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Is our mental grammar just a set of constructions? Commentary on Evans 2014

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According to this book, ‘each of us carries around in our heads a “mental grammar” far more impressive than any written grammar’ (6); this grammar is made up of ‘abstract rules’ that can generate a ‘seemingly infinite number of novel sentences’ (12). Since this mental grammar has properties, we could decide to be interested in them and seek to make them formally explicit. In that case, we could decide to investigate my mental grammar or yours, my grammar as an infant or as it is now, or even a more complex and abstract construct such as the ‘grammar of English’ or of French. Given that the above statements are made in a general fashion, however, that is, with respect to all (neurotypical) humans, we could also be interested in mental grammar as such, which exists in all human heads. We would then make assertions about grammar in general: this would be ‘universal’ grammar in a technical sense. A rational discussion can now arise over the properties ascribed to this mental grammar, and how, in general, it is acquired, for the goals of this research program are in fact SHARED with the generative tradition attacked in this book (though referring to it throughout as the ‘language-as-instinct crowd’ and presenting it as a bunch of lunatics living in a Hegelian dream world clearly will not help either the field or rational discussion).

Regarding how the grammar is acquired, Chomsky in particular has tirelessly argued for a half-century that the so-called ‘innateness hypothesis’ with respect to language acquisition is a trivial claim rather than a profoundly controversial one. All parties agree that acquiring a language is not like learning to play the piano. The former is universal (present in about 7 billion people), not only as such but also in terms of its development along biologically timed milestones. Hence it is not a particular achievement for which special effort or talents are required. Its acquisition is actually very hard to PREVENT—so hard that trying to do so would amount to an atrocity. It would be nice if the same were true of piano playing. So in whatever sense it would be true that language is not ‘something that emerges automatically, and effortlessly’ (3), it would also be false. The same applies to Evans’s fervent rejection of the claim that language is an ‘instinct’ (99–100). In whatever sense this claim would be false, it would also be true, for the reason just given: language

is an instinct in the same sense in which piano playing or cooking is not. Does E really think that Pinker's use of the term 'instinct' in informal expositions implies that he thinks there are no differences in how an infant acquires language and a spider spins a web (100–101)?

In the same sense in which language has features of an instinct, it is also uncontroversially not simply acquired from use, since cats, chimpanzees raised in a human home, or canaries do not acquire language from the use of it to which they are constantly exposed. Up to at least 25% of children on the autism spectrum (Tager-Flusberg et al. 2005), as well as children with nonsyndromic language delay due to specific genomic changes (Speevak & Farrell 2011), never acquire functional language either—it would be cynical to suggest they should pick it up from its use. Home-signing children do acquire language, but again not from its use, since they have not experienced any. Deaf language-less adults with otherwise normal cognitive abilities do not acquire language from its use either: even signed language can be a complete mystery to them, despite their normal desire and intent to communicate and to socially engage (Schaller 2012)—to make them even aware of the concept of language can require extraordinary efforts. Language is not experientially *THERE* in this sense, for those who do not already know or expect it, as very young infants instinctively do when distinguishing speech from nonspeech sounds long before even knowing the first words (Vouloumanos et al. 2014).

The moral is that language can only be acquired from use if the genetic preconditions are right. If they are, language can *ONLY* be acquired from use, of course, as no one has ever denied (with certain qualifications to this claim for the home-signing case). Since universal grammar in its technical Chomskyan sense is simply a name for these genetic predispositions, we again have nothing so far as a basis for fruitful controversy (as opposed to politics). What creates the conceptual possibility that the author's 'language-as-use' thesis is coherent and controversial, and hence potentially interesting, is that the genetic predisposition in question is misdescribed as being a (universal) GRAMMAR: another species-specific ability might substitute for it. E thinks that this possibility cannot be true for the 'speech production apparatus' (16), and I assume he would accept the same for speech perception/comprehension. But then there necessarily is a system, let's call it language, that is both externalized in production and internalized in speech perception. The familiar claim of this book is that this system—language—is not part of the species-specific genetic predisposition in question. Instead there is a *DIFFERENT* species-specific capacity, identified as 'cultural' or 'interactional intelligence' (3, 103, 229).

The obvious problem with this proposal, nowhere hinted at in this book, is that the 'cultural/interactional intelligence' hypothesis is an alternative to the 'universal grammar' hypothesis only if the former can be separated from the latter. But if we subtract language from human culture (or human social interaction), little of it would be left. The hypothesis that humans are specially equipped with 'pro-social' or 'communicative' intentions would be promising if there was evidence for the *KIND* of communicative intentions that we express in language, in the absence of language. Yet the proposal that chimpanzees, say, think like us, while tragically prevented from telling us, seems preposterous. It would assimilate the predicament of chimpanzees to that of human individuals with global aphasia, to the extent that these individuals can still seem to have relatively normal human thoughts and human communicative intents. On other views, cognition is fundamentally impaired when language is lost in acquired brain lesions affecting left perisylvian areas (Baldo et al. 2010), and evidence is mounting that communication impairment in neurodevelopmental disorders and language can be strongly linked (Hinzen et al. 2015). The same entanglement between language and allegedly nonlinguistic capacities of social cognition (in their human-specific forms) has long since been observed for the case of 'theory of mind' (e.g. de Villiers 2007, Paynter & Peterson 2010).

It seems pointless to argue about whether the gap between human and nonhuman cognition and communication is wide or narrow (E actually thinks it is wide in the case of nonhuman primates, whose communications even after the most intensive training do not 'come remotely close' to human language: 59, 28, 44), or whether chimps can really 'think'. In whatever way nonhuman forms of cognition or communication approximate human forms, they remain different. As E agrees, reviewing neither (well-known) facts of ape gestural communication, nor of whale song complexity, nor of sequencing abilities in starlings will change anything with regard to the human

uniqueness of language and our cognitive phenotype:¹ species remain as distinct as their communicative and cognitive styles, with referentiality in particular as an important dividing line, as correctly noted, in my view, by E (60). All parties, then, need an explanation for the differences in question. But does calling them ‘cultural intelligence’ help? The term does not identify a well-circumscribed cognitive mechanism and it remains fundamentally underarticulated as compared with half a century of descriptions of our linguistic capacity. The new hypothesis also seems misleading and unnecessary, if formulated as an alternative to an explanation invoking language (e.g. Tattersall 2014), given how entangled a human-specific culture and language are, and human gestural repertoires and language (e.g. Cartmill et al. 2014).

Other nonlinguistic cognitive variables invoked by E—‘pattern-recognition’ and ‘intention-reading’—are equally too vague to make any predictions for what we will find in human linguistic cultures and what we will not. Recognizing intentional behavior is not a human-specific ability, and like ‘cooperation’, it implies essentially nothing for whether a species will also be capable of language (unless we circularly assume that intentions will ipso facto be the kinds of intentions and have the kind of thought contents that they have when expressed in language). The suggestion that concepts, rather than being ‘innate’ or an inherent aspect of a linguistic capacity, ‘derive from the world itself’ or from ‘experience’ (190) is equally empty. Horses, chimps, and hamsters also confront the world and experience things, yet none has our conceptual capacity. Children with neurodevelopmental disorders do too, yet again they fail to develop neurotypical conceptual structures, often in parallel with their language difficulties. ‘Relational knowledge’ (191), too, is scarcely present or absent in nonhumans (Penn et al. 2008) and is potentially language-dependent (e.g. Baldo et al. 2010). E invokes ‘shared experience’ due to the same bodies and world to explain what makes language universal (250–52). Put differently, there is ‘universal thought’ or experience, but not a universal grammar. Due to what, however, is this ‘experience’ the one it is? In humans, it requires novels to describe. Chimps do not have the same experience, but then tragically lack language to write the novels. So our experience is different. But why? The category of ‘shared experience’ taken as such explains nothing: it is the explanandum. Human thought is not confined to recognizing that X CAUSES Y to RECEIVE Z (250), which a chimp perhaps can do, too.

Fodor’s Mentalese hypothesis (189–90) is equally nonexplanatory, as E agrees (189–90), but E omits the detail that he shares this view with Chomsky, who moreover has equally long rejected the functionalist philosophy of mind that E ascribes to the ‘language-as-instinct crowd’. And whoever formulated the poverty-of-the-stimulus argument that E discusses (103–6), it was not Chomsky: it is not that ungrammatical structures like *Is the man who cleaning is bald* are ‘outlandish’ and therefore the child needs to eliminate them. The point is that there is nothing outlandish about them if we subtract the grammatical knowledge that E wants to get rid of. Nothing in ‘pattern recognition’ or ‘intention-recognition’ rules them out. That adults do not use them is also no indication that they are ungrammatical, insofar as many structures that adults have not produced are grammatical.²

What, then, is our mental grammar like? E thinks it is a set of ‘constructions’, which are defined as sound-meaning pairs (243) (he does not say what is not a construction). This claim, however, is definitionally true of any linguistic expression by the lights of the generative tradition, so no basis for controversy. Next, he suggests that constructions can have argument slots in them, as in the schema X CAUSES Y to RECEIVE Z above. In such schemas the variables stand for phrases that can, in the nonidiomatic case, take any number of values belonging to the same type (say, ‘NP’). So phrase structure is assumed, as is required for generativity. Next, constructions, that is, linguistic expressions (sound-meaning pairs), are said to differ in ‘abstractness’. Words

¹ E’s interpretation of the birdsong facts in Ch. 2 is ill-informed, and the facts are discussed in much greater detail by the tradition attacked (e.g. in Berwick et al. 2011). The presentation of considerations from evolutionary biology is generally a disaster in terms of both rhetoric and content (see e.g. Berwick et al. 2013 on Neanderthals).

² What E refers to as ‘the most recent version’ of universal grammar (110) is thirty years old and defended by no one today.

are the most concrete, idioms in the middle, sentences on top. No controversy here either. Finally, a ‘principled distinction’ between words and syntax is denied (247). Now, below is an example of a word, and then an example of a phrase (which is not a word and does not lexicalize), and then an example of a sentence.

- (1) DOG
- (2) the dog
- (3) The dog I saw has now left.

Example 1 as such can only classify an item of our experience: it cannot distinguish between a dog I saw, the dog I like, some dogs I saw this morning, dogs in general, or dog-meat (as in *I ate dog/lamb/beef*). These are distinctions in how a given lexical content word can be USED REFERENTIALLY, and in each case, grammar is required, and indeed a different grammatical configuration (Hinzen & Sheehan 2013:Ch. 4). So there is a principled difference, in both structure and the KIND of meaning involved: *the dog* can be referential in the ways illustrated, but DOG as such cannot be. Example 3 in turn can be true or false, which neither 1 nor 2 can be. Truth goes with grammar, and requires its MAXIMAL COMPLEXITY as used in a context. Neither reference nor truth follows from any nonlinguistic cognitive variable that I know of, and if this is true, eradicating the lexicon-grammar distinction is bad advice: it would deprive us of a likely crucial explanatory variable. Human language and mind could thus not just be interconnected but ‘symbiotic’, in an even stronger sense than E rightly suggests (228).

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Not compelling: Commentary on Evans 2014

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Evans has written a book that contains all of the key ingredients of an introductory textbook on linguistics. It covers most of the subjects one would typically expect, and the reader encounters most of the big names any introductory text in linguistics usually mentions. The difference is that E couches everything in a pungently anti-nativist rhetoric. The empirical evidence presented is systematically taken to imply that tenets formulated by Chomsky and his followers, notably Steven Pinker, are incorrect. According to the author, everything we have learned about language—its structure, evolution, history, variability, acquisition, and representation/processing in the brain—since Chomsky famously proposed that language is an innate faculty of the human mind actually serves to disprove that thesis.

E's alternative to what he calls the language-as-instinct point of view is that language is an outgrowth of cultural intelligence—a specifically human trait—and that all of its properties are rooted in patterns of communicative use. The tone of the book is thus polemical. It is also occasionally tendentious, and the author does not eschew derogatory qualifications of the school of thought he is attacking or its representatives, suggesting that advocates of linguistic nativism are not objective and not sensible. Consider the following quotations:

The findings I've been discussing add up to the body of evidence that any objective person would find compelling. (57)

... does any sensible person really believe that language could plausibly have evolved for anything other than communication? (258)

Even the title, which includes the term 'myth' as a qualification for a scientific theory, reflects the book's polemical stance.

Setting aside these caveats, we can ask: does E have a case? Let me try to answer this by considering the points he makes about primary language acquisition. E does two things: he argues that the nativist account of primary language acquisition is not supported by the evidence, and he puts forward an alternative, usage-based account, arguing that this is adequate and complete. A major theme in E's critique of the universal grammar hypothesis is that the predictions it makes for the process of language acquisition are incorrect. On this count, I can go along with E. The available evidence does not show that acquisition of properties of a native language grammar is instantaneous and uniform across the board, and the trajectories that we see in young children's natural language development do not establish that a mechanism such as parameter setting is psychologically real. It is important, however, to remember that the original ideas about language acquisition in the Chomskyan framework were motivated by formal considerations; the acquisition of a grammar was approached as a purely computational problem. Psycholinguists subsequently took the computational theory as an idealized version of a theory at the level of implementation and tested its predictions. In fact, much of the evidence E considers to contradict the nativist approach results from empirical work on child language development propelled by the very proposals made by Chomsky and his followers. Science progresses.