

Responses to Reviewer #1 Round 2

We sincerely thank the reviewer for the thoughtful and constructive feedback, which has helped us to improve the clarity and overall quality of the manuscript. Below, we provide a detailed, point-by-point response to each comment. For ease of reading, the reviewer's comments are presented in black, and our responses are shown in blue.

Comments of Reviewer # 1

The revised manuscript is improved and the authors have taken several of the earlier concerns seriously. The introduction is stronger than before and now opens with a clearer paragraph on statistical anxiety, transferable competencies, and the relevance of SDG 4.7, and the background is more coherently organized around emotions in mathematics/statistics, PBL, and the rationale for the second-order model. The paper also now includes a fuller description of enrolment, participation, degree-program breakdown, age categories, and first-enrolment status in the Methods, along with the questionnaire in an appendix and additional reliability/validity material in the appendices. These are real improvements and they make the manuscript more transparent than in the previous round. The authors have also added explicit reporting of bootstrap confidence intervals, R^2 values, and an f^2 effect-size estimate for the second-order path model, as well as a brief sensitivity analysis for the main path coefficient.

That said, some of the central concerns remain only partially resolved.

First, while the sample description is fuller, the Methods still do not provide a genuine sample-size/power justification for SEM, and the rationale for choosing PLS-SEM over covariance-based SEM remains rather formulaic and generic ("small sample size and non-normal data") rather than being properly defended for this specific design.

Response

We thank the reviewer for this constructive comment. In response, we have substantially revised the Methods section to provide a more rigorous and context-specific justification for both the use of PLS-SEM and the adequacy of the sample size.

First, we have refined the rationale for selecting PLS-SEM by explicitly linking it to the characteristics of the present study, including its predictive and exploratory objective, the use of higher-order constructs, the nature of the data (self-reported measures with deviations from normality), and the complexity of the model. These aspects are now clearly connected to the methodological advantages of PLS-SEM, avoiding generic justifications.

Second, we have incorporated a formal sample size and statistical power assessment using the inverse square root method (Kock and Hadaya, 2018). Based on the most complex part of the model (two predictors), and assuming a significance level of 0.05, a statistical power of 0.80, and a medium effect size (0.21-0.30), the minimum required sample size is 69 observations (Hair et al., 2022; Exhibit 1.7, p. 27). Therefore, the

final sample of 82 students exceeds this threshold and is sufficient to support stable and statistically meaningful estimation of the proposed model.

Finally, we have explicitly connected the sample size justification with the choice of PLS-SEM, clarifying that the method is particularly suitable given both the model complexity and the available sample.

References:

Kock, N., & Hadaya, P. (2018). Minimum sample size estimation in PLS-SEM: The inverse square root and gamma-exponential methods. *Information Systems Journal*, 28(1), 227-261.

Hair Jr, J. F.; Hult, G. T. M.; Ringle, C. M.; Sarstedt, M. *Partial Least Squares Structural Equation Modeling (PLS-SEM)*, 3rd ed.; SAGE: 994 Thousand Oaks, CA, 2022, pp. 24-27.

The manuscript also still does not report model identification checks in a way that would reassure a methodologically demanding reader, and the handling of missing data is only indirectly visible through the descriptive appendix rather than being explicitly addressed in the main text.

Response:

In response to your constructive comment, we have strengthened the reporting of model identification and validity assessment in the main text.

Specifically, we now explicitly describe the key conditions for model identification, including the reflective specification of constructs, the use of at least two indicators per construct, and the statistical significance and expected direction of indicator loadings. We also report the assessment of internal consistency, convergent validity, and discriminant validity, together with multicollinearity diagnostics (VIF) and the use of bootstrapping procedures to ensure the robustness of the estimates.

Furthermore, the R^2 values obtained for the endogenous constructs ($R^2 = 0.210$, $R^2 = 0.165$, $R^2 = 0.205$, and $R^2 = 0.269$) indicate an acceptable, albeit moderate, level of explanatory power, which is common in social science research. The statistical significance of the path coefficients further supports the proposed hypotheses.

Regarding missing data, we now explicitly state in the main text that no missing values were observed in the dataset; therefore, no imputation procedures were required. This clarification ensures transparency in the data handling process.

A further unresolved issue concerns for stronger evidentiary support and interpretive caution around the cluster analysis. The authors have been explicit that clustering rests on quantitative constructs only, which is preferable to implying a qualitative dimension that is not there, but

the cluster interpretation remains somewhat overextended. Statements such as Group 1 “may face challenges in emotionally connecting with PBL” and may need adapted intervention are plausible, yet they remain inferential extensions from quantitative profiles rather than directly observed explanations. Since no qualitative reflections or open-response material are included, these claims should be phrased more cautiously.

Response

We thank the reviewer for this insightful and constructive comment. We fully agree that the interpretation of cluster results should remain cautious, particularly when based exclusively on quantitative constructs.

In response, we have carefully revised the section reporting the cluster analysis to adopt a more descriptive and less interpretive tone. Specifically, we have removed statements that could be understood as implying underlying psychological processes or students’ subjective experiences (e.g., references to students “facing challenges” or “perceiving” the methodology in a certain way), as well as direct recommendations for targeted interventions derived from the cluster profiles.

The revised text now focuses on describing the quantitative differences between groups in terms of Positive Emotions and PBL Engagement, avoiding inferential extensions beyond the observed data. In addition, we have explicitly clarified that these patterns should be interpreted with caution, as no qualitative data or open-response material were collected to support explanatory interpretations.

Likewise, while the discussion now links findings more explicitly to achievement emotions theory, PBL literature, and ESD competencies, the mechanistic explanations are still somewhat broad.

Response

We thank the reviewer for this helpful observation. We agree that, in the previous version, some of the mechanistic explanations remained relatively broad.

In response, we have revised the discussion to provide more precise and context-specific interpretations of the observed relationships. In particular, we now explicitly link positive emotions to the key dimensions of the model—namely perceived usefulness of PBL, teamwork dynamics, and communication with the instructor—thereby grounding the explanation of engagement in the empirical structure of the study.

We have also refined the wording throughout the section to avoid general statements and instead interpret the findings in relation to these specific dimensions. In addition, we have incorporated an explicit clarification that these relationships are discussed in connection with the constructs examined in this study, rather than at a general level.

At the same time, we have carefully avoided causal overinterpretation by reformulating the language in terms of associations rather than effects, ensuring consistency with the scope and limitations of the data.

The paper is strongest when it shows that post-PBL positive emotions are more strongly associated with engagement than pre-PBL emotions; it is weaker when it moves quickly from those associations to broad pedagogical recommendations without adequately ruling out alternative explanations such as self-selection, prior ability, or degree-program differences.

Response

We agree that the interpretation of the relationships between positive emotions and engagement should be made with caution, particularly when considering alternative explanations.

In response, we have strengthened the discussion of limitations by explicitly acknowledging the potential influence of prior academic ability and degree-program differences, and by clarifying that the observed patterns should be interpreted as indicative rather than causal.

In addition, we now explicitly recognize that self-selection effects cannot be ruled out, as participation in the activity was voluntary and may be associated with students' initial motivation or attitudes.

Furthermore, we have revised the discussion to avoid moving directly from empirical associations to broad pedagogical recommendations. Instead, any implications are now presented in a more cautious and appropriately qualified manner.

The authors do acknowledge the likely role of prior academic preparation and the absence of direct prior-achievement measures, which is a useful revision, but this remains a limitation that continues to matter for the interpretation of the degree-program pattern.

Response

We agree that, although the role of prior academic preparation is acknowledged in the manuscript, it remains a relevant limitation for interpreting the observed differences across degree programs.

In response, we have revised the manuscript to further emphasize this limitation. Specifically, we now make more explicit that, since prior academic performance was not directly measured, it is not possible to disentangle whether the observed differences between degree programs reflect the effect of the program itself or pre-existing differences in students' academic profiles.

We have also strengthened the interpretation of this result by explicitly stating that the degree-program pattern should be understood as indicative rather than causal.

There are still minor language problems that would benefit from another round of careful editing.

Response

We thank the reviewer for this observation. The manuscript has been carefully revised to improve clarity, grammar, and overall language quality. An additional round of thorough proofreading has been conducted to correct minor issues and enhance readability throughout the text.

The recommendation section is more prioritized than before, but it still does not fully match the more operational structure suggested in the previous review (specific actors, timelines, and direct links to particular empirical findings).

Response

We thank the reviewer for this helpful suggestion. In response, we have revised this section to make its operational structure more clearly visible. Specifically, for each recommendation we now explicitly identify: (i) the relevant actor (e.g., course instructors), (ii) the stage of implementation (e.g., at the beginning of the course, throughout the PBL activity, or after completion), and (iii) the direct link to the empirical findings of the study (e.g., associations between pre- and post-learning emotions and perceived utility, teamwork, and communication).

Importantly, these revisions do not introduce new content, but rather clarify and make explicit the connections between the proposed actions and the empirical results.

We hope that the revisions made adequately address the reviewers' comments and concerns, and that the manuscript is now suitable for publication. We sincerely appreciate the time and effort invested by the reviewers and the editor in improving the quality of this work.