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Comparison of the Effects of Gamification and Traditional Educational Methods on Students' Basic Psychological Needs and Prosocial and Antisocial Behaviors in Physical Education Classes

Jaafar. Ghadeer Abbas[1], Behzad. Behzadnia[1*], Mahta. Eskandarnejad[1]

[1] Department of Motor Behavior, Faculty of Physical Education and Sport Sciences, University of Tabriz, Tabriz, Iran

*** Corresponding author email address**: behzadnia@tabrizu.ac.ir

Editor	Reviewers
Thseen Nazir Professor of Psychology and Counseling Department, Ibn Haldun University, Istanbul, Turkey thseen.nazir@ihu.edu.tr	**Reviewer 1:** Mohammadreza Zarbakhsh Bahri Associate Professor ،Department of Psychology, Tonekabon Branch, Islamic Azad University, Tonekabon, Iran. Email: M.Zarbakhsh@Toniau.ac.ir **Reviewer 2:** Seyed Ali Darbani Assistant Professor, Department of Psychology and Counseling, South Tehran Branch, Islamic Azad University, Tehran, Iran. Email: Ali.darbani@iau.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

The final two paragraphs of the Introduction identify several useful research gaps, but the novelty argument should be more systematic. For example, the manuscript states that “previous studies have mainly focused on the satisfaction of basic psychological needs while neglecting need frustration” and later states that “no study has examined the effects of gamification on both prosocial and antisocial behaviors among junior high school students.” These claims are central to the manuscript’s contribution and should be organized into a clearly articulated gap statement addressing at least four issues: limited examination of need frustration, limited simultaneous assessment of psychological needs and moral/social behavior, limited direct comparison with traditional instruction, and limited evidence from Iranian female adolescents. The final paragraph should then derive the study hypotheses logically from these gaps rather than moving directly to the aim.

In Section 2.1, the sampling procedure requires clearer methodological justification. The manuscript states that “two seventh-grade classes were selected using convenience sampling from one of the schools” and that the two intact classes were subsequently assigned to the experimental and control conditions. This means that randomization occurred at the class level rather than the individual level, and there were only two clusters. The design should therefore not be described in a manner that

could imply individual random assignment. More importantly, students were nested within classes, while treatment condition was completely confounded with class membership. Any pre-existing class-level differences—peer climate, academic characteristics, social cohesion, or classroom history—could consequently be indistinguishable from the treatment effect. This substantial design limitation should be acknowledged explicitly in the Methods and Limitations sections.

The participant flow reported in Section 2.1 should be presented more transparently. The manuscript indicates initial group sizes of 34 and 29, followed by exclusion of one experimental participant and two control participants because they did not complete the posttest, and subsequent exclusion of two additional control participants as "outliers," resulting in final samples of 33 and 25. The authors should provide the exact statistical criterion used to identify the two outliers, specify which variables were affected, clarify whether the exclusion criterion was determined a priori, and report whether conclusions changed when these observations were retained. Post hoc removal of observations without an explicit objective criterion can alter inferential outcomes and potentially introduce analytic bias. A participant-flow diagram and sensitivity analysis would strengthen transparency.

The description of the Basic Needs in Gaming Scale raises an important measurement-validity issue. The manuscript states that a scale designed for "gaming contexts" was used to assess psychological need satisfaction and frustration in gamified physical education. However, the control group received traditional instruction rather than a gaming experience. The authors should explain how items were worded and whether the scale was adapted so that questions were meaningful and equivalent for students in both conditions. If gaming-specific terminology remained in the questionnaire, measurement comparability between the experimental and control groups could be compromised. The manuscript should therefore report the adaptation and translation procedures, content-validity assessment, and evidence that the instrument appropriately captured psychological needs in both gamified and traditional physical education contexts.

The internal-consistency estimates reported for the Basic Needs in Gaming Scale require more cautious interpretation. Cronbach's alpha values of 0.61 for autonomy satisfaction and 0.60 for autonomy frustration are characterized as "acceptable to good internal consistency," but coefficients close to .60 generally indicate relatively weak reliability, particularly when these subscale scores are subsequently used as dependent variables in multivariate inferential analyses. Although the small number of items per subscale can depress alpha, this issue should nevertheless be discussed. The authors should preferably report McDonald's omega or another reliability estimate suited to short scales, alongside item-level information. The potential attenuation of associations and reduced statistical power resulting from limited reliability should also be acknowledged when interpreting the nonsignificant autonomy findings.

The authors should strengthen the Results by reporting confidence intervals and interpreting effect sizes rather than relying predominantly on p values. For example, significant Time × Group effects are reported for competence satisfaction ($\eta^2 = .144$), relatedness satisfaction ($\eta^2 = .087$), prosocial behavior toward teammates ($\eta^2 = .068$), and antisocial behavior toward opponents ($\eta^2 = .091$). These effect sizes are scientifically informative because they indicate that the magnitude of the observed effects differs across outcomes. Confidence intervals around mean changes and between-group contrasts would provide information about precision and practical importance. Given the modest sample size, this is particularly important because nonsignificant findings may reflect insufficient precision rather than evidence of no meaningful effect.

The Discussion appropriately links improvements in competence and relatedness to gamification mechanisms, but several explanatory statements should be framed more cautiously because the mediating mechanisms were not directly tested. For example, the manuscript states that "the increase in competence satisfaction is likely the result of incorporating game elements such as meaningful and timely feedback" and attributes improved relatedness to collaboration and group formation. These are plausible theoretical explanations, but no mediation analysis, process measure, manipulation check, or qualitative data were used to determine which game components produced the effects. The Discussion should therefore distinguish clearly between empirical findings and theoretical interpretations by using formulations such as "may be attributable to" or "a plausible explanation is." Future studies could experimentally manipulate individual game elements to establish mechanisms more directly.

Authors uploaded the revised manuscript.

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1.2. Reviewer 2

Reviewer:

In the description of the Prosocial and Antisocial Behavior in Sport Scale, the manuscript states that the Persian version demonstrated acceptable validity, but the reporting should provide more specific psychometric evidence. The authors should indicate the factor structure supported in the Persian validation study, the magnitude of reliability coefficients, and whether the instrument has previously been validated specifically among adolescents or physical education students rather than broader sporting populations. Because prosocial and antisocial behaviors toward teammates and opponents constitute four distinct dependent variables in the current analysis, evidence supporting this four-dimensional structure within a comparable Iranian population would strengthen confidence in the interpretation of the subscale findings.

The Procedure section requires additional information concerning intervention fidelity. The manuscript states that "the teacher received training on the principles and procedures of implementing the gamification approach through a training course supervised by a university expert," but it does not report the duration or content of the training, whether standardized lesson plans were used, whether intervention sessions were observed, or whether adherence to the gamification protocol was assessed. Because the same teacher delivered both conditions, treatment fidelity is especially important. The authors should report whether checklists, observations, session logs, video recordings, or independent ratings were used to verify that gamification components were consistently implemented and that game elements did not inadvertently enter the traditional condition. Without such information, treatment differentiation remains uncertain.

The description of the control condition should be expanded to permit a more rigorous interpretation of the treatment contrast. The manuscript notes that the traditional group received the "skill acquisition approach" and that the research team deliberately developed "diverse and high-quality instructional sessions" to avoid making the control condition artificially monotonous. This is commendable, but insufficient detail is provided concerning teacher behaviors, feedback frequency, student grouping, opportunities for choice, competitive activities, cooperative practice, and motivational strategies in the control condition. Some of these elements may independently influence basic psychological needs and social behavior. A table comparing the experimental and control conditions across instructional dimensions would make it possible to identify which features were unique to gamification rather than simply differences in instructional quality or social organization.

The gamification protocol is one of the strongest components of the manuscript, but its theoretical mapping should be made explicit. The intervention contains narrative, avatars, teams, scoring, levels, challenges, collaboration, and immediate feedback; however, the manuscript does not clearly specify which gamification features were intended to support autonomy, competence, and relatedness. I recommend adding a table mapping each game element to its proposed motivational mechanism. For instance, progressive challenges and immediate feedback may primarily support competence; team membership and collaboration may support relatedness; and avatar choice or meaningful task options may support autonomy. This would be particularly valuable given the pattern of findings: competence and relatedness satisfaction changed significantly, whereas autonomy satisfaction did not. A priori theoretical mapping would allow the results to be interpreted against the intended intervention mechanisms.

The statistical analysis described in Section 2.4 is substantially underreported relative to the complexity of the analysis subsequently presented. The authors state only that normality was assessed and that "repeated-measures multivariate analysis of variance (MANOVA)" was conducted. The Methods should specify the two multivariate models, the within-subject factor (Time), between-subject factor (Group), dependent-variable sets, treatment of missing data, outlier assessment, assumptions of multivariate normality and covariance structure, and effect-size metric. Because there are only two time points, sphericity is automatically satisfied and should not be presented as a substantive assumption if discussed elsewhere. The authors should additionally report whether Box's M test was considered and justify the use of Wilks' lambda given the unequal group sizes.

The Results section should reconsider the interpretation of the autonomy-frustration interaction. The manuscript reports $F(1,56) = 4.008$ with $p = .050$ and then treats this effect as statistically significant. Given that the prespecified significance threshold is $p < .05$, a value reported as exactly .050 does not meet that criterion under a strict interpretation. The authors should report the exact p value to at least three or preferably four decimal places rather than rounding to .050. If the unrounded value is below .05, this should be demonstrated; if it is .050 or above, the result should be described as borderline or nonsignificant.

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This issue matters because reduction in autonomy frustration is subsequently presented as one of the principal positive effects of gamification in the Abstract, Discussion, and Conclusion.

Authors uploaded the revised manuscript.

2. Revised

Editor's decision after revisions: Accepted.
Editor in Chief's decision: Accepted.

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