Journal Website

Article history:
Received 04 August 2026
Revised 02 November 2026
Accepted 09 November 2026
Initial Publication 10 November 2026
Final Publication 10 May 2027

Journal of Adolescent and Youth Psychological Studies

Open peer-review report

E-ISSN: 2981-2526

Structural Modeling of Academic Boredom Based on Mindfulness and Spiritual Intelligence: The Mediating Roles of Self-Compassion and Metacognitive Awareness in Male Upper-Secondary Students

Hesam. Shamohamadi[1], Katayun. Haddadi[2*]

[1] Department of Psychology, CT.C., Islamic Azad University, Tehran, Iran
[2] Assistant Professor, Department of Psychology, CT.C., Islamic Azad University, Tehran, Iran

*** Corresponding author email address**: drhaddadi@iau.ac.ir

Editor	Reviewers
John S. Carlson Distinguished Professor of the Department of Educational Psychology, Michigan State University, East Lansing, MI, United carlsoj@msu.edu	**Reviewer 1:** Ali Sargolzaie Assistant Professor, Department of Management, Zahedan Branch, Islamic Azad University, Zahedan, Iran. Email: a.sargolzaie@iauzah.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 paragraph introducing spiritual intelligence states that students “who are better able to connect everyday academic activities with meaningful personal goals may be less likely to perceive learning as empty, monotonous, or irrelevant.” This is a theoretically plausible argument, but the proposed direct relationship between spiritual intelligence and academic boredom remains less empirically established than the other paths in the model. The manuscript would benefit from distinguishing clearly between empirically supported relationships and theoretically inferred relationships. In particular, the authors should explicitly identify the spiritual intelligence–academic boredom pathway as a comparatively novel hypothesis and explain why spiritual intelligence is expected to contribute incremental explanatory value beyond mindfulness, self-compassion, and metacognitive awareness.

In the Introduction, the manuscript proposes self-compassion and metacognitive awareness as “simultaneous mediators,” yet the theoretical justification for modeling them as parallel rather than sequential mediators requires further development. It

is plausible, for example, that mindfulness increases self-compassion, which then facilitates more adaptive metacognitive regulation, or that metacognitive awareness influences how students regulate self-critical thoughts. Because the present model assumes parallel mediation, the authors should explain why no directional path was specified between self-compassion and metacognitive awareness and why their covariance is considered sufficient. This is particularly important because the two mediators showed a substantial zero-order correlation and were allowed to covary in the structural model.

The same identification concern applies to metacognitive awareness. The manuscript states that "the two broad domains of knowledge of cognition and regulation of cognition were used" to represent metacognitive awareness in the structural model. A latent variable with only two observed indicators is statistically fragile and may be underidentified without constraints. The authors should report the identification strategy and justify why the eight theoretically meaningful MAI subcomponents were not used as indicators instead. Using the eight subscales might provide a more informative representation of the construct and reduce dependence on a two-indicator latent specification.

In the Self-Compassion Scale description, the manuscript states that the six components were used as indicators of a multidimensional latent construct. However, the manuscript should clarify whether negatively worded dimensions such as self-judgment, isolation, and over-identification were reverse-scored before forming component scores. This is important because the direction of factor loadings and interpretation of the latent self-compassion construct depend on scoring. The authors should also report reliability estimates for the six component scores, not only the total scale, because those component scores are the variables actually used as latent indicators in the SEM.

In the Statistical Analysis section, the manuscript states that "full-information maximum likelihood procedures were used" when assumptions were appropriate and missingness was limited. The Results later indicate that missing data were less than 1.5%. However, the authors should report the actual missing-data diagnostic used to support FIML, including whether the missingness pattern was assessed as MCAR or MAR and whether Little's MCAR test or another diagnostic was conducted. Because FIML assumes that missingness is ignorable conditional on observed information, simply reporting the percentage of missing responses is not sufficient to justify the procedure.

The manuscript reports that "univariate normality was evaluated using skewness and kurtosis values, and multivariate normality and influential observations were examined using Mahalanobis distance and related diagnostics." These analyses should be reported more precisely. Mahalanobis distance is principally an outlier diagnostic and does not itself constitute a direct test of multivariate normality. The authors should indicate whether Mardia's multivariate skewness and kurtosis coefficients were examined and whether maximum likelihood estimation was used under normal-theory assumptions. If meaningful non-normality was present, a robust estimator or bootstrap standard errors should be considered for structural coefficients in addition to bootstrapping the indirect effects.

In the Results, the manuscript reports that "all standardized factor loadings were statistically significant" and provides composite reliability and AVE values, but discriminant validity is not reported. Given the conceptual overlap among mindfulness, self-compassion, spiritual intelligence, and metacognitive awareness, discriminant validity is especially important in this model. The authors should provide evidence such as the Fornell–Larcker criterion, HTMT ratios, or confidence intervals around latent correlations. The correlation between mindfulness and self-compassion is already relatively strong at the observed level, and self-compassion itself contains a mindfulness component, making construct distinctiveness a substantive measurement concern rather than a purely statistical formality.

The manuscript states that "self-compassion and metacognitive awareness were also permitted to covary at the residual level when theoretically and statistically justified." This specification needs much greater transparency. The authors should report whether the residual covariance was specified a priori or added after inspecting modification indices. If it was data-driven, the relevant modification index, expected parameter change, and theoretical rationale should be presented. More broadly, all post hoc model modifications should be disclosed so that readers can distinguish the hypothesized model from any empirically respecified final model.

In Table 4 and the accompanying text, the structural model is described as demonstrating "good overall fit" on the basis of χ^2/df, CFI, TLI, IFI, RMSEA, and SRMR. While the reported indices are indeed favorable, model fit alone does not establish the correctness or uniqueness of the proposed causal structure. The authors should temper statements suggesting that the covariance structure "supported" the hypothesized directional model and emphasize that alternative structurally equivalent models may fit the same cross-sectional covariance matrix. Comparison with at least one theoretically plausible alternative

JAYPS Adolescent and Youth Psychological Studies
E-ISSN: 2981-2526

model—for example, reversing boredom and the mediators or testing a model without direct effects—would substantially strengthen the analysis.

In the mediation results, the manuscript states that significant bootstrap indirect effects "supported partial parallel mediation." Given the cross-sectional design, the terminology should be revised consistently throughout the manuscript. The analysis demonstrates statistically significant indirect associations, but it cannot establish temporal mediation in the causal sense because predictor, mediator, and outcome variables were measured concurrently. The authors themselves acknowledge this issue in the limitations, but causal language remains present in the abstract, results interpretation, discussion, and conclusion. Terms such as "indirect association," "statistical mediation," or "model-consistent indirect effect" would be more defensible.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

In the Methods section, the manuscript states that "a target sample of approximately 450 students was planned" because SEM sample-size requirements depend on model complexity, factor loadings, structural coefficients, and missing data. This explanation is reasonable but remains insufficiently reproducible. The authors should provide a more explicit a priori justification for N = 450, including the anticipated number of freely estimated parameters, expected minimum factor loading or structural effect size, desired statistical power, and acceptable Type I error rate. If a simulation-based or ratio-based criterion was used, this should be specified clearly. Merely describing the final sample as "sufficiently conservative" does not allow readers to evaluate whether the study was adequately powered for the smallest indirect effects.

In the sampling paragraph, the manuscript states that "Tehran was divided into northern, southern, eastern, western, and central geographical areas," after which educational districts, schools, and classes were randomly selected. The multistage cluster procedure requires considerably more detail. The authors should report the number of educational districts selected from each geographical area, the number of schools selected per district, the number of classrooms sampled, and whether probability proportional to size was used. In addition, because students were nested within classes and schools, the authors should address whether intraclass correlations were evaluated. Ignoring clustering may underestimate standard errors and inflate statistical significance if classroom- or school-level dependence is nontrivial.

In the eligibility paragraph, the manuscript excludes students with "a documented severe intellectual disability, major neurodevelopmental condition… active psychotic disorder, or another severe psychiatric or neurological condition." The manuscript should specify how these conditions were identified and by whom. It is unclear whether exclusion was based on school records, parent reports, counselor documentation, or clinical assessment. Because these criteria concern potentially sensitive health information and participants were minors, the screening procedure should be described carefully, including how confidentiality was protected and whether school personnel communicated eligibility without disclosing diagnoses to the research team.

In the data-collection paragraph, the manuscript states that questionnaires were administered collectively in classrooms and that "different questionnaire orders were distributed across classrooms when feasible." The phrase "when feasible" is too vague for a methodological report. The authors should specify whether questionnaire order was systematically counterbalanced, randomized, or merely varied informally. If multiple orders were used, the number of versions and allocation procedure should be reported. This is important because the full questionnaire battery is relatively long, especially given the 52-item metacognitive inventory, and fatigue or order effects could plausibly influence later responses.

In the Measures section, the manuscript states that academic boredom was assessed using "the 10-item Academic Boredom Survey Instrument (ABSI) derived from the work of Sharp et al. (2021)" and that the Iranian validation supported a single-factor structure. The authors should clarify whether the 10-item scale used here is an officially defined short form of the ABSI, a culturally adapted derivative, or a separate questionnaire validated in Iran using items originating from the ABSI framework.

JAYPS Adolescent and Youth Psychological Studies
E-ISSN: 2981-2526

The wording “derived from” creates ambiguity regarding scale provenance. The exact item-selection procedure, scoring range, any reverse-scored items, and evidence that the same 10 items were used in the Iranian validation should be stated explicitly.

In the description of mindfulness measurement, the manuscript notes that the Persian MAAS-A supported a two-factor structure and states that “the total score was used for descriptive and correlational analyses, while its validated dimensions could be incorporated as indicators of the mindfulness construct in the measurement model.” This wording appears tentative despite the Results section subsequently confirming that the two dimensions were used as indicators. The Methods should state the planned measurement specification definitively rather than retrospectively. Moreover, a latent construct represented by only two indicators requires additional identification constraints; the authors should explain how this factor was identified and whether error variances, loadings, or factor variance were fixed.

Authors uploaded the revised manuscript.

2. Revised

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

JAYPS Adolescent and Youth Psychological Studies
E-ISSN: 2981-2526