Language as ergonomic perfection

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Abstract: Christiansen & Chater (C&C) have taken the interactionist approach to linguistic universals to an extreme, adopting the metaphor of language as an organism. This metaphor adds no insights to five decades of analyzing language universals as the result of interaction of linguistically unique and general cognitive systems. This metaphor is also based on an outmoded view of classical Darwinian evolution and has no clear basis in biology or cognition.

For the last five decades, much linguistic research has adopted an interactionist position about the sources for Universal Grammar (UG). This approach partitions overt language universals into those determined by inborn linguistic structures and those shaped by language performance systems (e.g., learning and general cognitive factors). For example,

> Many an aspect of adult . . . linguistic structure is itself partially determined by the learning and behavioral processes that are involved in acquiring and implementing that structure . . . some formally possible structures will never appear in any language because no child can use [or learn] them. (Bever 1970, pp. 279–80)

Christiansen & Chater (C&C) adopt an extreme version of the interactionist approach, eliminating the contribution of linguistically unique genetic bases for language in the brain. Their designated metaphor is language as an organism that has evolved to fit the human brain: "Language is easy for us to learn and use,

not because our brains embody knowledge of language, but because language has adapted to our brains" (target article, sect. 1, para. 3).

Yet, mentioning "the brain" does nothing to enrich or support their claims. Unlike, for example, Deacon (1997), C&C say nothing about *which* properties of the human brain are such that language has adapted to *them*. C&C restate much that has been said before about the influence of behavioral systems, while claiming – without demonstration – that they exhaust the constraints on linguistic universals. The critical claim is that *the individual language learner/user* filters possible languages via general cognitive, communicative, and physiological processes. This is tantamount to the idea that language is a tool, ergonomically shaped to be maximally usable, no different in principle from a Boeing 707 or recipes for Salzbuger Nockerl. Yet no one would reasonably propose that airplanes or recipes are *organisms that evolved* to fit the human brain. C&C suggest that it is the alleged *unconscious* emergence of possible languages that makes language unique from other tools. Perhaps so, but they do not offer any theory of what the brain has to do with this that makes it reasonable to apply the organismic evolutionary metaphor only to language.

Characterizing language as evolving on its own also contributes unnecessary confusion because C&C restrict their notions of evolution to a classic model. For example, contrary to their interpretation, the adaptive complexity of biological organisms only *marginally* arises from random genetic variation, winnowed by natural selection. The many non-selective processes they cite (genetic drift, genetic hitchhiking, epigenetics, etc.), and others (e.g., evolutionary capacitors [Rutherford & Lindquist 1998] and alternative splicing [Blencowe 2006]), suggest that *no* complex organism, never mind language, evolved via selective winnowing of variants in the orthodox Darwinian model. Few biologists today adopt that pristine model, while some go as far as stating that *"natural selection is just one, and maybe not even the most fundamental, source of biological order"* (Gibson 2005; see also Carroll 2001; Sherman 2007; Wagner 2005).

After ignoring many lessons from current models of evolutionary processes, C&C proclaim it "astronomically unlikely" (sect. 4, para. 4) that *non*-adaptationist processes may have produced genuine evolutionary novelties. But, there are many "unlikely" cases in which non-selectionist mechanisms have been pivotal: the evolution of the genetic determination of sex (Quinn et al. 2007), the development of the eye (Sherman 2007), and the adaptive immune system (Hiom et al. 1998). Appraised "likelihood," of course, depends on the probability baseline that one adopts. Given a specifiable genetic configuration, combined with specifiable external and internal factors, the probability is close to one. If the assumed probability baseline is a random swirl of molecules (or a tornado in a junkyard, as Hoyle [1983, pp. 18–19] and Pinker [1997] famously suggest), then indeed the probability of evolving a camera eye, or Universal Grammar – or a Boeing 707 – is vanishingly small. But why should one take anything like that as a relevant baseline?

C&C note further that, "Small genetic changes lead to modifications of existing complex systems … they do not lead to the construction of a new complexity" (sect. 4, para. 5). This statement is false when master genes (homeoboxes) are involved (see, e.g., Ronshaugen et al. 2002; Sherman 2007).

In brief, insofar as one takes seriously "language evolving" as a meaningful metaphor, C&C's archaic view of biological evolution would render their arguments unlikely at best, and, most likely, wrong.

Secondly, the properties of cognition that C&C invoke to explain language occur in other species, including those that are less clearly present (e.g., hierarchical processing; Conway & Christiansen 2001). Why did language not "evolve" in the service of these species? Humans must have innate equipment quite different from those species that also display their version of "general" human traits.

For "language as organism" to evolve under selective pressures enforced by the brain requires a suitable starting point: C&C suggest that a "proto-language" (sect. 7.1, para. 1) could fill the void. They reference computational models of the emergence of compositionality from a proto-language (cf. Smith et al. 2003b), but ignore a critical feature of these models – the agents are equipped with a specific compositional mechanism for grammar induction. Thus, at a bare minimum, the brain must have contributed proto-language and such structural inductive systems as hierarchical processing, and recursion – abilities vastly undeveloped in primates (Conway & Christiansen 2001).

Furthermore, C&C still face the problem of innovation from "proto-langauge." What triggered the first truly structured sentence? The first recursive utterance introduced a transcendentally ungrammatical form. It is mysterious how this structure would ever be produced in the first place, let alone persist in the linguistic pool, if not for some innate capacity. At the very least, a possible pathway for the transition between unidentified cognitive mechanisms is needed, but C&C make no attempt to explicate one.

Finally, C&C report evidence of Broca's area involvement in an artificial grammar task and normal natural language processing (Petersson et al 2004) but omit drastically opposite data (Friederici et al. 2006; Musso et al. 2003).

On language and evolution: Why neo-adaptationism fails

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Abstract: I identify a number of problematic aspects of Christiansen & Chater's (C&C's) contribution. These include their suggestion that subjacency and binding reflect non-domain-specific mechanisms; that proto-language is a "cultural product"; and that non-adaptationism requires overly rich innate structures, and is incompatible with acceptable evolutionary processes. It shows that a fully UG (Universal Grammar)-free version of the authors' neo-adaptationism would be incoherent.

For all linguists who do not view language as a Platonic object, it should be uncontroversial that there is a "fit" between language and the brain (as expressed in Chomsky [1986], where Chomsky introduces his notion of I-language). The common goal is to find out how they fit, and to what extent our cognitive system is dedicated to language.

Christiansen & Chater (C&C) argue that no part is dedicated to language. In brief, that there is no Universal Grammar (UG). This is a strong position that deserves extensive research, taking into account the properties we know language possesses (the derivation of condition B, in Reuland 2005b, illustrates what this involves). How well, then, do the authors succeed?

C&C focus on what they call "abstract" properties (see sect. 2) and discuss two such properties to establish initial plausibility. One is subjacency (Chomsky 1981). C&C argue that subjacency is not specific to language, but derivable from general processing constraints (sect. 6.3) or, possibly, "cognitive limitations on sequential learning" (sect. 7.2), referring to Berwick and Weinberg (1984) and Ellefson and Christiansen (2000) in this regard. Subjacency effects are real. But we know by now that restrictions on extraction result from a variety of factors (see Szabolcsi [2005] for a state-of-the-art overview; and see, e.g., Khomitsevich [2008] for cross-linguistic variation in island effects). Subjacency is not a primitive. The nature of the resources for language processing is still open, and more may be involved