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**Language acquisition and the ontogeny of domain-specific cognition.
Conceptual review and insights from English data.**

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REVIEW

Through the lens of language acquisition, the present work takes on the long-standing core controversy in linguistic theorizing, which is centered on the relationship between language and our general cognitive faculties: is language innate, or does it emerge as a specific manifestation of our cognitive endowment? And in any case, what does it actually mean to claim that language is innate, or that language has a biological basis? The author summarizes this complex and multi-layered question by framing it as an issue of two fundamentally different theoretical approaches: the generativist view of language as a domain-specific skill facilitated by a dedicated brain function that separates humans from other species vs. the emergentist view, in which linguistic skills are argued to draw on domain-general cognitive resources and do not require a dedicated brain structure.

The volume consists of six chapters, five of which are devoted to specific components of the core question: after a theoretical set-up in the introductory chapter, the author provides critical review of the theoretical claims and empirical evidence from four distinct perspectives – language acquisition, language pathology, the relationship between language, music, and math, and, finally, an evolutionary argument. The book's final chapter presents a synthesis that leads to the author's own proposal about how to interpret the notion of innateness. The heart of his argument is firmly anchored in evolutionary logic, which leads him to conclude that our language faculty (including Chomsky's so-called FLN) had to emerge from brain circuits that were originally devoted to other cognitive tasks but through connectivity and neuronal recycling could adapt to additional, more specialized tasks, including language.

I read the manuscript from the perspective of a cognitive linguist with interest in linguistic theorizing and my review thus reflects this background. The subject matter is extremely complex but the reader is led through all the different layers in a carefully structured and easy-to-follow manner so that the text easily speaks also to linguists beyond the acquisitional or psycholinguistic specialization. The author tries, for the most part successfully, to take an unbiased view of the two positions vis-à-vis empirical evidence, and the text thus reads as a refreshing and illuminating attempt to disentangle the theoretical positions, naturally focusing primarily on the viability of domain-specificity. All of this requires careful work with the terminology, which alone is not a trivial matter since there are theoretical, conceptual, and observational ambiguities hidden in the existing scholarship, and much of it is also obscured by tangled and confusing terminology based on various hidden assumptions. (The author occasionally also falls victim to these ambiguities, see below.)

The overall contribution of the manuscript is at least three-fold, besides providing a lot of food for thought and articulating important questions for further research. (i) The analysis and discussion concerning *bilingualism* (or multilingualism) highlights the fact that this



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phenomenon is a lot more complex than traditionally thought and requires a more sophisticated, multilayered approach. In addition to issues of brain plasticity and cognitive development, the author shows convincingly that other factors and their mutual interaction must be taken into account as well: e.g. social settings, motivation, amount of exposure, age of L2 onset (simultaneous vs. sequential - early, late). (ii) Attention to the development of *reading* skills and to reading disorders is a newly added set of findings and arguments that bear directly on the theoretical debate. The evidence presented by the author provides very important support for appealing to the concept of connectivity as an alternative explanation, since reading clearly does not have a biological foundation. (iii) In close critical reflection, the author does an admirable job in bringing together a vast amount of studies, each dealing with just one small part of the whole picture and often presenting (seemingly) contradictory results. Based on massive evidence and thoughtful argumentation, the author essentially reframes the theoretical debate by concluding that persuasive evidence for domain-specificity concerning children's language skills is lacking, nor is there actually any need, for explanatory purposes, to assume the existence of a dedicated brain structure. Instead, both phonological skills, which appear very early on, and syntactic skills, which emerge later, can be accounted for by appealing to our general cognitive faculties, such as processing perceptual information, ability to calculate probabilities, ability to recognize contrasts, etc.

Since the topic is loaded with theoretical biases, one could easily quibble with various partial points in which the author seems to accept too readily the domain-specific bias in formulating his own commentary in cases, in which the domain-specificity claims can easily be interpreted as less conclusive, because they are based on linguistically simplistic design (e.g. the syntactic experiment discussed on p. 57) or assumptions (e.g. the case of the polyglot speaker). But more jarring are certain inaccuracies that betray insufficient awareness of relevant linguistic research and that will require fixing, minimally by making less categorical statements, e.g.:

- The claim that languages include just “very few rare cases” of non-symbolic signs (p. 95), reduced to onomatopoeic expressions, is patently not true; there's lots of published research on iconicity in language, starting with John Haiman's seminal work in 1980s on iconicity in syntactic structure and morphology. Why is it important to (over)emphasize the symbolic nature of linguistic patterning?
- The idea that “languages tend to reduce their complexity over time” (p. 127) touches on a very difficult topic which should not be taken lightly. To begin with, one should provide a definition of complexity in relation to language (a very problematic notion). And even if applied only to syntax, English syntax actually became more complex over time, as a consequence of losing its morphology. There seems to be some confusion here between ‘simplicity’ and ‘predictability’, which are two different things.
- It is hard to see how the Mundurucu experiment (p. 92) proves anything about what human brain allows access to. One should be less ready to accept claims of the sort offered here because the experimenters' conclusion smacks of a pretty outdated application of the Sapir-Whorf hypothesis. The argument that Mundurucu speakers



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cannot solve math problems accurately because they're "deprived of a language that can accurately describe quantities" is exactly backwards: they did not develop lexical labels for quantities (hence also corresponding practice with math operations) above 5 because they don't need them in their everyday lives. This is an issue of culture, not language or math or natural cognitive capacity.

- "animals do not appear to combine signs" (p. 104) – is this really true? Research on life-long learning of bird songs would suggest otherwise. Perhaps the issue may not be to teach animals human syntax but to observe communication systems of animals that vocalize in non-random and creative ways. The statement clearly needs unpacking.

In addition, the author's exposition suggests areas for further elaboration concerning specific points connected to the central research question in a non-trivial way:

1. I find some of the reasoning in Ch. 3 contradictory and it seems to come from the fundamental conceptual and terminological sloppiness which has plagued the whole field from the beginning and is inherent in the generativist habit of equating language with syntax (not the author's fault). If the basic issue is formulated as 'innateness of language', we have to know from the outset what is meant by 'language'. Does it include, for example, semantics? It seems that it does not - various authors seem to be testing just syntax, although often referring to it as 'language'. But then why is "semantic compositionality" as a relevant notion assessed as something that undermines the similarity of language to music (p. 86), when the issue is structure-building? Isn't it one of Chomsky's fundamental claims that syntax and semantics are independent of each other, to such a degree that it is possible to have structurally well-formed sentences that are devoid of interpretable meaning (cf. the famous example *Colorless green ideas sleep furiously*)? So, semantic compositionality, by that very reasoning, indicates nothing about syntactic structure-building and, hence, cannot be taken as a counterargument to, or evidence for, the exclusivity of linguistic structure as compared to musical structure. And then how should we understand the formulation "our brain sees language as a series of mathematical formulas" (p. 91)? Is this really 'language', or just syntax/structure? And if the latter, why should we worry about semantic compositionality as an intruder, so to speak, to the structure-based relationship between language and music?

2. The observation that babies show sensitivity to lip and facial shapes and movement (p. 36) is intriguing. And shouldn't this be taken as an indication that language is a multimodal phenomenon from early on? I.e., not just sound system, grammar and vocabulary, but also gesture as providing interlocutors with interpretive cues, thus possibly also aiding in language discrimination in bilingual children? Such a hypothesis is of course excluded from the generativist view, given the 'syntactic' definition of language (back to Chomsky's FLN), but that doesn't make it irrelevant, and it could be tested as yet another source of information children (and adults) draw on in learning and forming linguistic categories.



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3. A general theoretical issue: English has a clearly syntax-centered grammatical system; this has motivated the overwhelming attention paid to syntactic skills as a fundamental feature within the domain-specific theoretical proposals. But there are many more languages that have a relatively simple syntax by comparison and instead mark grammatical information primarily or exclusively not by function words (like English) but morphologically, including very specific and complex systems (e.g. Semitic, among others). What predictions are made about the “innateness” of morphological skills? What does “merge” and hierarchy mean in such contexts? Is there any research addressing such questions, whether in language acquisition or pathology?

It should be stressed, though, that none of this detracts from the indisputable contribution of this work, nor does it contradict the author’s general conclusions – those are valid, the reframing clearly advances our understanding in a number of relevant areas and on the whole, provides substantive new directions for further research. It’s just that the linguistic arguments can (and should) be tighter in places.

As for the formal qualities of the manuscript, there’s very little to criticize. The text is written in sophisticated, nicely flowing English (in spite of some minor lexical errors or awkwardness here and there), and the exposition is very lucid, although sometimes slipping to somewhat greater informality of style (e.g. p. 51) than would be expected in an academic genre. In the interest of greater reader friendliness, specific language examples in places would have been helpful, e.g. in describing the three conditions in the experiment and its results of Example 1 (p. 43ff.), or in illustrating the math formulas priming syntactic ambiguity resolution (p. 93). Finally, some footnotes (e.g. 22, 23, 25, 29) would be better placed in the main text as they either complement, elaborate on, or reinforce the discussion at hand in very meaningful and important ways.

All in all, in spite of my critical remarks, I conclude that the manuscript amply fulfills the requirements associated with a Habilitation work and I recommend it for consideration in further advancement proceedings.

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