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The Structural Model of Perfectionism Based on Childhood Trauma: The Mediating Role of Difficulties in Emotion Regulation Among Individuals with Obsessive-Compulsive Personality Disorder

Katayoun. Khademi[1], Maryam. Ghahremani[2*]

[1] Department of Psychology, Ar.C., Islamic Azad University, Arak, Iran
[2] Department of Psychology, Ash.C., Islamic Azad University, Ashtian, Iran

*** Corresponding author email address**: Maryam.ghahremani403@iau.ac.ir

Editor	Reviewers
Izet Pehlić Full professor for Educational sciences, Islamic pedagogical faculty of the University of Zenica, Bosnia and Herzegovina izet.pehlic@unze.ba	**Reviewer 1:** Parvaneh Mohammadkhani Professor, Department of Clinical Psychology, University of Rehabilitation Sciences and Social Health, Tehran, Iran. Email: Pa.mohammadkhani@uswr.ac.ir **Reviewer 2:** Farideh Dokanehi Fard Associate Professor, Counseling Department, Roudehen Branch, Islamic Azad University, Roudehen, Iran. Email: f.dokaneifard@riau.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

The statement in the Methods that "the statistical population comprised individuals with a clinical diagnosis of obsessive-compulsive personality disorder who attended treatment centers in Khorramabad, Iran, during 2024–2025" requires substantially greater diagnostic detail. The manuscript does not explain who established the OCPD diagnosis, whether diagnosis was made by psychiatrists or clinical psychologists, whether DSM-5-TR criteria were applied systematically, or whether a structured/semi-structured personality-disorder interview was used. Because the entire study rests on a clinically defined OCPD sample, diagnostic validity is fundamental. The authors should specify the diagnostic procedure, professional qualifications of diagnosticians, whether diagnoses were independently verified, and whether major differential diagnoses or comorbid personality disorders were assessed.

The manuscript reports that "HTMT values among the focal constructs were well below commonly used thresholds," yet the actual HTMT matrix is not presented. This is insufficient for reproducible assessment of discriminant validity. The authors should report all pairwise HTMT coefficients and state the decision criterion adopted, such as .85 or .90. Ideally, bootstrapped confidence intervals for HTMT should also be considered. Merely stating that the values were acceptable prevents readers from

independently evaluating whether childhood trauma, emotion-regulation difficulties, and perfectionism were empirically distinct.

The reliability results require more nuanced interpretation. Cronbach's alpha and composite reliability values exceed .90 for all three focal constructs, with childhood trauma and emotion-regulation difficulties reaching approximately .95–.96. Although high internal consistency is desirable, coefficients this high can also indicate item redundancy or excessively homogeneous indicators. The authors should avoid presenting these values as unconditionally optimal and should discuss whether construct breadth may have been restricted. Reporting rho_A would also strengthen reliability assessment in a PLS-SEM context.

The structural-model evaluation is incomplete. The manuscript reports path coefficients, VIF values, AVE, HTMT, Q^2, RMS Theta, and SRMR as metrics characteristic of PLS-SEM, but the Results section does not present R^2 values for the endogenous constructs, actual Q^2 values, effect sizes (f^2), or the reported SRMR and RMS Theta statistics. These indices are necessary to assess explanatory power, incremental contribution, predictive relevance, and model adequacy. The authors should provide a dedicated table containing R^2 and adjusted R^2 for emotion-regulation difficulties and perfectionism, f^2 for each predictor, Q^2 for endogenous variables, and the selected global approximate-fit indices together with their interpretive criteria.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The description of the sample contains an important internal inconsistency that should not remain unresolved in the published manuscript. The Methods explicitly state that eligibility required participants to be "age between 23 and 50 years," yet the authors subsequently note that "the source document reports a mean age that is inconsistent with the stated eligibility range; therefore, the mean age was not reproduced." Simply omitting the problematic mean does not resolve the underlying data-quality issue. The authors should return to the raw dataset, verify the age values, report the correct mean, standard deviation, range, and distribution, and explain whether any participants violated the eligibility criterion. If the original dataset is unavailable, this should be acknowledged as a serious limitation rather than silently handled by omission.

The sampling strategy requires stronger justification. The manuscript reports a convenience sample of 196 treatment-seeking individuals, but no a priori sample-size calculation, statistical power analysis, or PLS-SEM-specific minimum sample justification is presented. For a structural model involving multiple latent constructs and indicators, sample adequacy should not be inferred solely from the final number of cases. The authors should provide either an a priori power analysis based on the expected minimum path coefficient and desired power, or a defensible PLS-SEM sample-size calculation, preferably using inverse square root or gamma-exponential methods rather than relying only on the outdated "10-times rule."

The operationalization of childhood trauma appears inconsistent with the stated Childhood Trauma Questionnaire. The Methods correctly describe the CTQ as comprising five core domains—emotional abuse, physical abuse, sexual abuse, emotional neglect, and physical neglect—plus minimization/denial items, yet Figure 1 includes an additional observed indicator labeled "Overprotection." This variable is not part of the standard CTQ structure described in the manuscript. The authors must clarify the origin, instrument, scoring, and psychometric properties of "Overprotection," or remove it from the childhood-trauma latent construct. At present, the measurement model depicted in the figure does not correspond to the instrument described in the Methods.

The measurement specification of perfectionism also needs clarification. The manuscript states that the Multidimensional Perfectionism Scale includes self-oriented, other-oriented, and socially prescribed perfectionism, and Figure 1 models these three dimensions as indicators of a higher-order perfectionism construct. However, the Methods do not explicitly state whether a second-order reflective model was estimated, whether dimension scores or individual items were used as indicators, or how the hierarchical construct was specified in PLS-SEM. The authors should clearly describe the measurement-model architecture, including whether the repeated-indicator, two-stage, or another hierarchical component approach was applied.

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The manuscript states that “the structural results reported in the source document are characteristic of partial least squares structural equation modeling (PLS-SEM)” and that the focal paths were therefore “interpreted within a PLS-SEM framework.” This wording suggests that the authors may not actually have re-estimated the model themselves but instead inferred the analytic method from previously reported indices. That is not sufficient for a primary empirical article. The statistical software used for PLS-SEM, its version, the algorithm settings, weighting scheme, number of bootstrap resamples, treatment of sign changes, and model-estimation procedure should all be reported explicitly. If the current paper is based on extracted coefficients rather than independent analysis of the raw data, this must be made transparent because it materially affects reproducibility.

The use of the Durbin–Watson statistic in the preliminary analysis is not methodologically well justified for this cross-sectional SEM design. The manuscript reports “the Durbin-Watson statistic was 1.964,” but Durbin–Watson is principally designed to assess serial autocorrelation in ordered regression residuals, particularly in time-series contexts, and is not a standard diagnostic for cross-sectional PLS-SEM. The authors should either explain precisely why serial dependence was a plausible concern in these data and what residual sequence was tested, or remove this statistic and instead emphasize relevant SEM diagnostics such as collinearity, measurement quality, predictive relevance, and residual-based approximate fit.

The treatment of normality testing should be reconsidered. The manuscript reports that both Kolmogorov–Smirnov and Shapiro–Wilk tests were significant and then states that PLS-SEM does not require multivariate normality in the same manner as covariance-based SEM. Although the latter statement is broadly defensible, reporting omnibus normality tests without distributional diagnostics offers limited information, particularly because such tests are highly sensitive to sample size. The authors should report skewness and kurtosis for the focal variables and, if bootstrap inference is used, explain why the chosen bootstrap procedure provides appropriate inference under non-normality.

Response: Revised and uploaded the manuscript.

2. Revised

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

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