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PhD Thesis Report:

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"The development of L2 inflectional morphology and cross-language interference effects"

This thesis investigates production and perception of inflectional morphology in learners of English at different levels of language proficiency. Reaction time experiments were conducted to test how Czech learners of English respond to English inflections when processing novel and real English words. In a series of three experiments it was shown that learners – similar to English native speakers – seem to decompose stems and suffixes when processing novel words. In addition, native language phonotactic structures of the learners were shown to have an effect on morphological processing, with Czech having a mildly facilitative effect on performance. The thesis is well written, postulates clear and concise hypotheses, and the experiments are carefully planned and executed. The results can help fill important research gaps in the current literature

on morphological processing in language learners. Some conceptual and methodological issues are addressed below (per experiment). Generally, the dissertation is of impressive scope and Ms. Jirankova has demonstrated her excellent research skills and outstanding ability to investigate complex and interwoven psycholinguistic research problems.

Ad experiment 1:

Morphological compexity per se may not have the effect you suggest in the introduction to experiment 1. What is more important is the morphological similarity in form and/or function. The question is what inflectional morphemes do you find in Czech that are similar to the English ones? In terms of function, phonology, allomorphs, segmental position etc. In a foot note on page 54 you mention some differences between Czech and English inflections (consonantal versus vocalic). The exact nature of the inflectional differences and similarities between your investigated languages seems a crucial point to me and deserves a more thorough discussion in a cross-language study.

Page 10/11: I would argue that L1 may not be the main factor impacting the order of acquisition here. The frequency and probability of use of an inflectional morpheme in the target language is of paramount importance. The list presented on page 6/7 shows increasing complexity and decreasing frequency, and this could explain some of the findings you report in your study. A usage-based approach to the learning of inflectional morphology may be advantageous in explaining some of your findings. These arguments can be found, for instance, in Blevins et al. ("An entropy-based measure of morphological information"), Ackerman & Malouf ("Morphological organization: The low conditional entropy conjecture"), and Franzon & Zanini ("Entropy of morphological systems…").

Page 11:

"Goldschneider and DeKeyser (2001) imported these factors into a multiple regression analysis, and their findings show that "a very large portion of the total variance in the acquisition order of grammatical morphemes in English can be explained by a combination of five factors: perceptual salience, semantic complexity, morphophonological regularity, syntactic category, and input frequency"

The phenomenon of L2 inflection acquisition seems multifaceted and likely not explainable by single factors. It would have been advantageous for your study to include a wider variety of known variables (such as some of the ones cited above) in your statistical models.

You provide a very thorough explanation of the mixed regression models you performed. The statistics are sound and well done. On page 41, it would be interesting to know how many outliers you had per proficiency group. Table 46 in Appendix 1 shows the phonotactic probabilities as calculated with the KU phonotactic probability calculator. The software automatically adds a "1" to the sum of all frequencies (as stated in the publication describing the calculator), so each of the probabilities should be presented minus 1. Also, the probabilities that are calculated by the website are sums, so you would need to divide them by the number of segments or biphones per word.

You include the two phonotactic probabilities (segmental, biphones) in the 3.4.2.3. model. I was wondering if the two are correlated. The fact that one is significant in the model but the other is not, hints otherwise, but it would still have been informative to see the VIFs or a correlation coefficient. Generally, all graphs and tables are nicely done and informative.

You state on p. 51

"The dataset was therefore divided into two halves: (i) stimuli with shorter and (ii) with longer duration and it was plotted for a proper understanding of the situation"

How did you categorize "short" and "long" here? How did you "halve" the dataset?

Since all of your variables were shown to be significant in the model, you could have reported the standardized regression coefficient to show which of the variables contributes the most to the effect you found.

I agree with your assessment that A0 is not really comparable to the other proficiency levels. I still appreciate your inclusion of the results for A0 in the graphs, such as for instance Fig. 7 or Table 6, but they should be treated with caution in comparison to the rest of your data, as you mention.

Page 55:

„This suggests that morpheme decomposition might be simultaneously accompanied by some frequency effects and that both theories might equally predict the reaction times.“

I fully agree with that assessment. The frequency effect is a very interesting finding.

Page 55:

„r (118) = -0.316“

This correlation is rather weak. I would caution against drawing conclusions from this. However, figure 8 certainly looks interesting.

Ad experiment 2:

Page 90: I consider the three-way interaction between level*similarity to regular*similarity to irregular a bit confusing. What is the rationale behind it? The two-way interactions between level and the other two variables seems more informative to me.

The only variable that is really significant is “level”. P-values of >0.06 are actually quite a large. The only marginally significant variable is the three-way interaction.

Other variables that would have made sense to include are phonotactic probability (biphones) of at least English but potentially also Czech, and age-of-acquisition of the words to which the novel words are presumably similar. For instance, the word “dize” is a phonological neighbor of “doze” (“the baby dozes”) but this word may be unfamiliar to learners at the A1 or A2 levels. Similar for “glipse” and “glimpse” – at what proficiency level do learners know “glimpse”? Lexical knowledge of the learners at the various proficiency stages should have been considered. In a similar vein, did you control for the context of the novel words? For instance, “the baby dizes” is similar to “dozes” and makes sense in this context. There could be some priming from the context of the phonological neighbor words.

More variables may turn out to be significant if words are controlled for age-of-acquisition. For instance, the ANOVA results p. 94 may also reflect lack of knowledge of phonologically similar real words. Or p. 99:

“Group post-hoc tests have shown that the performance of the A1-, A2-, and B1-level participants was not predicted by either the similarity to existing regulars or existing irregulars. Such findings do not seem to

correspond to the single-route model and incline more to a dual-route model of morphological productivity."

The fact that only at the B2 level do you find a significant effect (p. 96) could indicate that your set of words is more suited to higher levels of lexical knowledge, such as native speakers or learners of higher proficiency.

Table 27: what comes to mind is that phonological similarity to real words may be perceived differently by different proficiency levels. The question of onset and rhyme phonological neighbors and easeness of phonological-neighbor-categorization might play a role. In addition, a word like "pank" may have been regularized to "panked" instead of "punk" because "punk" is already a word in the lexicon. Avoidance of homophones could also have played a role.

Ad experiment 3:

Similar to experiment 2, a control for lexical knowledge of the real words would have been beneficial.

Fig. 25 is difficult to interpret. The cause of the significant interaction is not immediately obvious. I agree with your assessment:

"It is reasonable to assume that low-frequency real words are recognized with greater accuracy in more proficient L2 learners, explaining the pattern observed".

It would have been interesting to see a graph with the plottet frequency classes of the words.

Despite some limitations and less satisfactory results, the third experiment is nonetheless interesting and offers intriguing insights into the role of L1 in L2 lexical processing.

Minor comments:

Page 154:

"Our findings thus do not seem to support the view that the dependence on L1 decreases with higher proficiency". Didn't you show in experiment 2 that Czech phonotactic legality has an effect in the lower but not higher proficiency levels?

"In general, our findings on language transfer and the effect of L1 in all three experiments have shown that Czech might have a facilitatory effect both on the perception and production of English inflectional

morphology. This might be caused by the fact that Czech is morphologically much richer than"

Since phonological similarity played a crucial role in your findings, one could assume that phonologically similar inflectional morphemes in Czech and English would be subject to transfer effects. I am sceptical that the morphological richness of the compared languages itself can explain your findings.

Language

- The use of "we" and "our" is unusual in a scientific work
- I wonder why the mean age of the participants is relatively high (e.g. in exp. 3 it is 36 years).

Typing mistake:

- P. 6: "undercover"

Page 44 (and ff): I think you are referring to the pseudo-code of the model rather than the "R code" as stated. Instead of using something like "*MGL_R:MGL_IR + MGL_R + MGL_IR + (1|part) + (1|item*)" (on p. 106), you may spell out the variables in form of a real pseudo code for better readability.

Questions for the defence:

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1. How do your findings on word learning support and enrich the current state of knowledge in the psycholinguistic literature?
2. What role, do you think, does inflectional similarity in form and function play in your findings?

I suggest the thesis to be accepted with a grade of *výborně*.

Eva Maria Luef, PhD