(Inter)views on Biolinguistics:
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Pedro Tiago Martins (PTM) – *I'll start by asking what you think language is.*

Marc Hauser (MH) – I think language at its foundation is a biological process. It's anchored in certain kinds of generative procedures or operations that basically enable a wide variety of expressions to be formed—meaningful, and meaningless—, internal to the head and also externalized. Crucial to that I would say are three different kinds of representations: syntactic, semantic and phonological. And, of course, those have to talk to each other in order for this system to work. And then, importantly, these operations and representations may then be externalized into spoken or sign language, with various kinds of ways in which we interact socially, different dialects and so on.

PTM – *And as a biologist, do you see in language something different from other things you've studied in the past? Is there something that makes you think that something in language is different, and that you cannot approach it the same way?*

MH – I don't think so. As a biologist, I think there is something that one might call the language phenotype. I don't think language is one thing; I think the definition has to be broken up. So I think the questions are similar, but evo-

lutionarily the answers are harder, in terms of how did it evolve, when did it evolve, why did it evolve, etc. But in terms of the different ways in which biologists approach questions from issues of development and mechanism, I think it's another kind of process. So I think you could approach it with the same kinds of tools. I guess that what makes it more complicated is that unlike studying, you know, the thumb or the knee, the phenotype is harder to define. Also, and as I discuss in an upcoming paper with several co-authors, including Chomsky, is that for some questions, the evidence is very hard to obtain, and may never be obtainable.

PTM – *In your talk yesterday,*[1] *you revised Darwin's quote,*[2] *and you claimed that there is a difference of kind, but isn't this in terms of cognition, almost simply behavior? Would you say that biologically the mechanisms that are behind that faculty, even though it's a bunch of things together, show discontinuity too?*

MH – I think there are some discontinuities, and I think there is some continuity. So, for example, the system that allows for us to perceive speech—our hearing system—is very conserved with other primates. There's been very little change in the hearing system of chimps and humans. That's very conserved. The vocal apparatus has changed. Tecumseh Fitch has sort of shown that other animals have the capacity to some extent lower their larynx, but a developmental change like we've seen in humans where the vocal tract of a young baby is more like that of chimpanzees, and then at around four months the larynx really descends and locks into position, that you don't see in the other animals. The vocal apparatus was reshaped, and so in terms of the production system…the other thing that's really changed…there's really a discontinuity. So the hearing system is a continuity, I think. As for the vocal tract itself, I think there's some discontinuities. I think what's really discontinuous—and this reflects what I said yesterday—is that what makes the internal process of the language, the competence, the faculty, and all its pieces really different from anything in the animal world that's externalized in communication is the separation from modality. So, obviously, if we lose hearing we turn to a visual modality, and other animals simply can't do that. So, if you were to take a songbird and cut its larynx, it would have no capacity to do in the visual modality what it can do in the auditory modality, so that's a complete discontinuous capacity. You know, other things like the

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[1]"Where Darwin went really wrong: An unbridgeable gap in cognitive evolution", talk delivered on March 13, 2014 at the University of Barcelona.
[2]"[…] the difference in mind between man and the higher animals, great as it is, is certainly one of degree and not of kind" (Darwin, 1871, 105).

conceptual-intention space, I think there's some continuity there. It's not as if animals have no concepts; they have concepts and often pretty rich ones. What they don't have is a way of externally symbolizing those internal concepts and to symbolize anything, the way we do. I think the evolutionary pattern is one where there are both discontinuities and continuities.

PTM – *If you have to refer to language as the conjunction of all of that, would you rather say there's continuity or not; which one wins?*

MH – I'd say there's discontinuity. I think if we define language as generative procedures, these different representations, and how they interface, I think that's completely discontinuous.

PTM – *And do you think there are some mistakes in the way that people approach these questions? Is there something we ought to do in order to better understand this?*

MH – Yes, I do, including my own work. I think there's been at least two problems. One is, I think, that there's been a tendency to see certain kinds of similarities as providing the evidence you need for thinking of the primate past as a foundation for language. And I've done that myself, so I think the error has been that when you look at those similarities, it's very hard to see how you would get from there to the current situation. That's not true in a lot of evolutionary analyses. When you look at, for example, the anatomy of the opposable thumb, which I showed a slide of last night, there it's really easy to document the progression from one piece to another. Even when it comes to behavior, for example, chimpanzees show this kind of coalitional violence, where they group together, and when they've got an asymmetry in power, they'll go and attack a lone individual and kill it. That, as Richard Wrangham has described, is something you see in human warfare all the time. Of course, we can do that, in some ways, on larger scales. Army troops of a thousand against army troops of a hundred. They [the chimpanzees] don't do that, but that's a scale issue. That scale issue has implications for the psychology of what humans do and what animals don't do, but you can see a progression there, I think. So I think that the error has been to jump too quickly from simple similarities to an evolutionary account, that's the first thing. The second thing is probably more problematic. Consider researchers who have managed to train animals and show greater capacities compared to what you see in their natural habitat, in the wild. Or consider those who have used spontaneous methods in captivity. What few of these researchers have done, and I will include myself here (at least a prior "me") is to step back and ask "well, if the

mechanism for acquisition is different, then maybe the underlying mechanism is completely different altogether." And that, I think, has led to some pretty big mistakes. There's a paper that just came out, about a year ago maybe, in Cognition (Rey et al., 2012), with baboons, where they train baboons to recognize patterns that were like center-embedding, and they concluded that because of this, you know, the Hauser-Chomsky-Fitch hypothesis of recursion (Hauser et al., 2002) is wrong, animals can do center-embedding…But the problem is that, first of all, we never said that center-embedding was unique to human's language faculty or the critical process, but more important was the fact that there's two problems with the result: one is, again, it took them thousands and thousands of trials to show that the animals had anything remotely like center-embedding. But worse was the fact that the generalization never went past 2 center-embedded structures. Center-embedding as a capacity should not be limited, at least as a competence capacity, in number, though performance factors such as memory may constrain what is demonstrated; they never showed that the animals could generalize to larger numbers, and a constraint of 2 is unlikely due to memory. So, even if you grant the results, it has nothing to do with center embedding in humans. So, again, even though structures look like they were center-embedded, they don't generalize to what happens in humans. So I think the problem is that if you use very different methods, specially training with reinforcement, it gives you the illusion that the behavior looks like human's, but the underlying mechanisms are very different.

PTM – *You mentioned your paper with Noam Chomsky and Tecumseh Fitch. It's been 12 years now. Has it had effects that went completely beyond your expectations? Do you think people have interpreted it the wrong way?*

MH – Yeah. I guess I have been very surprised. I guess, you know, anything that has Noam's name on it will get attention (for good and for bad as he often remarks to me!), so I think by default is was going to have effects. It's certainly the most cited paper I've ever written, so that's been kind of nice. It's been massively misinterpreted by a lot of people, but I think for interesting sociological and political reasons, more than anything else. For example, and this was actually a fault of ours, we were forced to write a very short abstract, and it didn't really match exactly what we said in the paper. So many people interpreted the thesis as us arguing that language is just recursion. Now, I mean, nowhere in the paper do we actually say that, but that's how it was interpreted, in part because some people think that all Noam thinks is language is just recursion. But even *he* doesn't think that! So, that's been a deep misinterpretation.

The other way we've been misinterpreted is that we actually never really said in the paper "this is how language evolved" or "this is what it is"; we just simply provided a framework—that's how we saw it—for thinking about the problem. What is important, so we suggested, is to distinguish between i) capacities that are shared with other animals, but there are deployed in language, like memory and breathing and attention, but not specific to language, ii) capacities that are maybe unique to humans, but involve language and other domains of knowledge, and then iii) things that are unique to language. What is interesting, at least to me, is the possibility that FLN is an empty set (see Fitch et al., 2005); that there's nothing in it, there's nothing specific to language. I don't know. The exception could be the interfaces. The recursive generative procedures are, at some level, part of domains of knowledge, and thus can't be unique to language. Merge, for example, is a mathematical concept, so it can't be unique to language. But how merge operates over conceptual-intentional representations, may be unique to language, at least in terms of the objects it creates. These are some of the reasons why we proposed that FLN is the recursive mechanisms as they interface with conceptual-intentional and phonological systems, because that's what makes language unique; it's those interfaces. So I think there has been a lot of misinterpretations, but there's also been a lot of good things. Obviously a lot of people took seriously the FLN/FLB distinction; I think that has provided a kind of a framework for many. I think the paper roped in linguists in a way that they had not been brought into the conversation before. For me, that was the greatest success, in some ways. There's obviously a reason why I wanted Noam to be on that paper, and it was in part because it sent a flag about the possibility of unification across disciplines. So I think that has been the key result of our paper: it has engaged linguists such as Cedric [Boeckx] to engage with the problem in a new and productive way. And that, to me, is a success.

PTM – *If you look at the history of biolinguistics, you see a sudden increase of papers, conferences, and books on the topic after that paper. So, in that sense, it's clearly had a good effect. But do you see a field when you think of biolinguistics? Do you see something well established, or do you think that many people still misinterpret biolinguistics as an entreprise as well? Sometimes it seems that people take biolinguistics to be something which simply it's not. They think it means reading a news piece on a gene and saying "oh look, it's the gene for language, it's biolinguistics!"*

MH – I think some people interpreted it as simply a Chomskyan agenda, and I think that is and was wrong. Some even perceived it as a minimalist agenda. I

think that is wrong too. In the originally submitted version of our paper, there was almost no mention of minimalism, and what's interesting is that in the original response to it by Steven Pinker and Ray Jackendoff (2005), they had an entire section where they went on and on about minimalism. We responded by saying "we don't even mention minimalism, how can you go on and on and on about minimalism?" They agreed and took it out of their reply. That was a clear case where they assumed it was a paper to promote Noam's views about minimalism, and thus launched a critique that was misplaced. But I think, again, because of Noam's presence, they saw that as Noam's attempt to get minimalism into biology. That wasn't Noam's interest, and certainly wasn't mine or Tecumseh's.

PTM – *So what was Chomsky's role in the paper?*

MH – Noam and I initially had several email correspondences and conversations about his views on language evolution and the role of biology more specifically. I think it is fair to say that Noam felt misinterpreted. Based on these exchanges, I proposed to him that we write a paper, both to better explicate his views and show unification between a biologist and linguist. We also invited Tecumseh onto the paper as he had very similar sympathies, and also ideas. Tecumseh and I wrote a significant amount of the first draft. Noam was fundamental in guiding a lot of the ideas, and with infusing critical commentary. In the end it was a clear collaboration. Oddly, many thought that Noam had written the entire thing and that it was just one more of his agenda pieces, and especially one peddling the minimalist program. But as I said above, this was false. Nonetheless, I think the paper helped fuel biolinguistics, although I think this disciplinary approach still has problems. I was talking to Cedric about this a little bit yesterday—I think the problem is I still see the animal work as being fairly peripheral to the lives of linguists and psycholinguists. And that may be a reflection of what I was being critical of yesterday, but there's a big difference with having a discipline where the body of work really informs your daily practice and one in which people merely show interest because the findings are intellectually of interest. So for example, take language acquisition: that work I think in many ways really began to inform theoretical linguistics because how the baby... you know, competence/performance issues, what's acquirable from early on, different patterns... that really began to inform theories, and theories began to inform the data collection. When the work by Jenny Saffran was coming out, you know, a lot of people jumped on that, because, well, how far could you get with this kind of statistical approach? And of course Noam immediately remarked "you're never

gonna get anywhere;" we know that by default. But that field kind of moved on its own for a long time, without impact, and of course, they hit the same road-block that we did with the animal work which is that it's very hard to show what kinds of processes are actually going on, and even when you do, it is hard to show that these processes are sufficient or even necessary to account for language acquisition. But this is at least a case where I think that both the theoretical side and the practical, empirical side were interacting. In contrast, if you look at most books in linguistics, and even the biolinguistics books, though they have chapters and papers by people working with animals, I don't get a sense that it affects the day-to-day work that's coming out. Cedric is unusual in that sense, because he does actually read the animal work, but he's unusual. Most linguists, someone like Norbert Hornstein, is interested because he is a smart and well-read linguist, but it doesn't impact his theoretical work.

PTM – *Do you think they just nod...*

MH – Yeah. I mean, they know it exists, but who cares if birdsong does whatever it does?

PTM – *So, let's say, if we were to discover something that completely changed the paradigm, something about the evolution of language, it wouldn't really affect the role of linguists, because they would just say "well, this is far back, I'll just keep drawing my trees and whatnot; it won't change my work"...*

MH – Well, I guess that's the question. My long term vision was—before I realized how hard the methodology was—that if I can show that animals have some kind of access to some kind of generative procedure, that this would inform how linguists go about their day job. They would have to think about which generative procedures are really critical to language, which are shared with others, and what makes linguistic processing unique. In an ideal world, someone would develop a technique that doesn't require training, and that can show that animals access a generative procedure in recognizing a patterned set of inputs, generalizing beyond the initial presentation.

PTM – *It's usable...*

MH – Yeah; It's usable in the broad sense. Well, I think that would force the theoretical syntactician to change his or her views. If, for example, center-embedding was demonstrated in an animal, that would show that this kind of computation is not specific to language or to humans. It would be part of FLB, because many animals have it (unless you want to give animals language). What

might be unique is how center-embedding, as a computation, interfaces with the conceptual-intentional system of representations. That would, I would think, force linguists to look for other kinds of generative computations that maybe you would not find in animals, and that could begin an interesting process. So, you know, when Ansgar [Endress] and I did that paper with Tamarins (Versace et al., 2008), on prefixes and so forth, that was the idea. It was, maybe just simply, the notion of something preceding something else is just a common pattern that animals can recognize regardless of what it is, and if that's true, then that pattern recognition is what's at the base of prefixes, the specific phonology—along your interests—is what we do. That's the interface issue. So that begins again to change this FLN/FLB relationship and says, ok, the pattern of the prefixes, that is an FLB issue; how it interacts with our distinctive phonological representation, that's the FLN issue. So I think that changes people's work. That's what I could imagine. If the work went in that direction, that would be exciting, that's what the next generation has to do, to really keep in mind the FLN/FLB distinction and ask what kind of evidence do we want to see with animals that would force us to change where we put things in terms of FLB/FLN. That's to me ultimately the interesting question, which is, is there anything actually particular to language? What I'll say today about evil[3] is that there's nothing particular about the nature of evil; it's all combinations of things that are basic to other domains. But that's obviously a large question in cognitive science, what is particular to a given domain, if anything.

PTM – *You kind of covered two questions that I had: one was what are we doing wrong and the other how we should go about this. Is there any special piece of advice or message you have for people who are now engaging with these issues, specially people who come from linguistics? I'm asking this because it seems to me that if you come from biology or anthropology, it's less of a shock in terms of looking at different data and from different perspectives. So what would be your advice?*

MH – I think it depends what direction you're coming from. If you're coming from linguistics and try to take on a biolinguistic approach, I think in some ways what Cedric did early in his career was really good: he steeped himself in the literature of biology: how do biologists who study animals in the wild do their work, how do people who work in captivity do their work, how are they interpreting their data? To really become well-versed with it. On the opposite side I

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[3]"Evilicious: How we evolved and developed the capacity for gratuitous cruelty", talk delivered on March 14, 2014 at the University of Barcelona.

think that a lot of people who are coming from biology, psychology, and anthropology don't really understand language, and so they go about doing their work and they have a very superficial understanding of language, so they're constantly focusing on communication as if that is language. And that's a mistake, and so part one I think would be to understand each side's literature. The second thing, which I think is the most productive, is collaborations, forming real collaborations. I was at Cambridge University before coming here, and I was talking with Bert Vaux, a phonologist, and we were talking about this dog work, with the dog Chaser, with object name recognition. There are some potentially interesting questions there, like phonotactics and endomorphology: when this dog hears the word "ball", does it actually hear [bɔːl]? Could you say "tall", "doll", "crawl", and he would go get the ball every time? So, how strong are the phonotactics? Those are interesting questions about perception, because you've only given this animal a certain set of things, how much tolerance does he have? Could you show speaker invariance? There's all sorts of questions there. Those are interesting questions about phonology. That's something that, for you… there's dogs everywhere in Barcelona, it wouldn't be hard to bring dogs into a lab and test them. So, take that as an example, as a linguist, what species should you test? I just suggested, well test dogs, and then you could ask the question, well, our evolutionary ancestry doesn't go to dogs; there's a big gap between dogs and us. But if you're testing the thesis "is the capacity unique to humans?", at some level it doesn't matter what species you show it in. Of course, it's possible, as we talked about yesterday, that maybe dogs have it, and it was lost as a capacity, and chimpanzees and monkeys don't have it. Working together with someone with different expertise, such as you as a phonologist, allows the ideas to develop. Given your interests in the evolution of phonology, you could profit from contacting labs that are doing work on animals, where they might think "that's a pretty cool idea, I'll happily test that", and you test it with them. When Ansgar Endress came to work with me, he didn't know a thing about animals; he had never worked on animals. Yet he had these great skills in psycholinguistics that I could learn from. That was a great postdoc, because he taught me and I taught him, and that's the best kind of relationship. So I think, if the field is going to move, it's going to require those kinds of collaborations: biologists learning more about language and linguists learning more about biology, and then putting that knowledge into the collaboration.

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