (if any) were adaptations for language, or for communication.

The papers which diverge from HCF fall into two classes, depending on their position concerning the core hypothesis of HCF, that the only component of the narrow faculty of language (FLN) is a capacity to build a recursive structure and map it to ‘conceptual-intentional’ and ‘sensory-motor’ (or semantic and phonetic) interfaces. It will be helpful here to fix an interpretation of the notoriously opaque discussion of this hypothesis.

Three statements in HCF come close to jointly implying a contradiction, but stop just short.¹ They are the following:

‘We assume... that a key component of FLN is a computational system (narrow syntax) that generates internal representations and maps them into the sensory-motor interface by the phonological system, and into the conceptual-intentional interface by the (formal) semantic system’ (p.18).

‘the core recursive aspect of FLN currently appears to lack any analog in animal communication and possibly other domains as well.’ (p.19)

‘One possibility... is that recursion in animals represents a modular system designed for a particular function (e.g., navigation) and impenetrable with respect to other systems. During evolution, the modular and highly domain-specific system of recursion may have become penetrable and domain-general.’ (p.37)

Now, maybe p.19 does contradict p.37. But a more interesting interpretation runs as follows: FLN, by hypothesis, consists of some sort of recursive structure-building, plus mapping to the interfaces. Recursive structure-building is a general human cognitive capacity. But recursive structure-building *as it appears in FLN* is still unique, because of its interaction with other aspects (unspecified in HCF) of FLN, plausibly the lexicon, and the mapping to nonhierarchical, linear phonetic forms.

This is how I interpret the core hypothesis of HCF (others have arrived at other interpretations, but they all seem to me to be more seriously contradicted by statements in the paper). The limited recursive structure in nonhuman cognition becomes a pervasive, domain-general hallmark of hominid cognition. The unique part of language consists in how we use those recursive structures.

Many papers in this collection implicitly or explicitly disagree with (this interpretation of) HCF in one of two ways. Either they assume that recursively generated structures as used in language evolved for communication (Bickerton and Jackendoff). Or, more radically, they assume that the syntactic structures which many still take to be at the heart of FLN (whatever they may have evolved for) are not particularly impressive from a comparative perspective (e.g. Bingham) or realized too diversely across languages to constitute a genetically determined species property (e.g. Dor & Jablonka).

This defines three classes of paper in this collection. The first is compatible with the broad outlines of the evolutionary story proposed by HCF, though it may disagree with the details. The second agrees with HCF in that it assumes some distinct genetic basis for some linguistic abilities, but disagrees concerning the adaptive nature of that genetically determined capacity. The third denies that there is any specific genetic basis to core linguistic capacities. This division corresponds to the major choice point in this reliably controversial area of evolutionary linguistic theory: core aspects of linguistic structure are either adaptive for communication, a spandrel arising from some other recent biological adaptation, or something else (massively non-species-specific, or culturally determined).

¹ This disregards the clearly inconsistent claim in the abstract that ‘FLN includes only recursion’ (p.14), acknowledged by Hauser and Fitch to be an error.

Papers in the last class make many linguists' blood boil (though certainly not all: see Evans and Levinson 2009 for a recent defence of a position very similar to that of Dor & Jablonka). This is because languages behave in certain ways, and not in other ways which are, *a priori*, just as likely. The Konstanz Universals Archive (<http://typo.uni-konstanz.de/archive/intro/index.php>) lists over 100 absolute, achronic, unconditional *linguistic universals*, to which no exceptions have been found, along with many more implicational universals, which all, if valid, represent constraints on the phonology, morphosyntax, and semantics (the "core grammar") of possible human languages.

An important linguistic research agenda involves the discovery and explanation of such universals, with some success, and one reason to adopt a nontrivial theory of linguistic form is as a way to approach such universals. Such theories are not the product of a perverse desire to keep linguistic theory unnecessarily complex and opaque. They exist because, at present levels of understanding, the facts demand them. But, of course, once we have such a nontrivial theory, we have to explain where it comes from and what it's doing here. This remains the central challenge in evolutionary linguistic research.

Researchers such as Dor & Jablonka, Lieberman, or Bingham, who downplay the importance or uniqueness of the structure underpinning language, owe an explanation of the basic empirical fact of the existence of such universals. One recurring strategy is to attempt to explain them away by appeal to domain-general constraints (the 'third factor' of Chomsky 2005). Such explanations may eventually prove valid, but are as yet too underdeveloped to inspire much hope. To be sure, every linguist would like to see the universals explained away, but as Piatelli-Palmarini puts it (see also Pinker and Bloom 1990), it seems that for some researchers, '*any* future, yet unspecified *possible* explanation [is] preferable to an actual, detailed, and satisfactory explanation, offered there and then, based on the specificity and autonomy of syntax' (p.154).

None of this affects the substance of Bingham's, or Dor & Jablonka's, theories. But we must be very wary of overexpansive claims made on behalf of those theories. They each describe *part of* the evolution of language, but these are not the complete theories of the evolution of language that they claim to be. A complete theory of language must account for the structural properties of language, and such theories do not.

The other two groups of papers are divided as to whether linguistic structure was a communicative adaptation, as the common-sense position would claim, or a spandrel. This question brings us back to HCF, and particularly to the fuller exposition in Chomsky's chapter.

A crucial part of the logic of HCF's evolutionary argument is that only the tightly circumscribed narrow faculty of language, rather than the more expansive broad faculty of language, is uniquely human. The less there is in FLN, the less compelling is the argument that properties of language are adaptive for communication, and the greater the plausibility that human language arose as a spandrel. As they say:

'If FLN is indeed this restricted, this hypothesis has the interesting effect of nullifying the argument from design, and thus rendering the status of FLN as an adaptation open to question... The question is not whether FLN in toto is adaptive. By allowing us to communicate an endless variety of thoughts, recursion is clearly an adaptive computation. The question is whether particular components of the functioning of FLN are adaptations for language, specifically acted upon by natural selection – or, even more broadly, whether FLN evolved for reasons other than communication.' (pp.23, 25).

This, from a Chomskyan perspective, is the point of HCF. When evolutionary concerns first enter Chomsky's writings, in the mid-1970s, the core theory of the architecture of language was large, complex, and *sui generis*. As the introduction and the papers by Chomsky and Piatelli-Palmarini point out, stories concerning the evolution of such an apparently unique and unwieldy system seemed doomed to failure: the structures involved were far too large and precarious for any

saltationist approach to be tenable, but the component parts did not show the kind of interrelationships which we would expect on any gradualist story.

Since then, a pan-theoretic trend in syntax has been towards radical simplification. This is true not just in Chomsky's own theories, but across a range of otherwise unrelated syntactic theories, from cognitive grammar to categorial grammar. Minimalism, in this sense, is not the sole preserve of Chomsky's Minimalist Program – it is a defining characteristic of responsible syntactic research. As syntactic theory shrinks, the prospects for accounts of the evolution of syntax grow brighter, but the question of whether linguistic structure grew gradually more complex (as Bickerton or Jackendoff predict) or emerged very rapidly as pre-existing complex structures from other cognitive domains are adapted wholesale (as Chomsky claims) remains wide open.

In this connection, one interesting point in Chomsky's paper concerns the nature of the vocabularies that Merge, his recursive structure-building operation, operates on. At times, here and elsewhere, Chomsky mentions the possibility of Merge, plus a single vocabulary item, yielding 'a form of arithmetic' (p.53 – although which form of arithmetic is unclear), and also mentioned the possibility that a lexical item could be 'some visual array' (p.53). Although this strictly goes beyond what Chomsky says, it is possible here to glimpse a conception of Merge as a domain-general trick for recursive structure building, with particular faculties being defined partly by the specific uses they make of this trick. This chimes with the interpretation of HCF defended above, where what is special about language is not so much recursion, as the particular use made of recursion.

In the picture that Chomsky sketches, the evolution of language depends heavily on the evolution of a recursive operation for building conceptual--intentional representations. This leaves the question of the pronunciation, or "externalization" (p.60), of these representations. This is where his theory predicts variation to reside: 'Why are there so many languages? The reason might be that the problem of externalization can be solved in many different and independent ways, either before or after the dispersal of the original population' (p.61).

In other words, although the use of FLN to generate conceptual representations is a species property, the use of any given strategy to externalize those representations is not. Multiple solutions may have arisen, in different populations, at different times, and externalization strategies are as culturally determined for Chomsky as they do for Dor & Jablonka ('Solving the externalization problem may not have involved an evolutionary change – that is, genomic change. It might simply be a problem addressed by existing cognitive processes, in different ways, and at different times', p.61).

Chomsky's contribution therefore contains a radical restatement of a recurring theme throughout his oeuvre: the thesis that the ways in which we happen to pronounce syntactic structures are incidental to the conceptual-intentional core of syntactic theory. The problem, of course, is that many linguistic universals are stated precisely in terms of such properties of the externalization process. In isolation, then, Chomsky's position, by focusing on the properties of conceptual-intentional representations, shares the problems of Dor & Jablonka's or Bingham's when it comes to the proper treatment of universal structural properties which are irreducible to properties of Merge or the conceptual--intentional interface.²

This striking similarity across theoretical positions which are apparently poles apart highlights certain conclusions which emerge from the collection as a whole, sometimes because of

² This is not a critique of the Merge-centric minimalist program in general. Full theories within that program invariably make use of further nontrivial devices at the interfaces with semantics and morphophonology, together with specification of the formal characteristics of lexical items and constraints on the syntactic configurations in which relations among lexical items and/or constituents can be established. The point is that these other devices are crucial in minimalist accounts of the special, universal structural properties of language, and there has, as yet, been no concerted effort to show that these apparently universal factors are either present in other domains or emerge from cultural evolution or "third factor" considerations.

the individual papers, sometimes despite them. Firstly, as discussed above, there is every reason to expect a theory of language to be pluralistic. Human syntax is special, but so are many other factors contributing to human language use. If this reading of Chomsky is correct, then perversely, Merge does not have a special status among these innovations: like many other factors, it is a simple trick with extraordinarily wide repercussions, stretching far beyond what researchers typically recognize as “language”.

The second conclusion from this collection is that language continues to look as mysterious as ever. Despite the real merit of every paper in this collection (and every paper is genuinely worth reading), none of the theories presented here really explains the evolution of a cognitive capacity with all the species-wide strangeness of human language.

Despite the real progress that this book reflects, then, we are only just beginning. This is an invigorating time to be involved in biolinguistic research. There is real momentum, but the unanswered questions remain enormous. Partly, as this book indirectly shows, this is because there are still basic disagreements about the proper formal characterization of language, and its relation to other cognitive domains: we still do not know what evolved. Perhaps, almost a decade later, this is the most useful aspect of HCF: the reminder of the importance of comparative research for making progress on questions related to the evolution of language. As this volume also shows, the interesting comparisons will not just be among species, but also among cognitive domains within a species. Once we have more of an idea how the language faculty differs from other cognitive capacities, perhaps we will be able to make some progress on the core disagreements (the degree to which language is purely biologically determined, the adaptive nature of the language faculty), now decades old, which continue to divide the papers here.

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