

Developing an Explanatory Model of Cyberostracism and Psychological Adjustment Among Adolescents

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E d i t o r

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R e v i e w e r s

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the Methods paragraph stating that “A total of 486 adolescents participated in the study,” the sample size requires a more rigorous justification. The current statement that the sample exceeded commonly recommended minimum requirements for SEM is too general for a high-quality methodological report. The authors should indicate whether an a priori power analysis, Monte Carlo simulation, anticipated parameter-to-sample ratio, or another formal approach was used. They should also specify the expected effect size, alpha level, desired power, and approximate number of freely estimated parameters considered in determining the adequacy of $N = 486$.

The participant recruitment paragraph reports that adolescents were recruited from Kuala Lumpur, Selangor, Johor, and Penang using multistage cluster sampling. The manuscript should provide substantially more detail about the sampling frame and implementation. Specifically, the authors should report the number of schools selected in each region, the number of classes sampled per school, school selection criteria, participation rates, and the number of students initially approached. Because participants are nested within classrooms and schools, the authors should also discuss whether clustering was considered

analytically. Ignoring non-independence may underestimate standard errors in SEM if intraclass correlations are non-negligible.

The inclusion criteria include “regular access to the Internet or social networking platforms during the preceding six months,” but the operational definition of “regular access” is not specified. This criterion should be made reproducible by indicating a minimum frequency or duration of Internet/social-media use. In addition, the authors should justify why six months was selected as the relevant exposure period. If no threshold was imposed in practice, this criterion should be revised to avoid implying a level of standardization that was not actually applied.

The Methods section states that the Cyberostracism Scale was translated and back-translated into Malay and reviewed by experts. This process requires more precise reporting. The authors should specify how many bilingual translators and experts participated, whether translators were independent, how discrepancies were reconciled, and whether semantic, conceptual, and cultural equivalence were formally evaluated. It would also strengthen the manuscript to report findings from the pilot administration, including pilot sample size and whether any items were modified after cognitive or linguistic review.

The structural model in Table 3 presents both the path “Cyberostracism → Psychological adjustment, $\beta = -.62$ ” and simultaneous paths from direct exclusion, indirect exclusion, and being ignored to psychological adjustment. This specification raises a serious conceptual and statistical issue. If the three dimensions are indicators of the higher-order cyberostracism construct, including direct paths from both the higher-order factor and its constituent dimensions to the same outcome may produce interpretational confounding and may not represent a conventional second-order SEM. The authors should clearly explain the model specification, provide the path diagram as estimated in AMOS, and justify how these effects can be interpreted independently. Alternatively, the overall higher-order model and the dimension-specific model should be estimated and reported as separate structural models.

The manuscript reports that the final model explained “46% of the variance in psychological adjustment, $R^2 = .46$,” which is potentially important, but the discussion characterizes this as “substantial explanatory power” without sufficient qualification. Because R^2 is sample- and model-dependent, the authors should interpret this value cautiously and distinguish explanation from prediction. Cross-sectional SEM does not establish predictive validity in a temporal sense. If the authors wish to make predictive claims, they should provide out-of-sample validation, predictive indices, or longitudinal evidence.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The paragraph on psychological adjustment states that BASE-6 item scores “were reverse-coded before construction of the psychological adjustment latent variable so that higher modeled scores represented more favorable psychological adjustment.” This transformation requires stronger methodological justification. Because the original BASE-6 is designed such that higher scores represent greater distress or maladjustment, reversing its direction can complicate comparison with the established literature and potentially create confusion regarding the meaning of factor loadings and path coefficients. The authors should explain the precise transformation, clarify that only score direction changed, and consider retaining the conventional scoring direction while labeling the latent construct “psychological distress” or “adjustment difficulties.”

The Data Analysis section specifies several conventional cutoffs, including “ χ^2/df below 3.00, CFI and TLI values of approximately .90 or higher, and RMSEA and SRMR values below .08.” The manuscript would be strengthened by treating these values as interpretive guidelines rather than rigid thresholds. More importantly, the authors should state the estimator used in AMOS and justify it given the distributional characteristics of the data. If maximum likelihood estimation was used, multivariate normality should be assessed directly, for example through Mardia’s coefficient, rather than inferred only from univariate skewness and kurtosis.

The Findings section reports that “missing data were minimal and did not exceed 1.4% for any individual study variable,” but the manuscript does not clearly state how these missing observations were handled in the final SEM. The Methods section refers to “an appropriate model-based estimation procedure,” which is too vague. The authors should specify whether full

information maximum likelihood, expectation maximization, multiple imputation, pairwise deletion, or listwise deletion was used. The mechanism of missingness should also be considered, ideally through a formal or descriptive assessment of whether data were plausibly missing completely at random or missing at random.

Table 1 reports correlations among direct exclusion, indirect exclusion, being ignored, and psychological adjustment, while the model simultaneously treats cyberostracism as a second-order latent construct. The manuscript should clarify whether these dimension-level correlations were calculated using observed composite scores or latent factor scores. Additionally, because the correlations among cyberostracism dimensions range from .58 to .64, the authors should provide a formal assessment of discriminant validity, preferably using HTMT ratios and/or the Fornell–Larcker criterion. The statement that correlations are “below levels that would suggest redundancy” is insufficient without direct evidence of discriminant validity.

Table 2 combines first-order construct reliability estimates with a “Cyberostracism second-order construct” showing CR = .91 and AVE = .63, but the procedure used to calculate second-order reliability and AVE is not described. The authors should explain whether these values were based on the loadings of the three first-order factors onto the second-order factor and report the individual second-order standardized loadings and their significance. In addition, confidence intervals around factor loadings or construct reliability estimates would provide a more informative assessment of measurement precision.

Authors uploaded the revised manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.