October XXth, 2022

Jarrod Lewis-Peacock, PhD

Action Editor

*Attention, Perception, and Psychophysics*

Dear Dr. Lewis-Peacock,

We have submitted a revised version of our manuscript PP-BB-22-011 “Alternating Runs and Random Task-Switching Produce Similar Patterns in the Consonant-Vowel/Odd-Even Task.” We are encouraged that our reviewers felt the study described in this manuscript was “interesting and novel” and, particularly, that it “makes a valuable contribution to the task-switching literature.” Below, we list our responses to each reviewer’s comments. To facilitate review, we cite page numbers when referencing specific changes and have made all primary modifications to the manuscript using blue-colored font. We hope that our revised manuscript is now suitable for publication in *Attention, Perception, and Psychophysics.*

Sincerely,

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**Action Editor**

**Comment 1:** One major concern from R1, that I agree with, is the appropriateness of this article for a special issue on working memory. At this time, it is unclear to me how this study advances our understanding of working memory. The sole mention of working memory here at present is for an interpretation of the increase in global switch costs for the predictive alternating runs, where it is suggested that working memory is taxed by "maintaining two task sets" and by the need to "monitor their progress across trials" to anticipate when a switch will be occurring. First, what is the evidence that both task sets were being actively maintained in working memory? Second, what would this task-switching performance tell us about working memory?

In a potential revision, the authors would need to address all the concerns raised by the reviewers and would need to pay extra attention to the framing of this work for a special issue on working memory.

***Response:*** [WORDS HERE]

**Reviewer 1**

**Comment 1.** It appeared to me that the introduction and theoretical rationale of the study substantially lack focus. For instance, the authors describe Stroop effects across several paragraphs even though they are entirely irrelevant to the study at hand and the research question they want to address in the realm of task switching. Similarly, the authors mainly refer to task switching studies that compared specific participant subgroups rather than more general task switching studies. They comment on individual and group differences at several occassions although these differences also appear to be irrelevant to the study at hand. I might have missed something here, but in my opinion, the authors need to streamline the paper to get the storyline straight and more clearly and explicitly articulate their research question's theoretical relevance. At present, it appeared to me as if they just wanted to confirm specific data patterns that, based on my reading of the manuscript, do not really add anything to prior knowledge. Is that really the case and the study is just about confirming a data pattern nobody would have doubted or is there more to it?

Depending on what the authors actually want to argue for, they might also have to substantially adapt their General Discussion accordingly.

***Response:*** Based on your suggestions, we have streamlined the Introduction. Specifically, we have reduced our discussion of Stroop effects (pg. xx), given they are less relevant to the task at hand. Second, we now provide more discussion of general task-switching paradigms (pg. xx) before introducing task-switching studies focusing on specific participant sub-groups. In doing so, we now incorporate several of the references you suggested (see our response to Comment 3) while also clarifying the link between task-switching and working memory (pg. xx). Please see our response to Reviewer 2, Comment 1.  
  
**Comment 2:** Based on the submission information, it appears that the manuscript was submitted for a special issue in working memory. In this case, the authors do not seem to relate to the relevant literature on working memory and corresponding models of working memory at all and they either do not frame their research question appropriately and relate it to working memory or actually there is scarcely a relation. At least that was my understanding based on my reading of the current version of the manuscript.

***Response:*** Please see our response to comment 1.  
  
**Comment 3:** The authors present a very particular and limited set of task switching studies. They might want to, for instance, refer to more recent review articles on task switching (or some of the papers cited in them) and relevant underlying mechanisms in task switching related to their research question (once it is clarified):  
  
Kiesel, A., Steinhauser, M., Wendt, M., Falkenstein, M., Jost, K., Philipp, A. M., & Koch, I. (2010). Control and interference in task switching—A review. Psychological Bulletin, 136(5), 849-874.  
  
Koch, I., Poljac, E., Müller, H., & Kiesel, A. (2018). Cognitive structure, flexibility, and plasticity in human multitasking—An integrative review of dual-task and task-switching research. Psychological Bulletin, 144(6), 557-583.

***Response*:** Thank you for providing these additional references. We have updated our review of the task-switching literature accordingly (pgs. xx-xx).  
  
**Comment 4:** On page 9 there seems to be a mistake when describing the results of Minear and Shah (2008). They report a pattern that is the exact opposite of their hypothesized results. That does not seem to be correct.

***Response***: [WORDS HERE]  
  
**Comment 5:** The methods should be described in more detail. For instance, the time course of a trial is not entirely clear. Was there a response deadline at all etc.? A figure of the trial time course would also be helpful in this regard.

Moreover, it was unclear to me, based on the authors description, whether participants were informed about the predictable versus unpredictable task sequence pattern. This is a rather important piece of information as the first trials of a task switching block should potentially not be included if participants were not informed about the predictable/unpredictable task sequence pattern beforehand.

Additionally, participant exclusions and outlier trial exclusions were not described clearly. What were the corresponding reference means here? For instance, were outliers determined based on the overall mean per participant or the individual cell means per participant (in my opinion the more appropriate option)?

***Response***: [NO RESPONSE DEADLINE] [PARTICIPANTS WERE NOT INFORMED] [TRIMMING PROCEDURE WHICH REMOVED QUESTIONABLE TRIALS] [DETAIL THE TRIMMING PROCEDURE]

We have updated the methods section on pg. xx to clarify these points.

**Comment 6:** The authors present a rather generic sample size justification. They should at least provide information on the corresponding alpha level, power, and on which effect(s) their estimation refers to (Omnibus test of the univariate ANOVA vs. specific comparisons etc.).

***Response:*** We have updated the sample-size justification on pg. xx accordingly.

**Comment 7:** The authors need to include information on their decision criterion regarding Bayes factors. According to the Bayesian criteria and analyses I am familiar with, they Bayes factors constitute inconclusive evidence if I am not mistaken. Here, a clarification of the meaning of certain value ranges is essential and should be added.

***Response:*** [WORDS HERE]  
  
**Comment 8:** The authors conducted multiple analyses on the same RT data. It was not mentioned whether they correspondingly adjusted the alpha level. This information needs to be added. In addition, it was not clear to me why they authors did not only conduct the analysis including vincentiles rather than both the simpler ANOVA and this analysis.   
  
***Response:*** [ALPHA LEVEL WAS NOT ADJUSTED] [WE DID BOTH, FOLLOWING THE DESIGN OF HUFF ET AL. 2015]

**Reviewer 2**

**Comment 1:** Overall, I think the current review of literature on attention control in the Stroop task in the introduction could be trimmed down, and greater emphasis could be placed on the relevant task-switching literature. For example, the introduction section contains several paragraphs describing attentional control in Stroop task and age-related differences demonstrated when using this task. Since the Stroop Task was not used in the study, such a detailed description of this paradigm distracts the reader from the actual experimental task and research questions. Probably, the authors reviewed this literature to introduce attentional control and related control processes such as activation and maintaining the task goal while suppressing task-irrelevant responses. In this case, it would be enough to explain this point in several sentences, or to review articles that used task-switching designs instead. Further, the detailed reviewing of literature on age effects (e.g. p.7) creates an expectation that the authors would later report results for different age groups in their study as well, which is not the case. Because only young participants were tested in the study, the discussion of age effects on attentional control in the introduction is not particularly relevant for the research questions and a bit misleading.

***Response:*** Thank you for your feedback. Yes, you are correct that we included a discussion of Stroop effects in the Introduction as a means of introducing attentional control. We have streamlined this section to be more in-line with the present research (please see our response o Reviewer 1, comment 1). [WORDS HERE]  
  
**Comment 2:** Considering, that the primary goal of the study was to examine the effects of predicted vs. random task switching procedures on different types of switch costs, the article would benefit from describing the relevant processes in more detail in the introduction or at the latest in the discussion section using relevant task-switching literature. The key explanation of the results is a task-set reconfiguration process for elevated local switch costs in random task-switching, and keeping track of the task sequence for elevated global switch costs in alternating runs switching. However, the concept of task-set and its reconfiguration when switching from task to another are only shortly mentioned in the introduction. In the discussion section, some theoretical explanations are included, but in several places the references are lacking (e.g. p. 20, line 47; p. 21 lines 20-33). I would recommend the authors to better adjust the content of the introduction to their research questions and to try to better integrate their results into task-switching literature in the discussion (e.g. Andreadis & Quinlan, 2010; Shahar & Meiran).

***Response:*** This is a fair point. We have updated the Introduction to provide a more in-depth discussion of the cognitive processes involved with task-switching. Specifically, we have expanded our description of task-set reconfiguration processes (pg. xx). We have similarly updated the General Discussion to provide a more appropriate framing of our research questions and now include the suggested references (pg. xx). We appreciate you bringing this additional literature to our attention.

**Comment 3.** The approach to investigate the processes during switching task sets by analyzing the RT distributions is very laudable and represents an important and interesting line of evidence for distinguishing between different types of control processes. The approach to analyse vincentized RT distributions was proposed earlier by Ritske DeJong (2000) AN INTENTION-ACTIVATION ACCOUNT OF RESIDUAL SWITCH COSTS. Attention & Performance XVIII (Ed. Monsell and Driver). The authors should cite that article and relate their approach to that earlier work.   
  
In addition, a more careful consideration of earlier work is necessary throughout the manuscript. For example, the usage of the terms global and local switch costs seems at bit old fashioned because more recent work uses the terms mixing costs and switching costs. If the authors do not want to change their terms, then this is fine with me. However, in that case at some place a reminder would be helpful to inform that global switch costs are the same as what is usually termed mixing cost and local costs are termed usually as local costs.

***Response:*** Thank you for providing this additional reference. We now include it on pg. xx where we introduce RT analyses.

Regarding the terminology used to describe switch costs, [EXPAND]  
  
**Comment 4:** In the description of procedure, the information on how task-sets were cued in switch blocks is a bit confusing. On page 13 (lines 26-28), the authors write that “…the words consonant/vowel or odd/even were presented at the top of the screen in the left and right corners to cue the task set”. On the next page (lines 13-15), however, it is indicated, that “…participants were prompted with the word “letter” or “number….This prompt was located above the stimulus pair”. Do “cues” and “prompts” mean the same, namely to signal the participant which task has to be performed on a given trial, or are these different kinds of stimuli both presented in each trial of switch blocks? The authors should give a more precise description of the task design´.

***Response:*** By “cue the task-set,” we simply mean that the words consonant/odd and vowel/even were always presented in the upper left and upper right corners, respectively, to remind participants of which key was associated with each response (i.e., consonant/odd responses were made by pressing the “p” key, while vowel/even responses were made via the “q” key. For switch blocks, an additional cue was placed over the stimulus (e.g., “Letter” or “Number”), informing participants of which task was to be completed for a given trial.

We have updated our description of the study procedure (pg. xx) to clarify this point.  
  
**Comment 5:** It is also desirable if the exact timing of the trial would be specified. Even though, in this study, the CSI was not manipulated, the indication of how long “the prompt” (or “the cue”) and the test stimulus were presented, as well as the temporal sequence of the cue and the test stimulus, would contribute to the comparability of this study with other task-switching studies.   
  
***Response:*** [WORDS HERE]

**Comment 6:** With regard to the results, the authors should be more cautious, when interpreting the dissociation between local and global switch costs as a function of the presentation sequence. The difference in switch costs is for the greater part driven by higher RTs in non-switch alternating runs trials. This supports the idea of tracking the trial sequence, which puts an additional load on working memory and leads to higher global costs, particularly in alternating runs paradigm. However, longer RTs in these trials also lead to smaller local switch costs in alternating runs, which restricts the interpretation that larger switch costs in random switching are due to more difficulty to reconfigure task-sets on switch trials. As a support for this interpretation, one would expect significant differences in switch trials between the two presentation sequences, which was not the case. Since the RT difference is only descriptive, I think, that this limitation is worth mentioning and discussing.

***Response:*** This is a fair point. We now discuss this potential limitation in the General Discussion (pg. XX).  
  
**Comment 7:** In addition, with regard to the Vincentized RT distribution for local switching costs as illustrated in Figure 2 and Figure 1. Did the authors use cumulative distribution functions for illustrating the vincentized RT distributions or not. If yes, then it is not clear to me how the RTs in the 6th bin (and perhaps in the 5th bin) in Figure 2 can be lower than the RTs in the bins 1-4. In case of cumulative distributions functions the RTs should be definitively longer in larger bins than in smaller bins. Otherwise the authors should include a reminder that the current analyses are not based on the common way of analyzing vincentized RT distributions because those usually are based on using cumulative data.

***Response:*** [WORDS HERE]  
  
**Minor comments:**

**Comment 8:** p. 6 (lines 42-50): When explaining the CVOE task, the authors argue that it can be presented in pure and in switch block which allows the computation of the local and global switch costs. This passage sounds like a special advantage and therefore an argument for using this task. As far as I know, this possibility is not that much limited to the CVOE task: there are very many other tasks, that can be used in both pure and in switch blocks and allow the computation of different types of costs.

***Response:*** You are correct in that this is not an advantage that is inherently unique to the CVOE task, as these costs can be computed using any task which presents participants with both pure and switch blocks. We have updated this section accordingly and now reference other switch-tasks which similarly allow for pure block-switch block comparisons (pg. xx).

**Comment 9:** p.20 (lines 45-54): The paragraph is difficult to understand, even after repeated reading. The authors explain the finding that random switching increased local costs by referring to the study of Huff et al. (2015). Did they accidentally mention only the alternating runs condition? They further explain that younger adults produced larger local switch costs than older adults and why, and close the paragraph with the conclusion that local costs in random switching become exaggerated relative to predictive switching etc. The logic of this paragraph needs to be checked.

***Response:*** [WORDS HERE]

**Comment 10:** p.22 (lines 26-29): The authors discuss why a decrease in local switch costs across bins found in their study is in contrast to the results of Huff et al. (2015) who found an increase in these costs. They discuss the inclusion of additional trials and potential fatigue effects as one reason for this discrepancy in the results. This interpretation is not really plausible to me, since the occurrence of fatigue effects is usually interpreted as causing more lapses of attention, which should lead to longer RTs in the longest RT task trials. Here the authors seem to argue the opposite. At this point I would like additionally remind the discussion of the DeJong account (2000) for interpreting residual switch costs.

As an alternative, it might be possible that the decreasing size of local switch costs in the current study is caused by learning effects. Since the authors have presented twice as much trials as the study of Huff et al. it might be the cause that the relative and absolute number of switch trials was far higher in their study than in the study of Huff et al. This might have changed the state of preparation for a switching trial, which might be far higher in the current study than in the Huff study. I am not sure what may be the reason for the discrepancy in findings in these studies. However, the author should come up with a more plausible interpretation than the current one.

***Response:*** [WORDS HERE]

All other minor grammatical and spelling errors have been corrected. We thank you for taking the time to review our manuscript.

(4)    The authors use the term task-set inconsistently, sometimes with and sometimes without the hyphen.

(5)    p. 17, lines 33-36: The sentence is incorrect