

**Author's response:** Dear Editor, we sincerely appreciate all of your valuable comments.

Dear Marcel Martončík,

Thank you for submitting the revised version of your Stage 2 manuscript to PCI RR. Most of the previous comments have been addressed satisfactorily. However, there are some small remaining issues that we ask you to further consider and address.

To make the first deviation (i.e., using the percentage of correct trials instead of the total number of correct trials as operationalization of decision-making) transparent, please explain the nature of, and the reason for this deviation, and the potential effects it has on the results in the main text where you mention this measure - essentially, the information that you have provided in the response letter.

**Author's response:** We have added the following Footnote "*We have changed the exploratory operationalization of decision-making from using the total number to using percentages. This adjustment has not affected the results but has made data processing easier, as the results were provided as part of the Psytoolkit output in this format.*" in the description of speed of decision making in the Methods section.

The newly added justifications for using certain operationalizations for attention, speed of decision making and reaction time may be informative. However, since there are strict limits on permissible changes in approved content between Stage 1 and Stage 2, and that the newly added information does not appear to be essential, please remove them.

**Author's response:** Justifications were removed.

In Data quality checks, "After inspecting the data, we excluded participants who reported practice time higher than 168 hours per week.". This sentence will need to be removed, since the results with this exclusion criterion are now presented in the exploratory analyses section instead.

**Author's response:** We apologize for the mistake. Sentence was removed.

The omega coefficients for the practice and deliberate practice items may be useful to the readers as well, please consider adding them into the manuscript, for instance in Table 3.

**Author's response:** We added omega total coefficients to Table 3.

Similarly, it is reassuring to know that the regressions do not suffer from potential issues of multicollinearity. Again, such information may be useful to the readers. Please consider adding this information into the online Supplemental Materials, and briefly refer to it in the exploratory analyses section.

**Author's response:** We have included the following sentences in the Exploratory analyses part of the Results section: *We have tested all four models for multicollinearity. VIF coefficients and correlograms (available in supplementary material: <https://osf.io/2ptqf>) do not point to a multicollinearity problem.*

Table 4: Thank you for providing the simple correlations between all variables. In addition to the point estimate, could you also add the confidence interval for each correlation? I think such information on the uncertainty of the estimates will also be useful.

**Author's response:** Confidence intervals are now presented alongside correlation coefficients.

We kindly bring to your attention that there are minor discrepancies between this manuscript and the one from a previous revision, specifically in Table 4 in the correlation coefficients. This was an accidental oversight during the coding process for which we sincerely apologize. In the previous revision, while providing the correlation matrix, we unintentionally omitted the "complete.obs" argument in `cor_matrix <- cor(as.matrix(selected_data))` on row 625, thereby accidentally including observations with missing values. We would like to apologize for this mistake and appreciate your understanding.

For the abstract, "In both esports, we found evidence for deliberate practice not having a meaningful effect ( $r > .3$  and  $r > .2$ , respectively) on performance." This sentence is a bit ambiguous. From the abstract alone, it is not entirely clear whether the observed correlations were larger than .3 and .2, yet still deemed not meaningful, or that the thresholds for meaningful effect sizes were set at .3 and .2. I think it would be clearer if you could provide both the observed effect sizes, and clearly specify .3 and .2 as what you mean by a meaningful effect. Relatedly, please also consider adding effect sizes for this finding in the abstract. "Additionally, we were able to confirm two game-specific findings: attention (CSGO) and non-deliberate practice hours (LoL) meaningfully predicted performance in one but not both esports."

**Author's response:** We agree that the current wording of that sentence is confusing. We have modified two sentences in the abstract as follows: "*In both esports, we found evidence for deliberate practice not having a meaningful effect on performance (null:  $r > .3$  in CSGO and  $r > .2$  in LoL; observed: .02 in CSGO and -.01 in LoL). On the other hand, the results confirmed younger age predicting better performance (-.33 and -.22, respectively). Additionally, we were able to confirm two game-specific findings: attention (-.30, CSGO) and non-deliberate practice hours (.26, LoL) meaningfully predicted performance in one but not both esports.*"