

## Incidental Biology: A Reply to Derek Bickerton's Response

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When we first undertook the task of reviewing Derek Bickerton's (2009) *Adam's Tongue* (AT), we had to make a decision about what points we should focus our attention on. Bickerton, in his reply, finds it amazing that we almost exclusively focus on "incidental uses of biology and NCT" (p. 128). Indeed, we eventually decided against articulating our criticisms along the communicative vs. non-communicative debate for the reasons we already expressed in the review (henceforth SM), but also on the (implicit) assumption that any account of language origins should be based on a sound biological framework, that biolinguistics is about taking biology seriously, and that, should any attempt at an explanation of the origins of language be based on a wrong or inadequate use of biological notions, it would immediately be flawed, independently of how the communicative vs. non-communicative debate is eventually resolved. It appears that, at least as far as this point is concerned, Bickerton agrees with us: "[I]f I had really gone wrong here, the book's major contentions would be seriously flawed" (p. 128) And we think they are. That's the reason why we concentrated on them. But also because we do not think there are any biologically incidental issues in biology; nor in biolinguistics, for that matter.

AT is not "a textbook of evolutionary biology nor a primer in NCT" (p. 128)—true, but this is no excuse for making a sloppy use of these disciplines. In fact, with our selection of quotes we tried to show that, in spite of the fact that all the appropriate leitmotifs of NCT appear in the book, what Bickerton is actually advocating for in AT is a necessity scenario not too different from the one suggested by Marx and Engels more than 150 years ago. The main features of this necessity scenario are: (i) ecological niches preexist the organisms that enter them; (ii) the environment is sufficiently structured to create the need for a new character; (iii) if the organism in the niche is not constrained by its biology (more about this below) it will respond by developing the (behavioral) character that satisfies the need imposed by the environment; (iv) a feedback process ensues,

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This work has been carried out through the project *Biolingüística: Fundamento genético, desarrollo y evolución del lenguaje* (HUM2007-60427/FILO) of the Spanish Ministerio de Ciencia e Innovación and partially cofunded by FEDER funds (Balari & Lorenzo), and received support from the Generalitat de Catalunya through grant 2009SGR1079-Lingüística Teòrica to the Centre de Lingüística Teòrica of the Universitat Autònoma de Barcelona (Balari).



not too different from reinforcement in behaviorist psychology, that eventually fixes the character; and (v) in this feedback process genes and environment interact and this interaction is mediated by behavior.

From this follows that *any* organism could develop language should it enter the appropriate niche *and* should it have the appropriate biology. This last phrase, which is ours, not Bickerton's, essentially means that the organism must not be variation-limited (Számádó & Szathmáry 2006), i.e. that its biology must not include constraints that would make this change impossible. This probably is, by the way, what, according to Bickerton, is supposed to explain why ants and bees don't have language, although, without knowing exactly what these biological constraints are, we cannot rule out the possibility, given that they seem to occupy the appropriate niche, that they will eventually develop it.

Now, this is not NCT—we reaffirm our contention. It is, as we pointed out in SM, at its best, a variety of adaptationism; see our quote by Lewontin in SM, but also the following:

Whereas classical evolutionary theory sees the organism as the key that has to fit into the environment's lock, both ecological developmental biology and niche construction see interactions between them. Niche construction emphasizes the ability of the organism to alter its environment; eco-devo emphasizes the ability of the environment to alter the developing organism.  
(Laland *et al.* 2008: 550).

At its worst, with the introduction of 'necessity' in whole picture, it is a modern version of Marxian or Rousseauian environmentalism:

[L]'usage de la parole s'établit ou se perfectionne insensiblement dans le sein de chaque famille, et l'on peut conjecturer encore comment diverses causes particulières purent étendre le langage, et en accélérer le progrès en le rendant plus nécessaire. Des grandes inondations ou des tremblements de terre environnèrent d'eaux ou de précipices des cantons habités; de révolutions du globe detachèrent et coupèrent en îles des portions du continent. On conçoit qu'entre des hommes ainsi rapprochés, et forcés de vivre ensemble, il dût se former un idiome commun plutôt qu'entre ceux qui erroient librement dans les forêts de la terre ferme. (Rousseau 1755: 109)

The reason, again as we pointed out in SM, is causation. Thus, even if Bickerton appears to be willing to accept that development plays *some* role in evolutionary processes, he explicitly declares his skepticism (e.g. AT: 130) for evo-devo explanations, at least in the case of language origins. Indeed, given the way he conceives of his model, developmental processes are not regarded as independent causes, but rather, by appealing to the variation-limited vs. selection-limited dichotomy (Számádó & Szathmáry 2006), he is implicitly accepting that the ability of these processes to constrain is fully explained by natural selection, that is they are just the proximate manifestation of some ultimate, exogenous, cause, namely natural selection and/or behavior.

Compare now this picture with the following:

An equally tenable approach, which we advocate, is to adopt 'reciprocal causation' in evolutionary explanations, in which characteristics of orga-

nisms are regarded as caused by interacting bouts of selection and construction [...]. One important ramification of reciprocal causation is that it is philosophically sound to argue that developmental processes can be evolutionarily causal, as they are not regarded as fully caused by earlier selection on genes. (Laland *et al.* 2008: 552)

So much for our sins of commission.

As for our sins of omission, it was not our intention to use SM as a means to defend our own views, since we have already done that, in print, elsewhere (Balari & Lorenzo 2008, 2009a, 2009b). However, since Bickerton accuses us of using SM as some kind of diversion strategy, we would like to say something in this connection. And we will start by saying that, no, we are not crypto-Chomskyans garrisoned in the Hauser-Chomsky-Fitch barracks. Quite to the contrary. We have raised a number of criticisms to the HCF paper (Hauser *et al.* 2002); thus, for example, in Balari & Lorenzo (2008: 6) we concluded that:

[O]ur proposal is compatible [...] with the idea that the computational regime subserving the human language faculty also subserves other, non-linguistic aspects of human mentality, and, consequently that the narrow faculty of language of [HCF] is, in fact, not specific to language.

And in Balari & Lorenzo (2009b: 45) we wrote:

We only slightly qualified [HCF's] assertion, pointing at evidence which may indicate the presence of computational systems with degrees of complexity similar to the FL but associated with cognitive capacities that give rise to such disparate behaviors as nest building.

To which we could add (see Balari & Lorenzo 2009a) that HCF is often read and interpreted as a defense of the specifically human and specifically linguistic character of FLN, the computational aspect of language. This interpretation, we believe, is correct; but only partly correct. HCF also, and perhaps above all, is a defense of the singularity of language *qua* communication system and, in this sense, a particular application of the research program on the evolution of the communicative function developed by Hauser (1997), whose explicitly declared main goal is to determine the causes underlying variation in natural communication systems, human linguistic communication included. Indeed, one should read as a direct appeal to this framework the fact that one of the basic assumptions of HFC is that, despite the conspicuous discontinuity among the systems on which communication is based in different species, these systems are nothing else but several instantiations of a unique but highly diversified organic function (2002: 1569; especially, fig. 1). HCF constitutes, then, no more and no less than an attempt to provide an answer for the particular case of linguistic communication within a broader research agenda seeking the causal factors responsible for the piecemeal diversification of the communicative function in its evolution within different species (Hauser 1997: 1–2).

Now, Bickerton attributes to us the opinion that “the debate over the initial emergence of language is now ‘sterile’ and too ideological to be pursued further” (p. 128) but our opinion, clearly expressed in SM, is that the discussion about

*whether language has its origins in some form of ancestral communication* or not is sterile and too ideological, which is quite different. To extend a bit this idea, given that we have been given to opportunity to do so (but, again, see Balari & Lorenzo 2009a), we contend that language does not have its origins in some form of ancestral communication; it *cannot* have its origins in *any* form of ancestral communication. The reason is, ultimately, conceptual: There is nothing in nature corresponding to what we ordinarily call ‘communication’. Pheromone transmission in ants, the feigned wing injuries of golden plovers, the alarm calls of vervet monkeys, and human language are such an heterogeneous collection of phenomena that cannot in any sense be considered a ‘natural class’. In a nutshell, our point of view is that ‘communication’ does not refer to a natural class that can be the object of evolutionary process (see also Klopfer 1973). Bickerton (1990) could have delivered evolutionary linguistics from this ‘communicative fallacy’—directly instantiated in research programs like that of Hauser (1997), and indirectly, in that of HCF—but the truth is that, twenty years after, not only the fallacy still pervades this discipline, but also Bickerton has now become one of its most vehement advocates.

Finally, Bickerton reproaches us of not having addressed in SM what, in his opinion, is one the major contributions of AT: The explanation why the human mind is so radically different from other animal minds. He contends that it is language that created it and not the reverse, that is, he contends that the evolution of the human mind could not have given rise to something like language. True, we did not touch on this issue. It would not have been appropriate, since this is not really one of the topics developed in the book. As correctly pointed out by Bickerton in his response, “[AT] is about the transition from the alingual state that characterizes all other species to something that might qualify as a genuine precursor of language, thereby opening the road for [...] the subsequent development of true language” (p. 128)—so AT’s evolutionary perspective is exhausted with what Bickerton calls a “genuine precursor of language” and it does not make any genuinely new contribution concerning the role of ‘true language’ in the evolutionary modeling of the ‘true human mind’.

Our suggestion to the interested reader, in order to compensate for AT’s limitations, is to read Bickerton (1990) and Bickerton (1995).

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