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Developing a Structural Model of Anxiety and Internet Addiction Through the Mediating Role of Alexithymia Among University Students

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the sampling paragraph stating that “a multistage cluster random sampling procedure was used,” the actual sampling procedure is insufficiently reproducible. The manuscript should explicitly identify which two universities were randomly selected, how the random selection was performed, how many classes were selected within each university, whether classes were sampled proportional to university enrollment, and how students were selected within classes. This issue is particularly important because the final demographic distribution includes students from five university types, whereas the stated procedure indicates that only two universities were selected. This internal inconsistency must be resolved because it directly affects the validity of the claimed probability-sampling design.

In the Participants paragraph reporting “a total of 209 university students were included in the final analysis,” the attrition from the planned sample of 250 to the analyzed sample of 209 should be fully documented. The authors should report the number of questionnaires distributed, returned, excluded for missing responses, excluded for invalid response patterns,

withdrawn, and retained for analysis. A participant-flow statement would strengthen transparency and allow readers to assess the possibility of attrition or nonresponse bias.

In the eligibility paragraph stating that participants reporting “a severe psychiatric or neurocognitive condition that could substantially compromise their ability to understand or accurately respond to the questionnaires were also excluded,” the operationalization of this exclusion criterion requires clarification. Because psychiatric status appears to have been determined exclusively by self-report rather than structured clinical assessment, the wording should avoid implying diagnostic verification. The authors should specify the exact screening question used, whether psychiatric medication use was assessed, how such responses were handled analytically, and whether these exclusion criteria were applied before or after questionnaire completion.

In the Data Collection Tools paragraph describing the “PROMIS Anxiety Short Form,” the exact version and source of the six-item instrument must be identified. PROMIS anxiety instruments exist in several short-form versions and item-bank configurations, and psychometric interpretation depends on the specific form administered. The authors should provide the official form name or version, language of administration, translation/adaptation procedure for Persian use, evidence supporting use of the Persian version, and whether analyses used summed raw scores or PROMIS T-scores. The manuscript presently mentions the possibility of T-score conversion but appears to analyze raw scores, and this should be made explicit.

In the paragraph describing the Problematic Internet Use Questionnaire Short Form (PIUQ-SF-6), the manuscript repeatedly uses the terms “Internet addiction” and “problematic Internet use” interchangeably. The sentence “the PIUQ-SF-6 was used as the operational measure of Internet addiction/problematic Internet use” should be reconsidered because these constructs are not necessarily psychometrically or diagnostically identical. The authors should select one primary construct label consistent with the instrument’s validated purpose and use it consistently throughout the title, abstract, methods, findings, tables, discussion, and conclusion. If “Internet addiction” is retained, a conceptual justification for treating the PIUQ-SF-6 as an indicator of that construct is needed.

In the assumptions paragraph reporting “the Durbin–Watson statistic was 1.92,” the relevance of this diagnostic should be explained. Durbin–Watson is primarily designed to detect serial correlation in ordered observations, particularly time-series or sequential regression data. For cross-sectional student survey data, independence is principally determined by the sampling and clustering structure rather than Durbin–Watson alone. Because students were sampled within classes and universities, the manuscript should instead consider whether observations were nested and whether intraclass dependence could have affected standard errors.

In Table 2, the authors report the total effect of anxiety on Internet addiction as $B = 0.41$ and $\beta = .41$, followed by the direct effect $B = 0.18$ and $\beta = .18$. The equality of standardized and unstandardized coefficients for the total path is unexpected given the reported standard deviations of anxiety (4.27) and Internet addiction (4.83). The unstandardized coefficient implied by $\beta = .41$ would ordinarily differ from .41. The authors should recheck the regression output and ensure that unstandardized B , standardized β , standard error, and t statistics have not been transcribed incorrectly. Similar verification is warranted for all mediation paths.

The reported coefficients in Table 2 and Table 3 also appear mathematically inconsistent. The unstandardized path from anxiety to alexithymia is reported as $a = 1.43$, and the unstandardized path from alexithymia to Internet addiction is reported as $b = 0.16$; their product is approximately 0.229, which corresponds well to the reported indirect effect of 0.23. However, the manuscript compares this unstandardized indirect effect of 0.23 with what is labeled a standardized total effect of .41 and a standardized direct effect of .18. The mediation results should therefore clearly distinguish unstandardized effects from standardized effects and should not combine them in a single decomposition unless all coefficients are on the same metric.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the TAS-20 paragraph stating that “scores of 51 or below represent relatively low alexithymia, scores between 52 and 60 are regarded as borderline, and scores of 61 or higher indicate elevated alexithymia,” the manuscript introduces diagnostic-style cutoffs but does not use them in the analysis. The authors should either remove these cutoffs or report the proportion of participants falling into each category and explain their analytic relevance. More importantly, because the mediation model treats alexithymia continuously, interpretation should focus primarily on continuous scores rather than categorical classifications.

In the validity paragraph stating that “content validity was additionally examined by eight experts in clinical psychology and research methodology,” the manuscript should report the actual content validity results for the three retained instruments rather than describing the procedure only. Because the original methodological material included CVR and CVI values for a larger instrument set that contained variables subsequently removed from the article, the revised manuscript should provide only the values for anxiety, alexithymia, and problematic Internet use. In addition, the claim that standardized validated instruments required item-level CVR/CVI reassessment should be justified, especially if no substantive adaptation beyond Persian wording was undertaken.

In the same validity paragraph, the statement that “construct validity was subsequently evaluated through confirmatory factor analysis within the SEM framework” is not supported by the Findings section. No CFA results are currently reported. The authors should provide factor loadings, item-level error variances, construct reliability indices, and model-fit statistics for each instrument or for an integrated measurement model. If CFA was not actually performed, this statement should be removed from the methods. Methodological claims should correspond exactly to analyses reported in the results.

In Table 1 and the accompanying paragraph, the manuscript reports Pearson correlations of $r = .52$ between anxiety and alexithymia, $r = .41$ between anxiety and Internet addiction, and $r = .48$ between alexithymia and Internet addiction. These correlations should be accompanied by exact p values where feasible and preferably 95% confidence intervals. Confidence intervals would communicate estimation precision and are particularly appropriate in a mediation study. The authors should also avoid stating that significant correlations constitute “prerequisites” for mediation, because contemporary mediation analysis does not require every constituent bivariate association or even the total effect to be statistically significant.

In the assumptions paragraph stating that “the Kolmogorov–Smirnov test was nonsignificant... supporting the assumption of approximate normality,” the approach to normality assessment requires refinement. With a sample of 209, inferential normality tests should not be treated as definitive evidence of distributional adequacy. The reported skewness and kurtosis values provide more informative evidence, and graphical assessment of residuals would be preferable for regression assumptions. Moreover, mediation inference using nonparametric bootstrap confidence intervals is relatively robust to non-normality of the indirect effect. The authors should distinguish normality of observed variables from normality of residuals and from the distribution of indirect effects.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.