

Evaluation of Malgorzata Forys-Nogala's doctoral thesis (Robert DeKeyser)

My general impression of this dissertation is that it shows great mastery of both the literature and experimental methodology.

The literature review is impressive, both in its comprehensiveness and its organization. The author is dealing with several complex and controversial topics (and their interaction!), and she does so with great skill, showing how well she knows the corresponding bodies of literature. I found the first two chapters to be a bit better than the third one, in the sense that in the latter sometimes the publications that are referenced are perhaps not the best illustrations of a certain point of view. I got the impression that the author is less familiar with the literature on age effects than with the topics of implicit vs. explicit learning, skill acquisition theory, and the interface issue. The third chapter, on age effects, is still a good one nonetheless. The fourth chapter, on the interface issue, also does a good job of summarizing a large and somewhat scattered body of literature, but there are two points I find less felicitous. First, after carefully using the terms declarative-explicit and procedural-implicit up to this point, the author shifts from declarative and procedural to explicit and implicit on p. 81: "if declarative knowledge can be of any use in the formation of implicit procedural knowledge." She does not go back to the explicit-declarative vs. implicit-procedural distinction till the conclusion chapter. I don't think she ever comes to terms completely with any difference that may exist between the declarative-procedural and explicit-implicit dichotomies.

Secondly, and perhaps more importantly, when the author describes the three basic "scenarios" to be tested empirically, she labels the second scenario "direct interface – enhancement" (p. 84), but it seems to me that what she calls the "scaffolding model" on p. 81 fits perfectly into this scenario (outcome expected is better "implicit procedural knowledge" with than without provision of declarative knowledge). Yet, on p. 81 the scaffolding model is called "indirect influence, not the interface," UNLESS I am misunderstanding the transition ("In fact") between the two sentences there (9-7 lines from bottom). At any rate, I agree that

positive influence of explicit knowledge through increased input is not interface, but I think the scaffolding model is (and therefore corresponds to scenario 2). Perhaps all that is necessary is a more explicit transition on that page. If, however, the author meant that the scaffolding model is not an instance of (direct or indirect) interface, then I find the terminology confusing. Also, the terminology on p. 84 of “direct interface” and “no direct interface” makes the reader expect there will be a third category of “indirect interface,” which does not materialize. I wonder if it would not be better to label the first two scenarios “no interface” and “indirect interface.”

In a few places in the literature review there are some viewpoints I have doubts about, but I don't think this is the place to debate them. I am only listing them in case they can be useful for the author:

- 1) On p. 28 the author quotes Paradis (2009) to say that implicit linguistic competence is procedural memory. There is no doubt that this competence is implicit, as children (and most adults) are not aware at all of its contents. Some linguists, however, would say that this competence is declarative, because it is about abstract rules that do not translate into behaviors. My feeling is that this is a consequence of the Chomskyan dichotomy between competence seen as mere representation and performance seen to be about processing. From a connectionist point of view, for instance I think there would only be procedural knowledge (and no competence in the Chomskyan sense, only in a wider sense, which would be compatible with the procedural characterization).
- 2) I don't think it is a good idea to say “there is a trade-off between the pace of learning and the quality of resultant linguistic representations” (this actually comes from the introduction, p. 9). Often there is such a trade-off, when children are compared to students of a foreign language, but adults without instruction and with little verbal aptitude do not learn fast and do not achieve good representations either.
- 3) On p. 53 (quoting me) and on p. 59 (quoting MacWhinney), the author states that phonology and phonetics are most affected by age of learning, followed by syntax and morphology, with lexical knowledge being the least affected, but she does not mention

non-lexical, i.e. phrasal semantics. I think the latter is quite age-sensitive, and that is not without importance for the learning of English articles by Polish learners, e.g., given that these are the targets of Exp. 1.

- 4) On p. 101, the author argues that the finding in Suzuki & DeKeyser (2017) that automatized explicit knowledge impacts implicit knowledge “does not discriminate between the indirect influence scenario, and the direct interface scenario.” If the indirect interface scenario were correct in the sense that explicit knowledge only helps because it helps to generate input for implicit learning, wouldn't there be a significant relationship between implicit aptitude and implicit knowledge then? Such a link was not found in that study.

The empirical part of the dissertation is equally impressive as the literature review. The study is innovative in three important ways: 1) the combination of the implicit-explicit distinction as both dependent (treatment) and independent (knowledge) variable, in a natural language, 2) the use of a semi-implicit condition as one of the three treatments (Exp. 1), and 3) the use of a cleverly-designed task to hide the grammar-learning purpose of the experiment, even in the explicit condition where grammar was taught (Exp. 2)!

The methodology of the empirical studies is state of the art: from very careful design with balanced input during the treatment, and careful measurement of both implicit and explicit knowledge, assessed through both task design and source attributions, to the sophisticated statistical analysis and the careful interpretation, often including follow-up analyses, to test alternative explanations.

I do have some lingering doubts about the nature of the third condition (semi-implicit) in Experiment. It seems to me that the combination of explicit information and relevant input is an invitation to guided induction (cf. work by Ron Leow and associates, e.g. Cerezo, L., Caras, A., & Leow, R. P. (2016). Deductive instruction on the development of Spanish complex *gustar* structures. An analysis of learning outcomes and processes. *Studies in Second Language Acquisition*, 38, 265-291), which would

mean an explicit learning process. Given the outcomes (neither implicit nor explicit knowledge), however, either my interpretation is wrong, or this particular implementation of guided induction was too difficult. I lean towards the latter, but the author is probably in a better position to judge that than I am, given her intimate knowledge of the details of implementation.

The writing is excellent, in terms of organization, clarity, and correct language use. In the whole dissertation I barely found a couple of typos or instances of non-nativeness. In spite of the rather technical nature of both the literature review and the account of the empirical studies, I found the dissertation relatively easy to read. That speaks highly of the quality of the writing.

The one point that I really regret the most is the very short duration of the experiments, with basically one grammar training session, which does not really give implicit learning, or even proceduralization of explicit knowledge, a fair chance. The author acknowledges this in the limitations section, but the absence of implicit knowledge under these circumstances was to be expected, given what has been shown, or at least argued, in previous literature. Still, the study is useful, as at least some researchers claim to have found (very limited) implicit learning of morphosyntactic elements in experiments of short duration, and as the present study, with more ecologically valid treatments and without diminishing experimental control, clearly showed there was no such learning here, neither for the semantically difficult and non-salient structure in Experiment 1, nor for the easier and more salient structure in Experiment 2.

In conclusion then, I find this dissertation to be a very important addition to the literature on implicit vs. explicit second language learning. I congratulate the author and her teachers and advisors, and I hope the findings will soon find their way into some of the most prestigious journals in the field.

