

In Defence of FLB/FLN: A Reply to Wacewicz et al. (in press)

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1. Introduction

"*Language* in language evolution research" (Wacewicz et al., in press) is a valuable synthesis of recent research on the origin and evolution of language. Its central message, however, is surprising: instead of encouraging the clearest possible specification of the object of study in research on language evolution, that is, a specification of what evolves when we say that language evolved, the authors argue that the lack of agreement on what language is has served to facilitate the significant progress made in research on language evolution over the last decade.

The absence of a more or less hegemonic theory of language (within and outside linguistics) may have made easier the proliferation of hypotheses, data sources, methodologies and opportunities for interdisciplinary collaboration in language evolution research. But, unless one wants to say that a certain definition of language can be an obstacle to the investigation of its evolution, it is difficult to justify the claim that things would have been worse, or progress less marked, had there been more agreement on the nature of the object of study. It is more logical to suppose that things might have gone even better, although it is impossible to know.

The argument Wacewicz et al. (in press) use to show that a precise definition of *language* in the expression *language evolution* is unnecessary is based on the fact that the best known attempt to establish a terminology intended to favour the collaboration between disciplines and schools of linguistic thought in the field of the study of language evolution has been a failure, and that such a failure has not prevented these studies from flourishing. The authors refer, of course, to the terminological proposal of Hauser et al. (2002; henceforth HCF) to distinguish the

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faculty of language in the broad sense (FLB) and the faculty of language in the narrow sense (FLN). The reason why they suggest that the HCF proposal is a failure is that Chomsky, Fitch and Hauser would have defined the notion of FLN in a contradictory and inconsistent way in HCF and in Fitch et al. (2005; henceforth FHC). As argued in Wacewicz et al. (in press), what is relevant is that this incoherence has gone unnoticed in the field (except for Wacewicz et al. themselves), which would prove the irrelevance of precise definitions of the object of study in the field of language evolution research.

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. In the absence of such assumptions, one cannot speak of biology or evolutionary theory in any grounded or meaningful way. There is no reason why (evolutionary) linguistics should be different here.

However, Wacewicz et al.'s central message seems to be that the (alleged) failure of the FLB/FLN terminology would have "freed" the study of language evolution from a restrictive conception of language, thus facilitating the proliferation of new points of view and new opportunities for interdisciplinary collaboration.

As I hope to show, it is not true that the definition of FLN is contradictory or inconsistent, nor is it true that terminological proposals, such as that of HCF, are irrelevant or unnecessary for the evaluation of real progress in research on language evolution.

2. On the Definition(s) of FLN

Wacewicz et al. imply that the HCF proposal intends to define language as FLN, that is, that HCF seeks to determine the FLN as the "authentic" object of study of language evolution research, rather than the whole FLB. But this is not so at all. In reality, HCF is not about language 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. More specifically, HCF is a proposal on "unpacking FLB into its myriad component mechanisms" (Fitch et al. 2005: 181) with the aim of better understanding how these different components have been able to evolve, under the reasonable assumption that "profitable research into the biology and evolution of language requires fractionation of 'language' into component mechanisms and interfaces" (Fitch et al. 2005: 179). Whereas we can appreciate that the choice of the adjectives *broad* and *narrow* respond to a certain conception of language, in no way can it be suggested that in both articles *language* is defined as FLN, nor that it is implied that the study of the evolution of language should be limited to the study of FLN. In fact, the object of study from the biolinguistic point of view adopted by HCF cannot be other than FLB (that is, FL), and one of the aspects of this study is to determine what components it has, and whether FLN (a subset of FLB) really exists as such or not. Of course, it is true that

Chomsky and others have continued to use the word *language* to refer to this specific part of the FLB, instead of using the expression FLN, although Chomsky has also used *computational system* (Chomsky 1995) and *basic property* (Chomsky 2016). Moreover, it is debatable whether something that does not include the sensorimotor component should still be called *language* (albeit in the narrow sense). In any case, we might note that using the word *language* to designate any of the components of language is not a sin exclusive to generative grammar.

Besides, it is not true, as stated in Wacewicz et al. (in press), that HCF and FHC offer two different definitions of FLN, and that they are incompatible:

The proponents of FLN have defined it twice, in fundamentally discrepant ways: the definition originally formulated in Hauser et al. (2002) and the one later formulated in Fitch et al. (2005) describe two distinct entities (not simply different versions of the same entity).

(Wacewicz et al., in press: 6)

In HCF, Hauser, Chomsky and Fitch 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*. 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" (Hauser et al. 2002: 1573), assuming, therefore, that only those elements meet the requirements to be included in that category.

What other definition of FLN appears in FHC that implies a different entity as a referent for FLN, or is in contradiction with the definition of HCF? None.

HCF reserves the term FLN for the computational component because its authors believe (as a hypothesis) that it is the only component of FLB that is specifically human and specifically linguistic. In FHC there is no change of reference or definition: the authors continue to maintain the same content of FLN. In fact, they discuss in detail why they think that the FL components that Pinker & Jackendoff (2005) (an article to which FHC is a response) also consider as specifically linguistic and human (certain aspects of human speech) are not part of FLN, but of FLB.

The supposed contradiction pointed out in Wacewicz et al. (in press) can be explained if we consider that the definition of FLN in HCF is *extensional* while the definition in FHC is *intensional*, but there is no inconsistency or contradiction. Actually, FHC alludes to this fact: "The term 'FLN' thus served dual duties in HCF" (Fitch et al. 2005: 182). Note that the intensional use of the term FLN is not new in FHC. In HCF the authors use it when commenting on Liberman's approach:

For example, Liberman and his associates [...] have argued that the sensory-motor systems were specifically adapted for language, *and*

hence should be considered part of FLN.

(Hauser et al. 2002: 1569, emphasis added)

In both texts there is the same terminological proposal ('let us call FLN that which is specific to human language') and the same empirical hypothesis ('only the computational system is specific to human language, that is, FLN'). Why does HCF consider that the FLN is made up of the computational component? Because Hauser, Chomsky and Fitch stipulate that only the computational component is specific to human language, but not because there is some other inherent connection between the adjective *narrow* and the computational component of FL. If the definition criterion of FLN in HCF were not (human and linguistic) specificity, it would make no sense that other components of FL as different as the conceptual-intentional (CI) and the sensorimotor (SM) systems were grouped into what is not FLN. What groups CI and SM together against FLN is that they are (by hypothesis) neither specifically human nor specifically linguistic. Hence, they are part of FLB, but not of the FLN subset. So, the reasoning seems to be as follows: we think that the computational system is specific to human language; we want to call that which is specific to human language FLN; hence, the FLN is the computational system.

Wacewicz et al. (in press) illustrate their argument by citing the following texts as an example of contradiction:

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. (Fitch et al. 2005: 181)

Second, although we have argued that most if not all of FLB is shared with other species, whereas FLN may be unique to humans, this represents a tentative, testable hypothesis in need of further empirical investigation. (Hauser et al. 2002: 1576)

But in both texts Chomsky, Fitch and Hauser indicate that the content of FLN must be empirically determined (and that it is possible that it is an empty set). The statement in the text from 2002 that the FLN (extensionally identified as the computational component) is specific to human language is a hypothesis in need of further investigation; the naming of both sets of components as broad or narrow (which is what the end of the text from 2005 refers to) is obviously not a falsifiable hypothesis, but a terminological convention. There is no contradiction.

Wacewicz et al. point out that "[i]nterestingly, Hauser, Chomsky and Fitch themselves have never addressed the inconsistency and may even remain unaware of it" (in press: 8), which is not surprising if such inconsistency only exists in their own interpretation. Wacewicz et al. (in press: 10) affirm that the

important thing is not the possible inconsistency, but the fact that it went unnoticed, which would supposedly be an argument in favour of the irrelevance of "top-down" definitions of the object of study. The argument (insofar as it has any force) is not actually evaluable, because the absence of the perception of incongruity is easily explained considering that there is no incongruity.

Moreover, the influence that the terminological proposal has had in the field could be estimated, for example, through considering the number of citations of HCF, which, according to Wacewicz et al., "is doubtlessly among the most influential works in the field, and probably its most widely cited article" (in press: 5).

3. Applying the HCF Terminology: Speech, Communication, and the Language-Ready Brain

The remainder of the article is a comprehensive and informative synthesis of the last ten years of research in language evolution, mostly that which does not use the HCF terminology. But since FLB/FLN is a terminological proposition, and not a theory of language, it is easy to see that it would have been very useful (or, at any rate, perfectly possible) to use it to classify and explain the four main lines of research (or models) that the authors consider, which are the following ones (including the main authors of each):

- (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 and Benítez-Burraco)

Model (i) 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.

Model (ii) identifies language with languages and, in this sense, the model is not particularly interested in FL as a biological object, nor, therefore, in the evolution of its components.

Model (iii) identifies language with communication: The studies inspired by Tomasello and Levinson are studies of the evolution of communication, not so much of FLB itself. Such work would thus constitute a part of the investigation of the evolution of the relation between the CI and SM components of FLB, while they simply ignore the computational dimension of language (FLN). Actually, in model (iii) language is a "cultural artefact" inserted into basic human communication, which is what would have evolved.

Model (iv) invokes the notion of a language-ready brain. This notion can be interpreted in two ways: Either the brain first developed, through evolution, those properties that make it capable of producing human languages (Chomsky's point of view), 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 (as in Deacon's 1997 approach). In the first case, the

relevant parts of brain architecture and physiology (FLB) determine the distinctive properties of human language (and of human languages); in the second case, languages somehow externally developed this complexity and motivated the adaptations that would lead to the language-ready brain. In my opinion, the second version is highly implausible, although it is increasingly popular (as Wacewicz et al.'s report shows).

Actually, Arbib's version of the language-ready brain notion also equates language with languages, since his notion of a language-ready brain presupposes the existence of languages in the environment, as a brain-independent phenomenon:

What evolved (Evo) was a language-ready brain—not a brain with an innate mechanism encoding a universal grammar [...] but rather one enabling a child to acquire language (Devo), but only if raised in a milieu in which language is already present, something which, it is claimed, required tens of millennia of cultural evolution after the emergence of Homo sapiens (Socio).

(Arbib 2018, *apud* Wacewicz et al., in press: 25)

Note that Arbib's qualification of what evolved (a language-ready brain and not "a brain with an innate mechanism encoding a universal grammar") is unnecessary, since, according to the HCF model of FLB (and according to the explicit model of Berwick and Chomsky 2016), there is no difference between, on the one hand, a brain programmed to develop a FLB and, on the other, a language-ready brain. A brain that includes that which makes only humans capable of learning and using the languages that they use (as Arbib points out, "only the human brain is language ready", Arbib 2012: ix, *apud* Wacewicz et al., in press: 25) is already a brain that necessarily develops allowing the acquisition and use of human languages, that is, a brain with a FLB (including FLN). To put it more clearly: there is no difference (terminology aside) between a language-ready brain and a brain programmed to develop a FLB.

Thus, if the assumption that human languages precede the language-ready brain is not included, then the language-ready model is identical (except in detail) to Chomsky's. This is the case with the version of the language-ready brain proposed by Benítez-Burraco and Boeckx. In fact, the explanation of the internal structure and evolution of the FLB is very similar in both cases: According to Wacewicz et al. (in press), in the language-ready brain approach to language evolution set out by Benítez-Burraco and Boeckx, language (its "core combinatorial operation" [i.e. FLN]) is the result of genetic mutations that alter the anatomy and physiology of the brain, while "[o]ther components of language, particularly, some forms of phonology and pragmatics, are assumed to predate this human specific innovation" [i.e. SM and CI, the other older components of FLB according to HCF]. In this model of the language-ready brain:

[T]his freely combining merging ability is argued to be constrained via its interfacing with other cognitive systems and with the devices

involved in speech/gesture production. (Wacewicz et al., in press: 27)

A characterization not unlike Chomsky's classic:

We take L [a language] to be a generative procedure that constructs pairs [...] that are interpreted at the articulatory-perceptual (A-P) and conceptual-intentional (C-I) interfaces. (1995: 219)

This language-ready brain model and Chomsky's model are also similar in terms of the idea that language (apparently its "core combinatorial operation") did not evolve for communication, but as a system of thought:

One interesting and distinctive consequence is a conception of language primarily as a tool for thinking (rather than for communicating), since our language-readiness would have initially emerged as a new, improved mechanism of conceptualising. (Wacewicz et al., in press: 29)

This characterization is clearly reminiscent of the Chomskyan conception of FL and its evolution reflected in HCF, and developed in Chomsky (2007): "the earliest stage of language would have been just that: a language of thought, used internally" (Chomsky 2007: 13; see also Berwick & Chomsky 2011; 2016). As Chomsky has summarized more recently:

Investigation of the design of language gives good reason to take seriously a traditional conception of language as essentially an instrument of thought. Externalization then would be an ancillary process, its properties a reflex of the largely or completely independent sensorimotor system. (Chomsky 2016: 73)

Apart from the fact that Benítez-Burraco & Boeckx's approach to the evolution of the FLN (as presented in Wacewicz et al., in press) is much more specific in biological and genetic aspects than that of Chomsky, it is evident that both models are variants of what is essentially the same theory of language.

Thus, the review of these four major research models of the evolution of language shows the usefulness of the HFC strategy of "fractionating FLB into several separate components, each of which might have different evolutionary histories" (Fitch et al. 2005: 205).

If it is not conceivable to give a coherent definition of language (and I agree), it is even less conceivable to speak of language evolution without further specification. The use of the expression *language evolution* without qualifying what *language* refers to implies, for example, that there is no clear distinction between the evolution of the organism that uses languages, and the historical change in the languages that this organism uses (as happens in some of the traditions mentioned above).

4. Conclusion

Wacewicz et al. (in press) argue that the absence of a restrictive definition of the object of study would be one of the causes of the progress that has been made in language evolution research, and they deny that the HCF initiative has had positive effects in that direction. This conclusion is based on the claim of an unnoticed inconsistency in the definition of FLN, but such an inconsistency does not exist.

Of course, it is possible that the absence of a hegemonic "top-down" definition of language has opened the door to more pluralistic and interdisciplinary research activity. But it does not seem (in light of the synthesis presented in Wacewicz et al, in press) that we have sufficient perspective today to affirm that remarkable progress has been made over the last 10 years in the understanding of how the human faculty of language evolved. I tend to agree with Lewontin in that "[w]e know essentially nothing about the evolution of our cognitive capabilities, and there is a strong possibility that we will never know much about it" (Lewontin 1998: 109), although I do not fully share his attitude towards the achievements in the field of language evolution research:

Reconstructions of the evolutionary history and the causal mechanisms of the acquisition of linguistic competence or numerical ability are nothing more than a mixture of pure speculation and inventive stories. (Lewontin 1998: 111)

I have no doubt, in view of the valuable synthesis presented in Wacewicz et al. (in press), that the field of language evolution research is in good health and is bringing together important intellectual and economic efforts. But I do have doubts that the lack of a more uniform and specific definition of what evolved when language evolved is a positive aspect here. I see no reason to affirm that the abandoning of an explicit proposal of the division of labour and the avoidance of misunderstandings, as in the FLB/FLN terminological proposal, is an advantage; rather, it might well be the opposite.

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