emerging sign languages, teaches us that our species relies on the human body in the creation and structuring of languages when it can.

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Adaptation to moving targets: Culture/gene coevolution, not either/or

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Abstract: We agree that much of language evolution is likely to be adaptation of languages to properties of the brain. However, the attempt to rule out the existence of language-specific adaptations a priori is misguided. In particular, the claim that adaptation to "moving targets" cannot occur is false. Instead, the details of gene-culture coevolution in language are an empirical matter.

We wholeheartedly agree with Christiansen & Chater's (C&C's) central point that much of language evolution is likely to be adaptation of languages, via cultural evolution, to leverage species-typical properties of the brain. They are right to point out that while many language universals have been taken to reflect properties of a genetically evolved Universal Grammar (UG), they could equally well be universal culturally evolved properties of languages. This possibility is insufficiently recognized and should be a major focus of study.

What we take issue with, however, are the ideas that this is an "either/or" issue and that it is possible to rule out a priori the existence of mechanisms that evolved specifically as a result of their effects on language acquisition. Instead, how genes and culture interact to produce language, and whether domain-specific mechanisms exist for language acquisition, are empirical matters.

The biggest problem with C&C's argument lies in their attempt to rule out the existence of UG a priori by proposing a "logical" problem of language evolution: namely, that adaptation to "moving targets" is impossible. Unfortunately, this claim is simply false. "Moving targets," in the sense of environments that vary over space and time, are the norm rather than the exception in biology. What matters for adaptation is not whether environments vary, but whether there are statistical regularities that natural selection could track. It is very likely that the world's languages do possess statistical regularities, some of which C&C point to.

In fact, it is not even necessary for the statistical regularities of the environment to be stable over time in order for adaptation to occur. One of the earliest ideas about this was Van Valen's (1973) "Red Queen" theory of antagonistic coevolution, in which different species, such as predator and prey, could be locked in ever-spiraling evolutionary arms races in which neither settle on a stable equilibrium. Nevertheless, these species can still evolve adaptations to the coevolving traits of the other, such as adaptations for fast running. Such coevolution is possible even when one party in the race can evolve "faster" than the other, as in host-parasite coevolution. This does not make impossible the existence of anti-parasite adaptations, such as the immune system (which C&C mention).

Perhaps the clearest and most relevant demonstration that genetic adaptation to culturally moving targets is possible can be found in the work of culture-gene coevolution theorists (Boyd & Richerson 1985; Richerson & Boyd 2005). The logic of these models is precisely that delineated by C&C: namely, that cultures vary substantially in time and space, such that precise genetic tracking of every cultural detail is impossible. Nevertheless, gene-culture coevolution models show that natural selection can favor genetically evolved mechanisms that facilitate the acquisition of local culture. Importantly, it is the *statistical* properties of cultural information that the genes track.

These models and associated empirical findings (Richerson & Boyd 2005) show that culture and genes can and do coevolve. Although it is certainly possible to make models in which genes are "locked in place" and culture evolves to them, or vice versa, it is very unlikely that this is empirically what happens. Culture exerts selection pressure on genes, and genes exert selection pressure on culture, simultaneously. This means that questions like "which came first, the genes or the culture?" are inherently problematic. The "circularity trap" of C&C (sect. 3.2.1) is a problem faced by people who think in an either/or way; it is not a problem faced by the evolutionary process itself.

We suggest that the proper way to think about the gene pool of our species – and about the pool of cultural phenomena such as language – are as statistical clouds spread across space and time, each adapting to the other. C&C are entirely right that the properties of languages adapt to the statistical properties of the mind to make it more learnable and more easily understood. But there is, contrary to C&C's claims, no a priori reason why genes that do the exact same thing would not also be selected for. Indeed, if such genes existed, they would inevitably be selectively favored. It is a mistake to think either that language leaps fully formed upon the stage, and genes evolve to it, or that a genetic apparatus for language evolves, and then language sprouts from it. The process is likely to have been coevolutionary, all the way up.

What we expect, on this view, is a mesh, or fit, between genetically evolved mental mechanisms and language. The kinds of mechanisms we expect are ones that fit well with the statistical properties of language, and the statistical properties of language should fit well with them; how much of this fit has evolved on either side is an empirical matter. Language acquisition mechanisms can be seen as "prepared learning" mechanisms that reduce the frame problems inherent in any kind of learning by expecting certain kinds of regularities, or statistically present properties, to exist in the local language. These might include properties like long-distance dependencies; lexical types such as nouns and verbs; word order as a disambiguating device; hierarchical structuring; mechanisms for marking conceptual features such as space, time, causation, agency, and mental states; and more.

We recognize that many of these features might be argued to emerge from interaction with mental mechanisms that are not language-specific, such as conceptual mechanisms (although the language/conceptual interface could be a language-specific adaptation). Moreover, C&C and others (including, perhaps, Chomsky) might argue that these features should not be regarded as part of "UG" because they are not "arbitrary" or "non-functional." However, we do not find it particularly useful to restrict UG to only "non-functional" features of language; among other things, it seems an odd way to carve up evolved structures. The important questions, for us, are twofold: (1) Do mechanisms exist that evolved because of their beneficial fitness effects on language acquisition? and (2) what are the computational properties of these mechanisms? We are happy to call these "UG," though they might end up being very different from what Chomsky proposed. While we applaud C&C's efforts to draw attention to the culturally evolved properties of language that play a role in this evolutionary process, the authors have not convinced us that we should stop trying to look for the answers to questions (1) and (2).

Languages as evolving organisms – *The* solution to the logical problem of language evolution?

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Abstract: Christiansen & Chater (C&C) argue persuasively that Universal Grammar (UG) could not have arisen through evolutionary processes. I provide additional suggestions to strengthen the argument against UG evolution. Further, I suggest that C&C's solution to the logical problem of language evolution faces several problems. Widening the focus to mechanisms of general cognition and inclusion of animal communication research might overcome these problems.

Christiansen & Chater's (C&C's) arguments against sudden de novo evolution of a highly complex domain-specific structure (Chomskian Universal Grammar or UG) could be strengthened by the following suggestions. C&C underemphasize the fact that evolutionary change (e.g., mutations) occurs at the level of individual organisms. Therefore, we need to deal with more than the problem of the extremely low likelihood of non-adaptationist "arrival" of UG. In order to spread through the population, this language acquisition device (which would require language as input) would need to be passed on to the next generations; and it is difficult to imagine what selective advantage would occur. While a hypothetical lone organism who somehow macro-mutated an eye would have an advantage over her eyeless peers, a lone UG would be of questionable use in a community of non-linguistic creatures.[1] Furthermore, evolution never plans ahead towards some future goal. Evolving a large, metabolically expensive brain because it could be "tweaked" into future UG (as proposed by Chomsky 1986; 2002) is almost certainly out of the question. Because evolutionary processes are restricted by existing structures and recruit not infrequently structures that originally evolved for some other purpose (excaptations; cf., Gould & Vrba 1982), we would expect UG to be similar to and not substantially different from other biological structures. These considerations strengthen C&C's arguments against the sudden occurrence of a monolithic Universal Grammar module (e.g., the "instantaneous Great Leap" defended by Chomsky as recently as 2006).

Concerning the problem of an adaptationist account of UG evolution, I suggest that C&C's arguments demonstrate convincingly why a monolithic UG with fundamentally arbitrarily (as opposed to functionally) determined constraints could not have evolved through natural selection. However, C&C have not shown that "specialized brain mechanisms specific to language acquisition" (sect. 1, para. 2) necessarily need to have the structure of UG. The exact nature of UG is controversial even among its proponents (e.g., Chomsky 1995; 2005b; 2006; Crain & Pietroski 2001; Fitch et al. 2005; Hauser et al. 2002; Jackendoff 2002; 2007; Lightfoot 1999) and critics point at the inadequacy of UG definitions (e.g., Cowie 1999; Deacon 1997; Pullum & Scholz 2002; Tomasello 2003). C&C repeatedly stress that their account is compatible with language-specific brain mechanisms that differ from UG, and it would be desirable to specify these mechanisms in some detail.

Having shown that a genetically determined brain structure like UG could not have evolved, C&C propose to shift the focus from brains to language itself. On their view (similar to proposals by Atkinson et al. 2008; Bichakjian 2002; Clark 1996; Deacon 1997; Kortlandt 2003; Ritt 2004; van Driem 2005), languages can be understood analogous to organisms that are shaped by brains and have evolved "to be easy to learn to produce and understand" (sect. 1, para. 3). This elegant solution to the logical problem of language evolution is not entirely unproblematic, and the following issues need to be addressed.

C&C claim that selection pressures working on language to adapt to humans are significantly stronger than pressures in the other direction because language can only survive if it is learnable, whereas humans can survive without language (sect. 5, para. 3). Survival is undoubtedly a necessary condition for natural selection, but it is not sufficient. Organisms need to reproduce to pass their traits on to the next generation. By analogy, a male peacock presumably can survive without a tail, but would he be able to attract a mate and reproduce? If language plays a role in mate selection (Burling 2005; Deacon 1997; Dunbar 2005; Franks & Rigby 2005; Johansson 2005; Miller 2000), then the selective pressures for language skills may be stronger than C&C allow.

C&C's suggestion that learnability is a constraint on languages sounds compelling for our modern, highly complex languages. But, assuming that the first (proto) languages were much simpler, the question arises why language did not remain much simpler. Further, as the example of language-specific performance differences in young Turkish, English, Italian, and Serbo-Croatian children (sect. 7.3, para. 6) shows, we find considerable differences in ease of learning and processing among existing languages. C&C's claim that languages are passed on as collection of interdependent features (sect. 5.2, para. 1) might explain why these differences continue to persist. But why and how did learnability differences arise in the first place? C&C suggest that "individual constructions consisting of words or combinations thereof are among the basic units of selection" (sect. 5.2, para. 2). Again, this already presupposes a fairly sophisticated language to be in place and sidesteps the important question of how more fundamental components of language evolved.

Finally, the intriguing computer simulations do not necessarily show that the algorithms used are not language-specific. It seems that in the examples cited, the problem space is limited by the initial conditions of the simulation; and therefore the results are compatible with UG (Bickerton 2003; Marcus 2001; Russell 2004). Further, the models seem to be restricted to some prespecified aspect of language, and it is not clear whether a complete language would require some domain-specific scaffolding (Marcus 1998; Russell 2004; Yang 2004).

Possibly some of these concerns can be addressed when we consider language evolution in the broader context of the evolution of other cognitive capacities. One interesting line of thought is the proposal that language comprehension, rather than production, was the driving force behind language evolution (Bickerton, 2003; Burling 2000; 2005; Origgi & Sperber 2000). Burling (2005) suggests that communication does not begin with a meaningful vocalization or gesture but with the interpretation of the behavior of another individual. An individual who can understand another's action even when no communication has been attempted gains an evolutionary significant advantage. This suggestion might direct us back at brain structures. Which structures underlie cognition in general and language specifically? It appears plausible that language is handled by several subsystems, which presumably also have non-linguistic functions (Deacon 1997; Fitch et al. 2005; Johansson 2005). It might be illuminating to include details emerging from research on animal communication (e.g., Arnold & Zuberbühler 2006; Doupe & Kuhl 1999; Fitch 2005; Gentner et al. 2006; Hauser et al. 2001; Orlov et al. 2000; Pepperberg 2000; Perruchet &