

Evans's (2014) modularity myths: A mental architecture digest

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Evans's book has certainly gotten the attention of linguists, and it has done so for two good reasons. First, E reminds us that linguistics is first and foremost a cognitive science, so one's account of language must be firmly grounded in a theory of mental architecture. Second, E insists that linguistic theory ought to be subject to careful empirical scrutiny from a multidisciplinary perspective. These reminders are certainly useful, and E should be commended for reigniting the discussion of these core theoretical and methodological issues. It is unfortunate, however, that *The language myth* does not abide by the same standards.

At the center of this book is a fierce critique of the modularity of mind—the hypothesis that the mind is innately equipped with specialized domain-specific faculties. Being one of those putative faculties, language offers a test case for the modularity thesis as a whole. E believes modularity is dead wrong—it rests on an implausible account of neurobiology, is fraught with logical fallacies, and is supported by no empirical evidence. Regrettably, E's own grasp of the theory is seriously flawed.

Considering first the argument from neurobiology, E contends that innately specified modules are precluded by the brain's plasticity, on the one hand, and the shortage of genes necessary to fully hardwire its synaptic connectivity, on the other. Indeed, an inspection of the brain language system reveals no brain region that is 'dedicated to language to the exclusion of all else' (142). Given that those presumed biological prerequisites for a language module are untenable, the thesis of innate modules is a priori doomed.

E is of course right about brain plasticity and the absence of 'language genes' or 'language-only hubs', but these observations are hardly new, and their relevance to the modularity thesis is tenuous. Reviewing similar concerns raised by Martin Gardner in 1952, Jerry Fodor (1983:98) urges us to

[r]emember that faculties are, in the first instance, functionally rather than physiologically individuated. And perhaps *localization* isn't precisely the notion that Gardner wants, since, after all, there might be neural specificity of some functions that aren't localized in the sense of being associated with large, morphologically characterizable brain regions.

E does not acknowledge (let alone heed) Fodor's warning and the many subsequent discussions in the literature. Brain plasticity—E's other strike against modularity—is likewise a questionable challenge, as plasticity is hardly evidence for an unconstrained equipotentiality of the brain. Finally, E's 'gene shortage' worries are based on unfounded assumptions about the genetic encoding of grammatical rules (Marcus 2004). These observations do not imply that evidence from neurobiology is irrelevant, and they certainly do not demonstrate that the modularity thesis is improbable or unfalsifiable. Rather, they remind us that the architecture of the mind cannot be reduced to the neural and genetic levels (Fodor & Pylyshyn 1988). Accordingly, modules can only be defined at the functional cognitive level.

E, to be fair, does consider in passing the functional definition of modules, but he rejects it outright on logical grounds. Once again, however, his objections are rooted in a misconception. At stake is the question of how a module selects its input. A module (in the Fodorian sense; Fodor 1983) is both 'eccentric' with respect to the inputs it admits and encapsulated from external sources. Computations within a putative 'face module', for instance, are immutable by one's beliefs and goals with respect to a given face, as those sources of information form part of central 'horizontal' systems to which the module has no access. E wrongly believes that these two properties—the selectivity of modules with respect to their inputs and their encapsulation from horizontal systems—are mutually incompatible. He worries that a 'face module', for instance, cannot sort out faces from, say, vases, so one must invoke central intelligence ('big brother', in E's

words) to direct the ‘perceptual traffic’ into the appropriate module, and this, of course, violates the encapsulation tenet. But this entire concern is misplaced. Fodorian modules are entrusted with their own ‘traffic policing’, and this is possible because they are strictly constrained with respect to (a) the range of information they can access, and (b) the range of computations they can effect. A face module can autonomously select its inputs by virtue of the innate constraints on the structure of human faces. Similarly, sentences would be recognizable as such by virtue of their conformity to the module’s innate internal theory of sentence structure (i.e. universal grammar). And all of these module-internal constraints are entirely in line with the encapsulation assumptions. Now, whether the encapsulation hypothesis turns out to be right remains controversial (e.g. Firestone & Scholl 2015), and the answer hinges on a careful dissociation of true perturbations of a module’s internal operation (‘top-down effect’) from ones occurring outside the module—a matter of considerable conceptual and empirical complexity. E’s cursory reference to the McGurk effect hardly settles these questions. In any case, encapsulation is not universally assumed by all proponents of ‘modularity’, since this hypothesis has acquired multiple versions (distinct from the Fodorian module), which E does not differentiate (e.g. Chomsky 1980, Pinker 1994, Carey & Spelke 1996). While these views differ on their processing assumptions, common to them all is the proposal that the mind is innately equipped with distinct universal-knowledge domains.

E believes that the evidence for innate knowledge is exceedingly rare. Innateness, in his view, is further countered by children’s sensitivity to (linguistic) experience and the incremental nature of learning. But the obvious fact that some aspects of knowledge are learned does not negate the existence of others that are innate. Furthermore, innate traits are routinely triggered by some input (internal or external to the organism; Ridley 2003), and this does not amount to (cognitive) learning. The critical question, then, is not whether innate traits are experience-independent. Rather, it is whether some representational primitives and constraints are acquired in the absence of (cognitive) learning (i.e. are innate; Samuels 2004). Contrary to E’s claims, the experimental support for innate specialized knowledge (also known as ‘core knowledge systems’) is hardly scarce—there is a very active experimental research program on infants’ instinctive understanding of number, object, space, and theory of mind, to name but a few examples, many of which have precursors in nonhuman species (for review, see Carey 2009).

Universal grammar (UG) presents a critical argument for the innateness of the language module. E dismisses the UG hypothesis as intellectual dogmatism, based on absence of evidence, but his own analysis falls into the same trap. E’s cursory mention of a handful of empirical challenges to the UG hypothesis does not allow readers to appreciate the true state of the art on any of these topics or, more significantly, to grasp their theoretical complexity. And indeed, like language itself, language universals are the product of multiple sources, ranging from putative universal grammatical constraints to sensorimotor restrictions and historical forces. To evaluate the evidence—be it typological or experimental—one must articulate a theory of UG and devise a strategy to dissociate its putative effects from nongrammatical sources. E’s analysis fails on both counts. Concerning theory, E claims that implicational typological universals are inconsistent with UG, but neglects to note that some grammatical theories (e.g. optimality theory; Prince & Smolensky 2004 [1993]) are explicitly designed to capture such facts. There is also a large relevant experimental literature that E entirely ignores. To cite one example from phonology, speakers are demonstrably sensitive to putatively universal grammatical constraints on syllable structure for which they apparently lack evidence (for review, see Berent 2013); their precursors are evident at birth (Gómez et al. 2014), and they are dissociable from sensory and motor constraints (Berent et al. 2015). While the experimental dissociation of putatively UG constraints from nongrammatical sources is never trivial, such questions are certainly amenable to experimental scrutiny—efforts that E entirely ignores.

To summarize, E’s discussion of modularity and innateness leaves much to be desired, but one should welcome his call to ground linguistics in a deeper understanding of cognitive architecture. Cognitive architecture, however, and the modularity of language, specifically, are functional hypotheses that are irreducible to brain localization. As a cognitive science, linguistics is thus at a special vantage point to unveil the functional design of the human mind and brain.

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Myths, magic, and poorly drawn battle lines: Commentary on Evans 2014

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Evans’s *The language myth: Why language is not an instinct* is a not-so-subtle attempt to counterweigh Steven Pinker’s (1994) *The language instinct*. For more than two decades now, opponents of Pinker’s view of language have lamented the fact that introductory students (and nonspecialists in general) do not have access to a well-written, accessible, yet opposing, view of how language works. Enter *The language myth (TLM)*, which reads like a manifesto out of the School of Cognitive Linguistics. In that respect, the book is a welcome addition to the fray, since there is, after all, significant room for discussion on just about every major point within the nativist program (dubbed, by E, the ‘language-as-instinct hypothesis’).

The basic premise of the book is that the language-as-instinct hypothesis consists of several related myths, which, when combined, provide the veneer of a believable theory. E deconstructs the overall hypothesis into six myths and attempts to debunk each of them individually. The manner in which this is organized and presented is both superbly readable and, unfortunately, fatally flawed. While it sounds like a good idea to present succinct ‘myths’, followed by a methodical take-down of each myth, this need for succinctness and ease of reading means that E is forced to