March 27th, 2024

Dear Dr. Veli-Matti Karhulahti:

Please find enclosed the revised version of our manuscript, *Applying a Synergistic Mindsets Intervention to an Esports Context* (PCI Registered Reports #364) (word count = 24262). The revised manuscript is 87 pages long and includes one table and four figures.

We are grateful for the thoughtful comments received during Stage 2 of the review process. Your suggestions helped us to improve the overall quality and robustness of our project. We are pleased to submit the revised Stage 2 version of our manuscript for your consideration. In this revised version, all amended sections have been highlighted in green for easy identification. Additionally, we have provided a detailed summary of deviations from the original registration, ensuring transparency and clarity.

We thank you again for considering our work. We look forward to hearing from you and hope you will find this revision appropriate for acceptance in *PCI Registered Reports*.

Sincerely yours,

Gratefully,
Maciej Behnke
Daniël Lakens
Kate Petrova
Patrycja Chwiłkowska
Szymon Jęśko Białek
Maciej Kłoskowski
Wadim Krzyżaniak
Patryk Maciejewski
Lukasz D. Kaczmarek
Kacper Szymański
Jeremy P. Jamieson
James J. Gross

Veli-Matti Karhulahti (Recommender)

#1. As noted by LM, the results section could use some clarifications especially in reporting the hypothesis tests. I personally prefer a structure that clearly reports "H1 = outcome, H2 = outcome etc", as it helps readers to easily locate and assess key findings. That's an option to consider. I also highly support IR's request to add descriptives.

Reply: We aligned the hypothesis with the results across the manuscript, including the Presents Study, Analysis, Results and Design Table. We hope this clarification will help readers find and evaluate critical findings.

As we stated in the response to Reviewer 2 #2, we showed the descriptives (M, SD) for the tested variables and all self-reports in Tables S4 and S5. Furthermore, we plan to make all raw psychophysiological and video data from our study publicly available as an open database, accompanied by an article that offers a detailed description of the dataset. Recognizing the dataset's considerable value and its potential for broad reuse in research, we

believe that a separate paper is warranted to describe and provide an overview of the collected data fully.

Changes in the manuscript: See Analysis section, line numbers 881 - 883. Results section, line numbers 1005:1010, 1026:1037, and Table 1. Design Table.

#2. Regarding the optimality of the created conditions and the limits they set to the intervention, I agree with LM and IR that some further engagement with previous related knowledge would make the discussion even more informative. E.g., to my memory, already the first-ever doctoral dissertation on videogames (Elizabeth Ellen Moulds, 1978) investigated arousal in the competitive gaming situation. It could be useful to review the experimental literature of the past decades in detail to further assess whether the stress response in the present study is unique or echo those in previous studies involving similar conditions.

Reply: In the updated version of the manuscript, we reviewed the findings from the relevant literature in the discussion.

Changes in the manuscript: See Discussion section, line numbers 1139 – 1168, 1245-1252.

#3. JK and IR point out some wordings in the discussion that should be rephrased. I made similar notes and would also suggest removing the first three words of the phrase "For this reason, we observed no effects.." (p. 47). Many other reasons could be too. I would also rethink some wordings in the last sentences: "However, we found the limited impact of SMI on the performance outcomes and challenge/threat affective responses... In sum, we recommend the SMI as a prevention tool." Please reconsider the conclusion of 'limited impact' for inferred no effects. As to the framing "we recommend the SMI", would it carry similar meaning if phrased "the findings lead us to recommend the SMI"? I think it's a small but important difference.

Reply: Thank you for these suggestions; we incorporated them into the manuscript.

Changes in the manuscript: See Conclusion section, line numbers: 1303.

#4. Please add the date July 5 2023 as the time of updating the randomisation plan.

Reply: Added.

Changes in the manuscript: See Supplementary Materials, line number 4874.

#5. Last, I recommend stating all theoretical implications in the discussion, even if briefly, as per the last column of the design table (Theory that could be shown wrong). They're already implicitly in the text, but being explicit would be preferred. In my experience, readers tend to draw their own theoretical inferences if they're not spelled out. You may also add a new results column to the design table; some Stage 2 RRs have it and it's often informative. If you wish to discuss any of the revisions before the next (perhaps final) version, I can be contacted as usual.

Reply: In the revised version of the paper, we explicitly clarified which findings support, do

not support, or are in contrast to the theoretical models. We also added a column with the results.

Changes in the manuscript: See Discussion section, line numbers 1105-1108,1182-1185, 1206-1207, and Table 1. Design Table.

Lee Moore (Reviewer 1)

I enjoyed reading this Stage 2 PCI registered report entitled 'Applying a synergistic mindsets intervention to an esports context'. When reflecting on the criteria and key issues to consider at Stage 2, I felt the authors did a great job with the manuscript. Indeed:
- **A direct url to the approved protocol was included.**
- **The introduction was largely the same as the stage 1 manuscript with only minor changes made and these were appropriate and transparently flagged.**
- **The authors stayed true to their approved Stage 1 protocol and where they deviated this was appropriate, clearly articulated, and well-justified (e.g., additional exploratory analyses).**
- **Additional exploratory analyses were informative, justified, performed appropriately, methodological sound, and clearly distinguished from the pre-registered analyses.**
- **The conclusions aligned to the preregistered aims/hypotheses and were appropriate, evidence-based, and well-communicated.**

Reply: Thank you!

Beyond these points, I just have a series of relatively minor suggestions that I believe could help the authors improve the manuscript further:

#1. Remove (including stress) from the abstract, stress is a process, not an emotion linked to negative affect (note: if the authors are referring to feeling stressed, the wording should better reflect this).

Reply: Clarified.

Changes in the manuscript: See Abstract section, line number: 37-38.

#2. In the methods, it would be good to see a breakdown of gender/sex (i.e., proportion of the sample that were male, female, etc.).
I also felt more information could be added beyond stage 1 details relating to missing data and outliers (e.g., stating the precise number linked to physiological variables).

Reply: As we only invited male gamers, we decided not to include a breakdown of gender/sex. We provided additional details on how many participants completed each phase of the study. Given the extensive volume of variables collected in this study (n=1752), providing detailed information on missing data and outliers for each would be impractical. Therefore, we opted for a more streamlined approach by aggregating these details at the apparatus level. Additionally, we provided the missing data for variables used in the analysis (Table S6).

Changes in the manuscript: See Participants section, line numbers 270-272; Analysis Plan section, line numbers 803 – 810; Supplementary Materials section, line number 4895.

#3. Regarding the results, some proof-reading of the inferential statistics reported is needed (e.g., page 44, paragraph 2), units should be included for relevant variables (e.g., bpm for HR), and the formatting of tables/figures should match the rest of the text (e.g., font style). I also found some of the wording tricky to interpret (e.g., a lack of increased challenge cardiovascular reactivity [TPT reactivity), and the results were generally relatively difficult to follow and link to the hypotheses noted on pages 12 and 13. I would therefore recommend the authors reflect and consider how best to restructure and rewrite the results section so it is as clear to the reader as possible.

Reply: We have incorporated the suggestions from both the Reviewer and the Recommender. We aligned the hypothesis with the results across the manuscript, including the Presents Study section, Analysis, Results, and Design Table. We hope this clarification will help readers evaluate the results. Furthermore, we did not adjust the fonts, as we do not know the journal in which our work will be published. We will adjust the font according to the guidelines of the chosen journal once it is confirmed.

Changes in the manuscript: See Results section, line numbers 995 – 1010, 1027-1037.

#4. The discussion would benefit from being more evidence-based and via the inclusion of more relevant citations/references (e.g., page 48, paragraph 3; page 52, paragraph 2).In particular, I would like to see the authors expand on their discussion regarding the post-intervention test not being high-pressured or stressful enough; what other factors could they have manipulated beyond performance-contingent rewards (see Baumeister and Showers [1986] for some suggestions like performance-contingent punishments, social comparison and evaluation, ego relevance, etc.)? Relatedly, other key discussion points could be elaborated upon (e.g., page 53, paragraph 2, lines 1206-1207). Finally, the writing could be checked to ensure it remains formal and scientific throughout (e.g., avoid colluqualisms like 'don't').

Reply: We have incorporated the suggestions from both the Reviewer and the Recommender. In the revised version of the manuscript, we reviewed the findings from the relevant literature in the discussion and included additional citations/references.

Changes in the manuscript: See Discussion section, line numbers 1139 – 1168, 1245-1252.

Ivan Ropovik (Reviewer 2)
My review of the Stage II report is going to be way shorter than my review of the Stage I report. That is because have done a great job making their research workflow very open, accountably owning the minor mistakes, and being frank about the lack of clear signal from the data in several respects. Due to the fact that the data do not support a more straightforward stylized narrative, I think that the paper would have much harder time in many mainstream journals that are rather "novelty-oriented". In my view, it is always very refreshing to see a study that reads like a report from a real study. Now to the point of what should be the target of my comments:

#1. The data are able to test the authors' proposed hypotheses (or answer the proposed research question) by passing the approved outcome-neutral criteria. In my view, the study is still informative, even though the situational context created to observe and test the target effect did not turn out to be optimal. This should probably be assessed by a substantive reviewer knowledgeable about gaming and affective responses, as it is potentially a limiting factor. Yes, the fact that esports competition may not represent a threat-type situation may be seen as logical, but only now, ex post. Although this aspect has a foul taste of failure, I think it is equally worthy of being published.
In that respect, I think the reader of the study would benefit from seeing the descriptives for the study variables to see how much limiting factor are these floor effects. Please, in the revision, include a table with basic descriptives. I understand that fitting it into the manuscript may be difficult, but there is no reason not to include it in the supplementary materials. I also cannot see the generated file with the analysis outputs – a knitted html, pdf or something of that sort.

Reply: We regret any confusion caused previously. The analysis outputs were initially placed in the folder named "Mplus_code." To improve clarity and intuitiveness, we have renamed this folder to "Mplus_analysis_code_and_outputs" in the updated version of our repository.

We included descriptives for the study variables in the supplementary materials, which, unfortunately, due to our shortcomings, the Reviewer did not find. We also apologize for this inconvenience. The descriptives (*M, SD*) for the study variable are presented in Tables S4 and S5.

Additionally, we are in the process of making all raw psychophysiological and video data from our study publicly available through an open database. This initiative will be complemented by a detailed article dedicated to describing the dataset. Given the dataset's substantial value and its potential applicability across various research questions, we are committed to detailing it in a standalone paper rather than as supplementary material. This forthcoming publication will offer an in-depth overview of the entire dataset collected during our project, ensuring its maximum utility for future research.

Finally, we reformatted our *R* script file into *R* markdown and generated a knitted HTML file with the analysis outputs from *R*.

#2. The introduction, rationale and stated hypotheses (where applicable) are the same as the approved Stage 1 submission. The track changes document is not very clear but I believe it is the case. As a sidenote, I think it okay not to track some low-level edits (like changing to past tense) as it clutters the track changes document a lot.

Reply: Thank you! Due to the lack of clarity of the tracked version, we also included the marked version, which was much clearer.

3. The authors adhered precisely to the registered study procedures. Any unregistered exploratory analyses are justified, methodologically sound, and informative. Yes, they are, IMO. Of course there were several acknowledged deviations. To me, several of the deviations are not really deviations _from the research plan_ at all. But okay, it is great to see that the authors were really very diligent about disclosing any change. I think that the deviations had little chance to jeopardize the integrity of the RR.

Reply: Thank you!

4. The authors' conclusions are justified given the evidence. The authors offer a measured interpretation of the results, the narrative is not overblown. The discussion would, however, benefit from taking more into account the constrains on generality posed by the present design and tightening several too-much-generalizing claims accordingly.

Reply: We toned down some of our claims in the discussion. We also supported our claims with additional relevant citations/references. We hope that the revised version is a more balanced and cautious interpretation of our findings.

Changes in the manuscript: See Discussion section, line numbers: 1111, 1115, 1139-1168, 1303.

#5. I now see that the authors refer to supplementary materials, I just cannot see them on the osf repo.

Reply: We apologize for the misunderstanding. The supplementary materials were included in the folder PCI_RR_Stage2, named PCI_RR_SMI_Esports_SuppMat_Stage2.docx. In the updated version of the manuscript, we included a direct link to the supplementary materials to avoid this inconvenience.

Changes in the manuscript: See line number: 1315.

#6. I tend to agree how the authors resolved unexpected situations with the pre-reg deviations, except for the randomization schedule. Unequal groups would have induced no causal bias whatsoever – only some drop in power if H0 is indeed false. But I agree that the fix is an okay solution, albeit a one to a non-problem.

Reply: Thank you for your understanding.

#7. I do not see how the present data support the claim that SMI is a universal prevention tool.

Reply: We no longer refer to the SMI as the "universal" prevention tool. However, we believe that our findings support recommending the SMI as a prevention tool (but not as a performance-enhancing tool). We found some evidence that it can have important benefits (e.g., changing one's mindsets) and found no evidence that the SMI causes harm. Even small changes resulting from SMI that harness these psychological adaptations may have implications for broader applications to promote long-term mental health.

Changes in the manuscript: See Discussion section, line numbers: 1115.

#8. The model did not fit the data well. Even with very modest N and low magnitudes of interrelationships, the formal test of the model, the chi-square test, had enough teeth to reject the exact fit hypothesis. As you can see, the value of the chi^2 is almost 4 times as large, which is not terrible, but it points to model-data deviations inconsistent with purely random sampling variability. Also, the CFI is not reaching the (rather benevolent) threshold preached by Hu & Bentler (1999). I would say that even the approximate fit is mediocre at best.

Reply: Corrected. We included the Reviewer's suggestions as the additional limitation of our study.

Changes in the manuscript: See Limitations section, line numbers 1282-1292.

Jacob Keech (Reviewer 4)

Well done to the authors for completing this study and submitting a well-written Stage 2 Registered Report. In my view, all necessary information has been adequately described, the analyses have been competently conducted, and the manuscript is well written.

Reply: Thank you!

I have three minor comments which I recommend addressing:

#1. Double check the sentence structure for accuracy where the SMI abbreviation has been used throughout. For example, a "the" is missing in this sentence: "we adapted and validated SMI in real-world performance".

Reply: Corrected.

#2. For the measure of Situational Affect Regulation, is there any indication of validity when using half of the items from the original scale?

Reply: The validity of using half of the items from the original scale had not been tested prior to our study. We reduced the number of items to minimize participants' burden and maintain engagement, as it was only a manipulation check in our study. Leveraging the extensive experience of our team members in measuring affect regulation, we (MB, KP, JG) carefully selected the items to ensure they were representative of each factor. In the specific context of our study, we determined that reducing the scale from 2 to 1 item per factor was a pragmatic decision despite recognizing it as a less-than-ideal solution that may have compromised the scale's reliability and validity to some extent. We clarified this issue.

Changes in the manuscript: See Measures section, line number 653-654.

#3. Line 2068: This phrase seems to overstep the results "that the SMI is the universal prevention tool that is recommended." The data suggest that the intervention may be promising, but there is no evidence yet that it is effective for universal prevention.

Reply: We toned down some narratives in the discussion, and we no longer refer to the SMI as the "universal" prevention tool.

Changes in the manuscript: See Discussion section, line number 1115.