



Warsaw, 17.09.2023

Prof. dr hab. Ewa Haman  
MultiLADA Lab  
Faculty of Psychology  
University of Warsaw

## REVIEW

of the PhD Thesis by **Jonas Walther**

entitled

**“REGULATION, COMPETITION, OR COOPERATION?  
EXPLORING LANGUAGE ACCESS AND COGNITIVE CONTROL  
UNDER DIFFERENT ENVIRONMENTAL DEMANDS”**

The PhD thesis by Jonas Walther, written under the supervision of Professor Zofia Wodniecka-Chlipalska and Dr. Kalinka Timmer comprises six parts (plus references and appendices). It is a compilation of three manuscripts prepared for publication (one already published in a renowned journal: *Bilingualism. Language & Cognition*; the status of the other two is not provided). The three manuscripts (called “Investigations” in the thesis) are accompanied by an Introduction and overview of the research program as well as a General discussion summing up all research presented earlier. The three investigations present the results of two different experiments conducted with adult bilingual speakers of Polish (L1) and English (L2). The first and the second ones are related to the same experiment examining environmental/contextual effects of bilingualism (immersion in L1 vs. L2 and reimmersion in L1) on lexical access in L1 (Investigation 1 analysing behavioural data from the experiment, Investigation 2 analysing electrophysiological data from the same experiment). The third investigation provides results (both behavioural and electrophysiological) of another experiment where different type of contextual effects is considered: forced vs. voluntary language switching context. This investigation also includes two pilot/preparatory studies that enabled the careful selection of linguistic stimuli for the main experiment. The author of the dissertation is the first author of all three Investigations

(in the case of the two first investigations he shares first authorship with another researcher: Alba Casado). The other co-authors are supervisors and other members of the LangUsta Lab.

The overarching goal of the research program presented in the dissertation is to explore flexibility and dynamic changes in cognitive processes underlying language processing in bilinguals in relatively short time scales and different contexts. The program clearly addresses and advances the current state of the debate on the cognitive consequences of bilingualism and contributes to the specification of conditions under which using more than one language can affect the cognitive resources of a bilingual person. To this end, complex and novel experimental designs were used and an impressive number of participants were recruited, allowing for advanced multifactorial statistical analyses. Below, I provide a more detailed evaluation of each Investigation and finally provide a review of the structure and logic of the whole dissertation.

Investigation 1 explores the issue of lexical access in bilinguals. Lexical access is measured in an oral picture naming task by naming latencies and compared in two groups of relatively balanced bilinguals: (1) immersed in their L1 (Polish, adults living in Poland), and immersed in their L2 (English, adults living in the UK). Crucially for the aims of the study, all participants are tested twice: the group immersed in L1 serving as a control group is tested in stable /not-changed conditions (both time points being immersed in L1); the group immersed in L2 with the status of experimental group is tested once being immersed in L2 and once right after a temporary re-immersion in L1 (visit to Poland). Also importantly for the design, since participants are assumed to be late bilinguals (have started to learn English after early childhood), the language performance of all participants in their L2 (English) is not only assessed subjectively (with a self-rating scales) but also with standardised tests. Materials for the study (pictures and their adjacent target names/words) were also carefully selected (with the control of word frequency etc.). Authors formulate clear hypotheses based on previous studies regarding the expected results. What I like most about this study is that the results are not fully accordant with the hypotheses and that the authors are brave enough to

admit this and to propose an interpretation of the actual results, suggesting new venues for understanding flexible and dynamic lexical access in bilinguals depending on changing environmental context. First, according to the results, long immersion in L2 does not hinder lexical access in L1. This could be good news for migrants who may sometimes be scared of their L1 undergoing attrition. Second, short-term re-immersion in L1 enhances lexical access to high-frequency L1 words, which shows the flexibility of the system that is able to quickly adapt to changing linguistic context. Since this Investigation is already published and must have undergone the rigorous review process, I am not going to comment on any details of the study. However, I have some questions regarding the methodology and analysis that could help in clarifying to what extent these results indicate that hindered lexical access (in L1) in bilinguals may be a myth.

First, how single picture naming (where pictures present exclusively physical objects evoking nouns) can be related to the everyday use of language in interactions and situations rich in communicative contexts? Using a language (in fact: a single word) in a picture naming situation, when just one object is visible and of interest is much different from a real-life situation when people are talking about various things/events/topics not being present around and not being objects alone. I am aware that picture naming task (evoking nouns only) is a common way of studying lexical access and also that it was already criticised for its low ecological validity. Still, I wonder what ideas of the author's of the thesis are on this issue and how this limitation of the study could be overcome, maybe by proposing a different/new paradigm. Sometimes only a simple modification of the experimental design is enough (as in the case of Investigation 3, introducing the condition of voluntary language switching, also see (Blanco-Elorrieta & Pylkkänen, 2018), but in the case of measuring lexical access in a situation more resembling everyday language practices, this may be a more demanding challenge.

Second, the effect of word frequency (in fact interaction of word frequency and context) was revealed in Investigation 1. This is quite astonishing considering that the pictures to be named (and adjacent target words) come from a base of pictures intended to

be used for language assessment in relatively young children (under the age of 6), so differences in frequency cannot be that huge. Still, I wonder why really low-frequency words were not added to the materials, since the real effect of word frequency may be in fact hindered here (results of Investigation 2 may additionally support my suggestion). Although no examples are provided in the description of the experiment, still raw data are available (which is becoming a kind of new standard, and I appreciate very much that the authors follow this standard!). I have extracted examples of words of lowest and highest frequency from the dataset (Table 1.) and it is clear that low-frequency words are not very different from high-frequency ones.

Table 1. Five target words with the lowest frequency index and five target words with the highest frequency index in the materials for Investigations 1 and 2.

Low-frequency words		High-frequency words	
PL (language of the study)	EN (English translation)	PL (language of the study)	EN (English translation)
pieczarka	champignon	drzwi	door
cymbałki	cymbals	dom	house
temperówka	pencil sharpener	samochód	car
kręgiel	bowling	telefon	telephone
domofon	entry phone	serce	heart

I would welcome a comment from the author on whether adding verb items (available in the picture base used for the experiment) could help in both ecological validity (see above), and/or in widening the frequency range and what could be the consequences (both empirical and theoretical) of using a wider frequency range.

In Investigation 2 neuroimaging data from the same study as in Investigation 1 are considered. The pattern of the results is somewhat different than that obtained in Investigation 1 for behavioural data, and most of the hypotheses are not confirmed. First, there were no overall differences between groups (in any of the two neural components used: P2 and N300). Second, there were no frequency effects (as expected based on behavioural results). Third, the N300 component occurred to play no role in any of the analyses. The only significant result was the effect of context in the migrant group for P2. Here again, I wonder whether the specificity of the materials used (common objects with

adjacent names being relatively frequent words, mostly easy even for children) prevented observation of processing demands that are present in real-life situations or one can assume that the processing demands in L2 immersed bilinguals are mostly not that different from those in L1 immersed bilinguals. Also again, I very much appreciate how hypotheses are presented although they are mostly not confirmed by the results. However, I am curious why no Bayesian statistics were applied to the data when non-significant results were revealed. This could show to what extent it is possible to infer real no-effect results (confirmation of the lack of differences). If the results are in fact inconclusive, further interpretation is just pure speculation.

For both Investigation 1 and 2, a very smart semi-manipulation of the context was used: participants from the migrant group were tested once after the period of constant immersion in L2 (no travels out of the country of residence for a longer time) and once just after their visit to the country of origin (Poland), treated as a short-term reimmersion in L1. Since the effect of context was measured in the within-subject design, the potential low opportunity for controlling the manipulation (e.g. length of the reimmersion; intensity of Polish use, interval between two testing points etc.) should not be a problem. Still, I wonder whether there would be some other effective way of pure manipulation of the reimmersion that could open new opportunities for both controlling additional factors and introducing them into the analysis. In the current design, there was no effect of the length of stay in Poland (although the range in days was quite big, when checking in the raw data: from 4 to over 40 days). However, since the experiment was to attest to the flexibility and dynamic changes in the bilinguals' cognitive system according to the changing context, it could be interesting to establish the minimal level of reimmersion that is needed for the effect to take place. How this could be approached?

Investigation 3 partly answers this question, although not exactly in the context of lexical access, but assessing another aspect of the functioning of the bilingual mind, i.e. language switching and its cognitive costs. The experiment described in this part of the thesis explores the effects of short language switching training (in the form of picture naming

in forced vs voluntary language switching conditions) on the non-verbal domain (cognitive control as measured by non-linguistic task switching paradigm). Its results reveal that although there was no overall effect of language switching type (no significant change in switch cost from pre-test to post-test between the two groups), the careful design of the experiment (and in particular very good control over characteristics of the linguistic material used for training) allowed for observing the difference in mixing costs between the groups. This behavioural result was also supported by neural data (P3 component). The main experiment presented in Investigation 3 required particularly careful preparation of experimental materials. This was achieved by two pilot studies in which specific characteristics of pictures to be named and relevant target words was gathered. Namely, in Pilot 1 participants (Polish-English bilinguals of high proficiency in both languages, similarly to the main experiment) were supposed to indicate in which language they would prefer to name the picture. This was needed for identifying pictures with high language preferences for one of the languages to be used in the main experiment (i.e., Polish and English), also participants were to name the pictures which enabled to assess naming agreement. In Pilot 2 the possibility of eliciting voluntary language switching with pictures of high language preference was assessed. These two pilots were crucial for obtaining materials for the main experiment. Much effort was put into this and I appreciate the process very much. Since the training used in the main experiment had to be relatively short, I wonder whether it might be interpreted as a kind of minimal time needed for modulating bilinguals' cognitive control processes by introducing demanding (forced) switching condition. In other words: could be the duration of the training interpreted in this way? Would it make sense to ask whether by manipulation of the training duration, we could assess the minimal time (or effort?) of training needed for obtaining some (or more?) transfer to the non-linguistic domain of cognitive control?

Above I have posed some questions that can possibly enable us to further discuss the results obtained in the two experiments, described in the three Investigations. I will sum up with general remarks leading to the final conclusions. Overall, I appreciate the

composition and structure of the dissertation. The introductory and closing parts are clearly showing the theoretical background and the content of the three main parts (Investigations). In both experiments, participants' characteristics are very carefully measured and described (in particular their language performance in L2 is measured not only subjectively which is a kind of common practice, but also by two standardised tests). To this end, I wonder what caused quite a strong unbalance in gender in both experiments. It is a bit astonishing that there were substantially fewer female than male participants (in many studies the opposite pattern is visible just due to more problems with recruiting male participants). I would very much like to know how to recruit more men to any study. Could this imbalance affect the results?

All three Investigations seem to be ideal material for preregistration. As their content currently shows even with such a careful control over participants' characteristics as well as extremely careful selection of the material used and of the design of the procedure, obtaining the results accordant with the hypotheses posed at the beginning is almost never possible. Maybe I missed something in the description, but it seems that none of the analyses/investigations was in fact preregistered. Why? I think that preregistration (especially journal preregistration) can protect the authors in many ways (also when no significant results are obtained) and I wonder what is the author's position on this issue. In case of non-significant results that are opposite to what was expected, I wonder why Bayesian statistics was not used and whether the author thinks this could help in the interpretation of the results.

This work certainly provides new evidence and knowledge on the flexibility and ability of the bilingual cognitive system to adapt to changing linguistic contexts (being either short-term reimmersion in L1 or short experimental training in language switching). This new evidence is described in a very elegant and straightforward way even though the topic is very complex and requires careful presentation of many details (which concerns both the design of the experiments and the presentation of its results). Although the final answers to the biggest questions regarding bilingual minds are not provided, I would not expect them to occur in the PhD dissertation. On the contrary, what seems more important is that it opens

new venues for further research and I would be happy to hear more about this from the author since the format of the thesis does not allow elaboration on this.

Overall, I am fully convinced that the above evaluation of Jonas Walther's dissertation proves that the author demonstrates: (1) the general theoretical knowledge in the field of psychology (and in particular cognitive psychology and psycholinguistics), (2) the ability to conduct research independently and also in collaboration within the research team (which is in my opinion also an indispensable asset of an independent researcher), (3) the ability to propose an original solution to a scientific problem (which here means presentation of the conditions under which bilinguals' cognitive systems show flexibility in adaptation to quickly changing linguistic contexts).

Thus, in accordance with the Act of 20 July 2018 The Law on Higher Education and Science (Article 187 points 1 and 2) I request that the PhD candidate be admitted to the further stages of the procedure and be awarded the degree of Doctor in Social Sciences (Psychology).

