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Neanderthals between Man and Beast: A Comment on the Comments by Barceló-Coblijn & Benítez-Burraco (2013)

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1. Neanderthal data and Neanderthal perspectives

Lluís Barceló-Coblijn and Antonio Benítez-Burraco (2013; henceforth BB13) have provided some insightful comments on and pertinent criticisms of my previous article 'The talking Neanderthals' (Johansson 2013; henceforth J13).

First, I appreciate their kind words about my review of the evidence. It appears that we largely agree on the facts of the matter, and also that we agree on the main conclusion of J13, that, as they express it, "Neanderthals had to count on some form of language" (BB13: 199). Our disagreements are more a matter of perspective, interpretation, and methodology. BB13 have two main criticisms:

- (1) They believe it is possible to infer "that the Neanderthal language was not like AMH's [anatomically modern human's] because it lacked *modern* syntax" (BB13: 199–200, original emphasis), and imply that I am too timid in refraining from drawing that conclusion in J13.
- (2) They disagree with my interpretation or methodology on a number of specific points throughout J13.

It is also interesting to read J13 and BB13 in the light of another recent review of the same topic by Dediu & Levinson (2013), published shortly after BB13. Dediu & Levinson reach largely the same conclusions as J13 (but along slightly different routes) and go one step further in that they (like BB13) do take a stand on whether Neanderthals had modern language. But their conclusion is the opposite from BB13: Neanderthals did have "essentially modern language" (Dediu & Levinson 2013: 1).

2. On language, communication, and productive debates

As BB13 now concede the presence of "some form of language" in Neanderthals, and also state that "[s]yntax is not an all-or-nothing question within this frame-



work” (BB13: 200), they apparently accept a non-monolithic language concept. But it is clear from their section 2.1 that we do not fully agree on the details, though it is less clear precisely what their language concept looks like.

Within this non-monolithic framework, BB13 still give a privileged place to AMH language, and find interest in Neanderthal language only as it relates to AMH language: “[T]he real productive debate is whether or not Neanderthals had the *same* faculty of language that [AMHs] have” (BB13: 199, emphasis in original) and “what is really worth characterizing is the proto-typical AMH language, and then to determine whether or not Neanderthals could have developed something like this” (BB13: 201). I agree that this is an interesting question. But I do not agree that this is the *only* interesting question, or even “the real productive debate” concerning Neanderthal language. Evaluating Neanderthal language solely on whether they match AMH or not is too flavored with *scala naturae*, with us as the pinnacle of creation, for my taste. Neanderthals, and the Neanderthal language faculty, are well worth studying in their own right, not just as poor relatives of AMHs.

If the Neanderthal language faculty were the same as the AMH language faculty, this would not tell us much new about the computational structures of language — but it would have far-ranging implications for the study of language evolution, falsifying a long list of AMH-specific hypotheses of language origins.

On the other hand, if the Neanderthal language faculty were *different*, as BB13 contend, and given that we already agree that they *did* have some form of language, and thus a language faculty, this would show that there is more than one way to build a language faculty. Such a discovery could catalyze fruitful investigations into possible alternative structures of language faculties, today overshadowed by our focus on the AMH language faculty.

The choice of null hypothesis is a key issue here (cf. section 2.4 in J13). BB13 state that “... we actually can proceed with a null hypothesis: In our opinion, current evidence supports that the Neanderthal language was not like AMH’s because it lacked *modern* syntax...” (BB13: 199–200). This sounds like they are positing their conclusion as null hypothesis, which skirts very close to the *petitio principii* fallacy, assuming your conclusion. Later, they state that “[i]n fact, it is our contention that the available data do not support non-AMH hominin ‘languages’ being syntactically structured like AMH languages are” (BB13: 201).¹ I actually agree with this contention — but also with its complement, that the available data likewise do not support that non-AMH languages are *not* structured like AMH languages. As I said in J13, “whether they had syntactic language can be neither confirmed nor refuted.” (p. 23). In the absence of evidence, the null hypothesis becomes the conclusion by default. There are several statements in BB13 where they reiterate their assumption that AMH language is unique among hominins, notably in their conclusions at the end; but nowhere in BB13 can I find any actual positive evidence supporting this assumption (as opposed to a lack of evidence to the contrary).

On syntax, I can concede that I did not define it carefully enough in J13.

¹ The scare quotes on ‘languages’ here seem to contradict their earlier statement that “Neanderthals had to count on some form of language” (BB13: 199). Did Neanderthals, in the opinion of BB13, have language or ‘language’?

Instead of going into a full discussion here of all the complexities of this issue, I will in the interest of brevity simply state that I largely agree with the model posited by Jackendoff & Wittenberg (in press); see also Johansson (2005). This circumvents, among other things, the false dichotomy that BB13 raises between syntax in the loose sense 'to put in order' and in the strong sense of modern AMH syntax.

Concerning lexical semantics, there are indeed many current theories on lexicon structure, another issue that I will abstain from discussing here at the length it properly deserves. Suffice to say that most people agree that modern language contains some kind of trilateral mappings between form, meaning, and syntactic features. There are various frameworks for describing this mapping, including both frameworks that don't call it 'lexicon' at all, and frameworks that give primacy to the lexicon over syntax (e.g., Boeckx 2013), but I will leave that aside; *some* way of connecting form and meaning, and plugging it into syntax, is required for modern language. Removing one of the three sides of the trilateral mappings leaves something that one may or may not wish to call a lexicon, but it is not logically incoherent; it is simply a bilateral mapping. Boeckx (2013) apparently argues for an early stage in language evolution with bilateral mapping connecting syntax with meaning, lacking the externalization ('form') mapping. I would instead argue for the possibility of a bilateral mapping with form and meaning (cf. Jackendoff & Wittenberg, in press); such a bilateral mapping, freely extensible, is the minimum required for me to call something 'language'. I concede the logical possibility (if not the plausibility; see Johansson, in preparation) of the scenario of Boeckx (2013), but would not call it language.

Ape 'language' (Kanzi *et al.*) is invoked by BB13 in an apparent attempt at a *reductio ad absurdum* of my language definition, both concerning syntax and lexical semantics. This *reductio* fails on several points: (1) it works as a *reductio* only if ape language is inherently absurd, which it is only if language in all forms is assumed *a priori* to be unique to humans, (2) apes do not display these language-like behaviors in the wild, only when taught by humans,² (3) there is scant evidence that apes do any (proto-)lexical mapping on their own, beyond those mappings provided by humans, and (4) the productive 'syntax' displayed in ape utterances fits only a very loose definition of syntax, much looser than the one I adopt from Jackendoff & Wittenberg (in press).

The issue that BB13 raise concerning function versus structure (p. 202) is not a productive debate. It is a fallacy to place questions of structure and function in opposition — instead they are complementary questions, belonging to different levels in Tinbergen's (1963) classification of explanations in biology. The function and the structure of a biological feature, such as language, are both interesting questions, and neither should be neglected in a proper biolinguistic analysis, nor should either be given primacy over the other. With that said, there are methodological considerations involved in inferring structures and functions in extinct species, that BB13 do not fully take into account. To put it briefly and simply: Only structures that fossilize can be studied directly; functions may be

² *Why* apes have a capacity to learn language-like behaviors, a capacity that is to all appearances unused in the wild, is a very interesting question, but is beside the point here.

inferred either from fossilizable structures, or from behavioral traces; non-fossilizable structures, such as computational devices, can only be inferred indirectly through the behavioral traces produced by their functioning. Even for modern humans, where we have full access to both behavior and soft tissue, there is still no consensus on the structure of the computational device behind language.³ For this reason, the emphasis of BB13 on the evolutionary trajectory of this computational device, ahead of function, is misplaced, as this is effectively unknowable without going through functional inferences that are difficult and contentious even in living humans.

3. Interpretative and methodological issues

3.1 *Globular or lateral brains?*

Concerning the neural substrate of language, BB13 first invoke allometry and the structural changes that may follow from size changes. This is highly relevant when comparing for example the brains of humans and chimpanzees, as it is a non-trivial issue to disentangle which structural differences are just byproducts of the threefold size difference. But as there is no significant difference in size between Neanderthal and AMH brains,⁴ allometry does not contribute anything informative to the issue at hand. The average brain development trajectory, as invoked by BB13, may indeed be different (Gunz *et al.* 2012), though this issue is not totally settled yet. But it remains to be shown whether this difference is relevant for language. As noted by Benítez-Burraco (2013), language ontogeny in AMH is highly robust against perturbations, presumably including the full range of development trajectories of AMH, and there is no evidence that this robusticity does not extend to the Neanderthal pattern.

The argument from globularity of Boeckx (2012) is more interesting, as it focuses on the main difference between Neanderthal and AMH skulls (and presumably brains), the more globular shape of AMH skulls. But while the idea is intriguing and well worth pursuing further, especially in connection with developmental patterns, at present the proposed link from globularity to language is purely speculative and cannot warrant any conclusions concerning Neanderthal language.

On lateralization, I do not see any major disagreements between J13 and BB13. As is clearly stated in J13, the proposed link between handedness and language is not strongly supported, and cannot stand on its own as evidence of language. But I think BB13 are overstating their case for continuity somewhat; while lateralization in various respects is indeed ubiquitous and ancient among many animals (not just mammals), the population-level handedness ratio of humans is not.⁵

³ Nor is there consensus on whether the primary function of language is communication or something else, though the vast majority of language-evolution researchers work on communicative hypotheses. But in the interest of brevity I will leave that debate for a different day (Johansson, in preparation).

⁴ If anything, the Neanderthal average brain size is slightly *larger* than that of AMH. But the difference is slight, and all known Neanderthals are well within the AMH size range.

⁵ Whether there is *any* population-level handedness among other apes remains a contentious issue (J13: 47, fn. 7). Hopkins and associates, cited by BB13, are just one side of that debate.

3.2 *On linguistic genotypes*

On the genetic issues, there is again little disagreement between me and BB13 on substantial issues, but much disagreement on interpretation.

Concerning FOXP2, as noted also by BB13 this is a gene with both sequence and physiological role highly conserved, certainly among mammals and likely among vertebrates.

Concerning other possibly language-related genetic changes in the human lineage, Dediu & Levinson (2013) review these in more detail than either J13 or BB13. In the interest of brevity I will not go through the whole list here, but instead just state that I largely agree with the analysis of Dediu & Levinson. Worth mentioning is just the work of Maricic *et al.* (2013), cited by both BB13 and Dediu & Levinson (2013), according to which a regulatory region of FOXP2 has changed in AMH but not Neanderthals. What Dediu & Levinson (2013) but not BB13 mention is that Maricic *et al.* (2013) find the ancestral allele present at a frequency of about 10% in some modern African populations. If this change made a key difference with respect to language, the effect on language in these populations ought to be obvious.

The statement of BB13 that “even if they [Neanderthals] were endowed with the same ‘linguistic genotype’ [...], we cannot automatically rule out the possibility that the former had a different faculty of language” is not supported by the argument of Benítez-Burraco (2013) that language development is highly robust in humans, also against variations in the ‘linguistic genotype’: “In particular, we argue that developmental dynamics (and hence, an assorted set of regulatory factors) strongly canalizes variation, to the extent that the same phenotype can robustly emerge at the term of growth from diverse genotypes” (Benítez-Burraco 2013: 1). It is technically true that different outcomes from the same genotype cannot be ruled out — in some contexts this is even fairly common, influenced by environmental cues (cf. West-Eberhard 2003). But language development of AMHs is clearly highly robust against variation in the external environment, with AMH babies throughout history developing normal AMH language faculties despite a range of environmental variation that encompasses and exceeds the typical environment of Neanderthals. What remains is the possibility that the ‘internal environment’ in the child during ontogeny differs systematically in Neanderthals in ways that go beyond the robusticity limits of language development; it is unknown whether this is actually the case, and pure speculation either to assume that it is or that it isn’t.

3.3 *Introgression among strawmen*

In their section 2.4, BB13 say they are arguing against section 5 of J13. However, the position that they are apparently attacking is nowhere to be found in J13.

Their statements — “[h]ence, one cannot take granted that Neanderthals automatically had full language” (p. 205) and “[h]owever, our main criticism against any relevant role of the interbreeding fact in granting Neanderthals a modern faculty of language is of quite different nature” (p. 206) — indicate that BB13 are arguing against someone who believes that interbreeding proves that Neanderthals *had* full language. Whoever that someone might be, it is not J13,

where this conclusion explicitly is *not* drawn, notably in the two sentences quoted by BB13 on their page 205, repeated here in full from J13 (p. 18): “Evidence of successful interbreeding would thus add some modest weight to the case for Neanderthal language, despite some caveats about heterozygotes and mating systems. But it is not clear what form of language is supported.” The second sentence in the quote makes it abundantly clear that J13 does *not* jump to the conclusion that Neanderthals had full modern language, but instead leaves the issue open. It is also explicitly stated in J13: “[W]hen I talk about ‘some form of language’, this includes proto-language” (p. 7), but in several places BB13 are arguing as if “language” in J13 means full modern language. BB13 are criticizing a strawman here, possibly caused by misreading how I use the word “language” in J13.

Another strawman is erected in the final paragraph of section 2.4 in BB13. Here they are apparently arguing against a claim that introgression from AMHs to Neanderthals transferred ‘language genes’ to the Neanderthal population, giving them language. I fail to see how anybody could possibly misunderstand J13 as making such a claim. Possibly Benítez-Burraco (2012), where the same counterargument appears, is contaminating their reading of J13?

Concerning North Africans (BB13: 205, fn. 8), their point is technically correct, but irrelevant. I wrote “Africans” as shorthand for “Sub-Saharan Africans” (J13: 17), which was admittedly sloppy, but the status of North Africans (Sánchez-Quinto *et al.* 2012) has no impact on the argument of J13, which does not hinge on the specific AMH populations possibly affected by introgression.

BB13 are quite correct in noting that introgression is common in the animal world, and I would not be surprised if AMH and Neanderthals did interbreed. But BB13 jump to conclusions too quickly when they treat interbreeding between AMH and Neanderthals as if it were a proven fact. The case for interbreeding is still not robust enough for categorical statements like “Neanderthals and AMHs interbred” (BB13: 205) or “the interbreeding fact” (BB13: 206), even though some further indirect support exists from, for example, Yotova *et al.* (2011).⁶

3.4 To knot, or not to knot?

On the ‘symbolic’ archeology, I see no major disagreements, except the issues of structure vs. function and language *sensu* J13 vs. language *sensu* BB13 already discussed earlier.

When it comes to alleged archeological proxies for syntax, however, our conclusions differ substantially. As noted by BB13, the attempt by Camps and associates (Camps & Uriagereka 2006; Balari *et al.* 2012) to tie a knot between knots and syntax is given rather short shrift in J13. This is not only because I find the knot-syntax connection *per se* unconvincing (cf. Lobina 2012; Lobina & Brenchley 2012), but also because it would be uninformative with respect to Neanderthal language even if it were established that a knot is a proxy for syntax. As noted in J13, we have no direct evidence of knot-making among Neanderthals. But we do have indirect evidence in the form of technologies — hafting (e.g., Cârciumaru *et al.* 2012), clothing (e.g., Wales 2012), and possibly

⁶ Incidentally, Yotova *et al.* use the same “African” shorthand for which BB13 berate J13.

pendants (e.g., Zilhão *et al.* 2010) — that typically involve knots, so it would be imprudent to assume that a Neanderthal couldn't tie a knot, and unwarranted to conclude from this that they had no modern syntax.

The various connections between language and the motor system invoked by BB13 in this context are unobjectionable in themselves. But BB13 fail to show how these points tie into the knot-syntax argument. A general language-motor tie is not evidence of a specific knot-syntax connection.

Otherwise, I have considerable sympathy for the hypothesis that the computational machinery behind language is not domain-specific, but used for computations in many areas, be it tool use (cf. Barceló-Coblijn & Gomila 2012), navigation (cf. Kinsella 2009), music (cf. Asano 2013), or whatever. I am just not convinced that actual knot-tying (as opposed to doing knot theory) is one of those areas.

3.5 *Who is dynamic, and who is static?*

In support of the knot notion, BB13 invoke the cultural dynamism of AMH. How knotting causes dynamism is not shown, but the issue of cultural dynamism is interesting in its own right, with or without knots, and I thank BB13 for pointing this out, as J13 did not give it enough attention.

By 'cultural dynamism' I presume that BB13 mean the much more rapid rate of cultural and technological change, and the cumulative effects of such change, in some human populations compared with earlier hominids. This difference in rate is certainly real when comparing *Homo* with other extant apes, among whom the rate of cumulative cultural change is indistinguishable from zero. And even in *Homo erectus* the rate is very modest, with a recognizably Acheulean tool kit changing very gradually over a million years or so. But during the last few hundred thousand years, the situation is more complex. Compared with today's Western culture, the rate of change remained glacial well into the Holocene — but compared with any previous population, it accelerated dramatically, both among early AMHs and among other contemporary humans, including Neanderthals. In the last 50,000 years, further acceleration took place among some, but not all, populations of both AMHs and Neanderthals. On one hand, the Châtelperronian shows the accelerating cultural dynamism of a subset of Neanderthals (see, e.g., Soressi *et al.* 2013 for one recent piece of evidence) — and on the other hand, the recognizable continuity of San culture back to 44,000 years ago (d'Errico *et al.* 2012) shows that not all AMH populations accelerated in the same way. Cultural dynamism is an issue that clearly deserves more attention as a proxy for cognitive evolution, and most recent AMHs are indeed more dynamic than most Neanderthals — but it is far from a clear-cut case of all AMHs being dynamic and all non-AMHs static.

It is also interesting to note that BB13 invoke differences in working memory (Coolidge & Wynn 2005) as an explanation for the difference in dynamism. This is a defensible, if speculative, hypothesis — but working memory is part of the performance system, well outside the core language systems, and the dynamism issue would in that case *not* support any difference in core syntax between Neanderthals and AMHs.

4. Summing up: Stringent, stringenter, stringentest

Both J13 and BB13 agree on the basic methodological issue that we should not seek a single “magic bullet” proof of language, but instead look at the total pattern formed by all the various proxies and other types of evidence available. Likewise, we both agree that our own implementation of this methodology is the proper stringent one, and in the other paper it is done wrong.

But I also believe that BB13 in their Section 3 are barking up the wrong methodological tree. Our different conclusions do not really hinge on differences in how we apply the pattern-forming methodology that we have in common. Instead our key differences are two:

(1) *What, if anything, is the proper null hypothesis in Neanderthal studies?*

As discussed in section 2 above, BB13 posit a null hypothesis that, in the absence of positive evidence one way or the other, subsequently becomes their conclusion. I think it is methodologically more stringent to avoid *petitio principii* and refrain from conclusions in such a case.⁷

(2) *Is language a monolithic integrated entity, or can there be different ways of having language?*

My position, here as well as in J13, is that we should not assume *a priori* that language faculties that are not identical to the AMH one are impossible. The position of BB13 on this issue is unclear, or possibly inconsistent; on one hand, they admit that Neanderthals had “some form of language” (p. 199) or “could have been endowed with regular-like grammars” (p. 210) but deny them full modern language, which entails that BB13 grant Neanderthals a language faculty different from the AMH one. On the other hand, in Section 3 they appear to be arguing that language is “an integrated entity” (p. 209) and “it is not so much a matter of when *a* component of language appeared, but, above all, of when *all* the components were put together” (p. 209, emphasis in original), which sounds more like an argument for a monolithic indivisible language faculty. And throughout their paper, as discussed in section 2 above, they argue as if the only issue were whether Neanderthals have an AMH language faculty or not, which likewise sounds as if they believe different language faculties are either impossible or irrelevant.

We agree that somewhere along the human lineage things happened that “improved the computational abilities” (BB13: 210) that are relevant for language. But unlike BB13, I would argue that it remains to be shown both (i) whether this was a single step, or multiple steps, and (ii) whether the step(s) took place before or after the split between AMHs and Neanderthals. We agree that Neanderthals had some kind of language, and thus some kind of language faculty, which entails that at least some of the above-mentioned steps took place before the split. But for the reasons given throughout both J13 and this paper, I remain agnostic on how the language faculty that we agree that the Neanderthals did have compares with the AMH language faculty.

⁷ I likewise believe that Dediu & Levinson (2013) are somewhat premature in jumping to the opposite conclusion from BB13.

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