April 20th, 2024

Edmundo Kronmüller, PhD

Handling Editor

*Cognitive Processing*

Dear Dr. Kronmüller:

We have submitted a revised version of our manuscript COPR- D-23-00175 “Predictive Alternating Runs and Random Task-Switching Sequences Produce Dissociative Switch Costs in the Consonant-Vowel/Odd-Even Task” for your consideration. We appreciate the thorough examination provided by our reviewer and are pleased that our manuscript was well-received. Below, we list our responses to each comment and cite page numbers when referring to specific changes. To facilitate the review process, we have made all primary modifications to the manuscript in blue-colored font. We look forward to your response and hope that our revised manuscript is now suitable for publication in *Cognitive Processing*.

Sincerely,

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**Major points:**

**Comment 1:** In the task description, it is mentioned that participants did not receive prior instruction regarding the specific sequence in which the switch would occur in the "switch blocks". I find this aspect problematic as if participants had been informed in advance that one block was random and the other had a specific sequence, the only difference between both blocks would be the predictability of switching, which is precisely the contrast that was intended to be made. However, by not informing participants of the block structure beforehand, participants might tend to try to find a rule of switching during both blocks, in addition to what they are already required to do. This undoubtedly adds a noise factor, given that it is unknown whether participants are or are not looking for a rule and for how long (for example, if they first had the block of the predictable switch and learned the rule, they are likely to intuit that the second switch block also has a rule). It would be beneficial for the authors to provide an explanation for this methodological choice and discuss its implications in the discussion section.

***Response:*** This is an interesting point. We elected not to inform participants of the switch sequence a priori as we did not want pre-existing knowledge of the pattern to influence their performance. To mitigate concerns regarding order effects on switch block performance, four counterbalanced versions of the experiment were used, which randomized the order of the CV/OE pure blocks and the order in which the predictive and random switch blocks were completed (see pg. 15 of our initial submission for details). As such, any potential effects of switch block order (i.e., participants searching for non-existent patterns in the random block after completing the predictive switch block) would likely be distributed across counterbalances, leading to no differences in switch performance as a function of block order.

To test this assumption, we re-ran all ANOVAs assessing mean error rates, RTs, and switch costs with switch-block completion order included as an additional between-subjects factor. For all analyses, no main effects or interactions of block order were detected (*p*s ≥ .20). It is therefore unlikely that random assignment to complete the predictive switch block prior to the random switch block influenced participants’ performance on random switching.

In our revised manuscript, we have clarified the counterbalancing process on pg. 15 and provided additional rationale for this procedure. For completeness, we now include the findings discussed above as a footnote reported on pg. 15.

**Comment 2:** The authors articulate their predictions clearly, but they do not similarly articulate the hypotheses (mechanisms) from which these predictions derive. I believe that doing so would aid in understanding and clarifying their own objectives. I suggest this also because there is inconsistency throughout the text regarding what the study aims to achieve. In the abstract, it is stated that "The current study compares predictive versus non-predictive task sequencing effects on task-switching performance". In the introduction, it is stated that "the goal of the present study was to investigate how different task-switching contexts would affect working memory processes", and in the discussion, it is stated that "Our primary goal was to assess the effects of predictive and random sequenced task-switching on working memory and attentional control processes by investigating the effects of task-switch sequencing on local and global switch costs". Stating that the objective is to observe the effects on working memory and attentional control seems imprecise. While it is evident that these processes are linked to the task, as their proper execution requires both, there are other processes that could be influencing (cognitive flexibility, inhibitory processes). Without a more direct measure (such as a group comparison using high and low Working Memory Capacity (WMC), or measuring brain activity), it is difficult to conclusively determine that the study is assessing the effects on these processes specifically.

***Response:*** Yes, we agree that clarifying the rationale for predictions would improve their clarity. As such, have we centered our revision around clearing up the inconsistencies you noted above while also streamlining aspects of the Introduction (see our response to Comment 5). Specifically, we now state on pg. 10 of the Introduction that the primary motivation for our study was to assess how different task-switching contexts affect “task-switching performance and, specifically, the effects of these patterns on local and global task-switching costs.” We have additionally revised the first sentence of our General Discussion to specifically emphasize changes in task-switching performance (pg. 20).

Finally, we note that while the focus is on changes in task-switching performance, local and global switch costs provide useful proxies for investigating hypothesized working memory processes. We have clarified this in the Introduction (pg. 4) and General Discussion while also noting the need for future studies employing more direct methods of assessing working memory processes within the context of task-switching (pg. 26).  
  
**Comment 3:** Related to the previous point, the authors predict that mean error rates and reaction times (RT) would be higher in switch blocks (regardless of presentation) than in pure blocks. In turn, they anticipate higher error rates and RT in the random switch block compared to the predictive switch block. None of these predictions are fulfilled, but the authors do not adequately discuss this in the discussion section. The lack of discussion around these unmet predictions leaves a gap in the interpretation of the study's findings. It would be informative and relevant to attempt to account for these negative results.

***Response:*** In our initial submission, we emphasized possible differences in local and global switch costs patterns given that these measures provide insights regarding working memory and attentional processes which may be overlooked when focusing solely on changes in mean error rates and RTs (please see our response to Comment 2). In our revised manuscript, we have expanded the paragraph in the General Discussion summarizing the differences in RTs and error rates between predictive and random switching to include additional discussion of how these findings relate back to our initial predictions (pg. 21).  
  
**Comment 4:** The authors transparently comment that a previous reviewer noted that the calculation of local switch cost and global switch cost is influenced by a common factor, which is the reaction time (RT) of the predictive non-switch task. However, they do not fully address what this means, and beyond adding that it should be interpreted with caution, they continue the discussion as if the local switch cost had increased in random switch and, separately (as stated in the abstract), the global switch cost increased in predictive switching. In my opinion, given that the local switch cost aims to measure reconfiguration processes, the fact that it is calculated as the difference between switch and non-switch trials is because it seeks to measure the cost effects in the switch trial. However, it is a relevant data point that the RTs are not different between random and predictive switch, indicating that the local switch cost is artificially reduced in predictive switch due to the increase in RT in the non-switch trials. Therefore, under this reasoning, I believe that the authors should only rely on the results of the global switch cost, which are indeed measuring what is expected.

For further reflection on this matter, I would also like to point out that the global switch cost has a clean and common reference point, which are the trials of the pure blocks. However, the calculation of local switch cost does not have a common or pure reference point, since the non-switch trials are not comparable. For a cleaner calculation, there should be a block of "Pure Predictive Switch" that does not heavily involve working memory, of the type ABABABAB, in addition to the one used in the present study AABBAABB. In this way, the non-switch trials of the "Pure Predictive Switch block" could be used for the calculation of local switch cost. This way, there would be no problems like the one presented in the current study.

***Response:*** For consistency with the task-switching literature (e.g., Huff et al., 2015; Minear & Shah, 2008, etc.) and transparency, we have elected to retain the comparisons of local switch costs and their corresponding discussion. However, we now note the lack of a common reference point as a potential limitation in the General Discussion (pg. 22) and suggest the use of a pure predictive switch block as a consideration for future research investigating task-switching costs.

**Minor points:**

**Comment 5:** Several suggestions are provided for reducing the length of the Introduction.

***Response:*** In our revision, we have focused on streamlining the Introduction. Specifically, we have shortened the paragraph on pgs. 3-4 discussing the Stroop task, streamlined our discussion of Huff et al.’s (2015) comparisons between age and AD status (pg. 6), and have reduced the discussion of previous task-switching studies initially reported on pg. 8 of our initial submission (pgs. 7-8).

**Comment 6:** Reviewer suggested moving the discussion of distributional analysis in the Introduction to the Results section.

***Response:*** We have elected to keep the section on distributional analyses in its current location, as this section provides important rationale regarding these analyses which needs to be discussed prior to reporting the results. Moreover, by broadly discussing how these analyses are computed, we provide a more technical explanation of how these parameters were computed, providing important background information for readers who may be unfamiliar with this approach (see Yap, Tse, & Balota, 2009; Tse et al., 2010 who organized their Introductions similarly).

**Comment 7:** Several suggestions are provided for additional citations throughout the Introduction.

***Response:*** We have added additional citations where appropriate. We appreciate the additional literature.  
  
**Comment 8:** The authors should present the knowledge gap and research question early in the introduction to clearly set the stage for the study and provide context for the reader. This will help to frame the research, guiding the reader through the rationale and significance of the study.

***Response:*** In our revised manuscript, we now introduce the research question on pg. 3 of Introduction before beginning our review of the task-switching literature.  
  
**Comment 9:** In the methods section, certain decisions are made, such as "blocks were always ordered such that participants completed the two pure blocks before completing the two switch blocks", and previous studies are cited. It would be beneficial to briefly explain the rationale behind this decision.

***Response:*** We have updated the Methods section accordingly. For example, on pg. 15 of the Procedure, we now note that counterbalancing procedure was specifically modeled after Huff et al. (2015), who had all participants complete the pure blocks before completing the switch block, which ensured that performance on the pure blocks was not impacted by the task-switching instructions, allowing for a non-biased baseline measure of non-switch task performance.

All other minor comments have been addressed in the manuscript. We thank you for taking the time to review our paper.