Review of dissertation

entitled

Event-Related Potentials in the Study of Hungarian-English Bilingual Visual Word Recognition

written by Petra Ihász

The topic of the dissertation concerns bilingualism focusing on the recognition of written words in late Hungarian–English bilingual subjects. The goal of the study is to gain information about the temporal characteristics of the recognition of various words and pseudo-words at the orthographic, phonological, and semantic levels of processing. The investigation of the process of visual processing and word identification in bilinguals is a rarely investigated field with Hungarian L1. The author used various test methods including EEG examination, which is unique in the research of bilinguals with Hungarian L1. The results are interesting and greatly contribute to the field of written word recognition.

The Introduction of the dissertation consists of three chapters with several subchapters and ends with the introduction of the author's own study. The methodology is well-written and covers the participants, test materials, procedure, and analysis. The chapter on the results consists of 3 subchapters according to the various tasks. The dissertation has a Discussion and a Conclusion chapter followed by References and Appendices.

I have to admit that the EEG examination falls outside of my research area. I am familiar with this kind of analysis to a certain degree (based on several papers I read in the relevant literature), but I do not possess the knowledge to make critical comments about that part of the dissertation. My impression is that these results are correct and give new insights into the process of written word recognition.

Based on the topic and the content of the dissertation, on the methodology applied, and on the results and their evaluation, I strongly recommend the PhD degree to the author of the dissertation. My task is, however, to give feedback on various aspects of the study. Therefore, in the next paragraphs I am going to call the author's attention to some problems, inadequacies and shortcomings of the dissertation. At the same time, I would like to emphasize that the points I will mention do not decrease the value of the study.

I would like to emphasize that the points I have mentioned are not listed in order of importance.

The structure of the dissertation is logical, clear, and the subchapters are well-connected. The dissertation, however, is not well-proportioned. The introduction is a bit longer than the empirical part (52.5 pages vs. 46 pages), which is highly unusual. In addition, I have not found any reasons or explanations for this anomaly.

The Introduction with the diversified subchapters makes the whole concept unclear. The reader's impression is that the author could not select among the really decisive findings, theories and models available in the literature, and could not judge them.

Studies on bilingual processing at the word level (p. 2) are introduced very briefly. In addition, the related Hungarian studies are not mentioned (e.g., F. Honbolygó, V. Csépe and many others). What is the reason for that?

Handedness can be important, but the way it is discussed in the Introduction shows the old schools' concepts based on 20-year-old papers and a 9-year-old paper (p. 9). The statement that "the dominant hemisphere for language cannot be absolutely determined by one's preferred handedness" on page 10 is absolutely correct, but it seems to be a commonplace since a great number of studies have confirmed this interrelation. My opinion is that the topic of handedness is completely superfluous in this study. The author deals with that neither in the data analysis (no comparisons depending on handedness) nor in the Conclusion. Why then does she discuss it in the Introduction?

I do not see how the five most common hypotheses on brain lateralization in the case of bilinguals (p. 14) are related to the recognition of written words.

The Critical Period Hypothesis (p. 16) is hardly connected to the present dissertation, particularly considering the fact that there is no agreement about it among the scholars.

I think that the author wanted to report many parts and findings found in the literature concerning the recognition of written words (from brain anatomy through visual recognition processes up to the mental lexicon, and many others). My opinion is that less would have been more. The decisive findings should have been discussed in detail and the less important ones that are not closely related to the topic could have been introduced more briefly. It would be better to focus on the main parameters and to provide deeper and fine-grained information in order to establish the upcoming experiments more appropriately.

Some paragraphs that are in the Discussion and Conclusion chapters should have been put into the Introduction (pages 87 and 88). The points raised in the Introduction should be reflected upon in the Discussion or in the Conclusion and vice versa. I think that this expectation is not fulfilled appropriately in the dissertation.

The Introduction contains a paragraph on the most frequent psycholinguistic methods for measuring lexical processing (p. 36). It is very unusual to list the possible methods in the Introduction. If this is important, it could be outlined at the beginning of the Methodology section as an explanation for the selection of the author's own experimental methods.

The claim that "Modern neuroscience techniques prove that the left occipito-parietal junction is significantly involved in visual word recognition compared to the right occipito-parietal junction (Cohen et al., 2002)" cannot be supported by a reference that is more than 20 years old. Not in this scientific field.

The description of the phonotactic rules (p. 48) does not go beyond the characteristics of a textbook. For this study, the phonological approach should have been more scientific.

I could not find any transition from the topics of the Introduction to the present study (p. 53). The paragraph on word superiority effect is abruptly followed by the description of the experimental part of the dissertation. Here a more compelling argument should have been provided for why this research is relevant in a Hungarian bilingual context.

Although the tasks of this study are carefully designed and have methodological advantages, it is not clear how the results can be interpreted and contextualized for several reasons.

For me, it is not clear how the experimental group was formed. The authors admit that out of the 23 participants, 17 identified themselves as bilinguals. Does this mean that 6 participants were not bilinguals according to their own judgement? Were they still members of the experimental groups?

We learn about the participants that they spend at least half an hour a day reading English books and articles. We do not know any information about their reading habits in Hungarian, which may be important particularly in our days.

I do not think that Table 2 contains any relevant information considering the goal of the study. No further analysis can be found on the language knowledge of the bilingual subjects.

It is favorable that the author examined the frequency of words since this parameter is the most decisive one in the processes of lexical access. I am aware of the fact that there is no

methodology to check the individual word frequency of the participants. If this dissertation is planned to be published, it is highly recommended to note that the Hungarian National Corpus (used in this study) is a corpus based on written texts (journals, books, etc.), thus, the word frequency data does not meet necessarily with the speakers' own word frequency parameters that are based both on read and verbally produced and comprehended words.

What kind of post hoc tests were used in the statistics (Anova, page 68)?

Sometimes, the results of the statistical analysis are given in an unusual way (e.g., on page 78).

The discussion contains some unnecessary repetitions, such as the typological differences between Hungarian and English.

The author claims that her data revealed significant differences between the recognition of the two languages, especially between 320 and 520 ms in the central region (p. 87). No explanation is given for this result.

The finding that phonological and semantic representations are needed to identify a visual word (p. 90) is not new, moreover, it is obvious and has been confirmed by several authors so far. This statement is repeated again on page 96, claiming that phonological awareness has an important role in visual word recognition. This concept can be found in the reading models already in the 80s. It would have been better to phrase these parts of the text as some confirmation based on the author's new experimental data.

The author states: "Furthermore, regardless of the typology, there is no difference between the recognition of L1 and L2 words in the case of highly proficient bilinguals." I do not think that the results of the present experiments allow the author to generalize her findings as this sentence suggests. More importantly, the results of the study cannot be extended to bilinguals, in general, just to those who participated in these experiments. In addition, the question may arise whether the fact that the participants of this study are late bilinguals and acquired English as a second language at school (I suppose mostly via writing and reading) influences the results of written word recognition in the participants' L2.

On the first page of the dissertation the author writes: "The present paper draws attention to the significance of how bilinguals might differ from monolinguals, and how their (language) learning strategies and word recognition patterns differ from each other" (p. 1). It would be a good topic, but the present study does not deal with any comparisons between bilingual and monolingual speakers. Thus, this statement is unclear.

Finally, let me repeat my opinion again. This is an outstanding dissertation with a number of new findings; therefore, I strongly recommend the PhD degree to the author of the dissertation.

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