

Why don't L2 learners end up with uniform and perfect linguistic competence?

(Open Peer Commentary on Epstein, Flynn, & Martohardjono "Second language acquisition: Theoretical and experimental issues in contemporary research", *Behavioral and Brain Sciences*)

Ping Li

Department of Psychology
Chinese University of Hong Kong
Shatin, NT, Hong Kong
ping@psy.cuhk.hk

Abstract

Children in a given linguistic environment all uniformly acquire their target language, but adult learners of L2 do not. UG provides an alternative to account for children's uniform linguistic behavior, but it cannot serve a similar role in accounting for adult learners' linguistic behavior. I argue that EFM's study does not answer the question of why L2 learners end up with non-uniform and imperfect linguistic competence in learning a second language.

Epstein, Flynn, & Martohardjono (EFM) propose that UG is available to L2 learners wholly, in all of its power, and in the same way as it is available to L1 learners. This proposal is thought-provoking, but as it currently stands in the paper, I see major gaps between theoretical exposition and empirical evidence, and between UG principles and L2 learners' linguistic behavior. EFM undertake a lengthy analysis of the alternative hypotheses and subsequently reject them, but provide little solid empirical grounds to support their own position. Their strong argument leaves many fundamental issues unanswered, two of which I will focus on here: (1) the differences between child L1 and adult L2 acquisition, and (2) individual differences among adult L2 learners.

My first question is that if adults can indeed "grow" a language with the right UG parameters as children, why don't they all end up with the same linguistic competence in L2 as children in L1? In L1, UG serves as a solution to the "logical problem of language acquisition", namely, limited and "degenerate" input from the linguistic environment can trigger parameter setting for a particular language, resulting in a productive grammatical system. Here, UG principles are not explicitly taught to children who learn their L1 by natural exposure to the language.

Children, given a linguistic environment for a limited number of years, all learn their target language (L1, L2, or L3) without much trouble. Apparently, motivating a similar account for adult L2 is difficult, because (a) adult L2 learners may receive rich and systematic input through formal instruction; (b) unlike children, adult learners approach the L2 task at different ages, with different linguistic backgrounds, and using different learning methods; (c) most adult learners, if not all, fail to achieve perfect linguistic competence in a second language, no matter how hard they try and for how long; (d) there is no empirical evidence that UG principles cannot be taught or are not actually taught in classrooms (e.g., in English we must have a subject before a verb, hence "-null subject"; in Chinese we can omit the subject of a sentence when the context is clear, hence "+null subject"); and (e) many errors that we observe with L2 learners are errors not due to UG parameters, but due to the influence of L1 properties (e.g., subject-verb agreement errors and word order arrangement errors produced by Chinese learners of English). I think that no one would want to deny that as distinct groups, child L1 learners and adult L2 learners display qualitative differences in their ultimate control of the target language.

Related to the above question is the fact that there are also individual differences among L2 learners in how well they acquire a second language. Again, if L2 learners have full access to UG principles and if UG serves as a solution to "the logical problem in L2 acquisition", why can some L2 learners acquire their second (or even third) language to near-native proficiency while others can barely produce complete sentences after years of exposure to the language? One might be tempted to say that the latter are not motivated to learn the language, or that they are too old to learn it. But, then we are using UG-external factors to account for individual problems for

which UG does not give uniform solutions. How much weight are we going to assign to UG versus non-UG factors in accounting for the learner's ultimate performance? Individual differences of the kind observed in L2 are rare among children, because children (at least in the claims of UG) all seem to uniformly acquire their first language to native proficiency no matter where they grow up and which language they learn (not counting putative feral child cases like Genie).

In order to address these two issues, I think that EFM need to give strong empirical evidence showing that (a) the differences between L1 and L2 acquisition are indeed not due to variability in the accessibility of UG, and (b) there is a cause-effect relationship between UG constraints and L2 learners' linguistic behavior. I don't think that we are provided with such evidence, or even that it is possible for EFM to provide such evidence, given the state of the art in L2 research. We are told by EFM that L2 learners are sensitive to some grammatical violations (in grammaticality judgment) and are able to repeat some formalized sentences (in elicited imitation). However, grammaticality judgment, elicited imitation, or the like at most tell us whether L2 learners are sensitive to certain parameters; even there the results are often consistent with alternative explanations. Consider the experiment in which a L2 learner judges the grammaticality of the sentence "which man did Tom fix the door that had broken?" (3.2.2, p. 28). The learner could reject such sentences on a number of grounds, for example, that the phrase "which man" cannot be assigned a sentence constituent (something they have learned in school about English wh-questions), or that they have never heard of a question with the structure "wh-element + auxiliary + subject noun + verb + object noun + relative clause". Moreover, how can one tease apart the constraints of UG from the influence of the learner's L2 experience in a grammaticality judgment task or an elicited imitation task? This becomes more difficult when one uses subjects who have substantial linguistic experience in L2 (as are the "advanced" students who have studied in the US, or children who have lived in Massachusetts for 3 years in EFM's experiment, 3.2.2; 5.1). There are too many degrees of freedom in accounting for EFM's data, and it is not clear to me that there is a direct link between UG principles and the experimental patterns they observe.

EFM argue that UG allows us to describe the knowledge base of the L2 learner, i.e., the end-state grammar. They insist that the *no-access hypothesis* "fails to distinguish between what the L2 learner knows (content of grammar) and how he attains this grammar (process)" (2.3, p. 10). However, it seems that EFM's *full-access hypothesis* fares no better in this regard because it tells us at best *what* the learners possess (the content), but not *how* they come to possess it (the process). To EFM, *what* implies *how*, since UG is biologically determined. It's like saying: UG leads to end-state grammar with the right parameter settings, for both children and adults, but don't ask me how, because UG is innately given. At one point, EFM say that it is difficult to empirically distinguish the claim of direct access to UG versus the claim of access to UG via L1 (2.4.). The difficulty arises, I think, because the UG account of L2 does not allow us to tap into the underlying processes governing L2 acquisition. The difficulty also poses a serious question related to my earlier point: How can we test the role of UG empirically in L2 through rigorously controlled experimental situations?

In sum, I think that there is more to be said about the relationship between UG principles and L2 acquisition, but I don't think that EFM have provided us with strong lines of theorizing or rigorous methodology in testing UG principles in L2 acquisition.

Acknowledgments

I would like to thank Liz Bates, Michael Bond, François Grosjean, and Thomas Lee for their helpful comments on an earlier draft of this commentary.