

Explaining a Model of Obsessive–Compulsive Symptom Severity Based on Perfectionism and Intolerance of Uncertainty, with Emphasis on the Mediating Role of Rumination Among Women Attending Psychological Clinics

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E d i t o r	R e v i e w e r s
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1. Round 1

1.1. Reviewer 1

Reviewer:

The sampling description in the Abstract states that “five active psychological clinics located in different areas of Bandar Abbas were selected, and 50 eligible women were recruited from each clinic.” The manuscript should explain how these five clinics were selected and whether the fixed recruitment of exactly 50 participants per clinic was predetermined or occurred coincidentally. If clinics were themselves selected by convenience, this creates a two-stage convenience sampling process that should be transparently described. The authors should also report the number of individuals approached, the number who declined participation, the number excluded because of incomplete questionnaires, and the final number included in the statistical analyses.

In the first paragraph of the Method section, the authors state that “a sample size of 250 participants was considered adequate for the present study” based primarily on general rules of thumb for structural equation modeling. This justification should be strengthened substantially. Sample-size adequacy in path analysis depends on model complexity, number of estimated

parameters, distributional properties, anticipated effect sizes, estimator, and desired statistical power. The authors should report the actual number of freely estimated parameters in the proposed model and show how the stated 10:1 or 20:1 ratio applies to this specific model. Preferably, an a priori power analysis or Monte Carlo-based justification should be added rather than relying exclusively on historical heuristic recommendations.

The Method section contains an important conceptual inconsistency regarding the population. The manuscript states that the statistical population included “all women attending psychological clinics in Bandar Abbas in 2025,” whereas the inclusion criteria specify that participants “had received a diagnosis of obsessive–compulsive disorder from a psychiatrist or clinical psychologist.” These definitions describe substantially different target populations. The authors should revise the population statement to indicate explicitly that the target population consisted of women with a clinically established diagnosis of OCD who were attending psychological clinics. The diagnostic procedure should also be described, including whether diagnosis was based on routine clinical judgment, DSM-based structured interviewing, review of medical records, or another standardized procedure.

The inclusion criteria paragraph states that participants were required to be “between 25 and 45 years of age to maintain age homogeneity within the sample,” but no theoretical or methodological justification is provided for selecting this particular age interval. The authors should explain why women younger than 25 or older than 45 were excluded and whether this range was selected because of epidemiological considerations, clinic demographics, or an attempt to control developmental heterogeneity. Because restricting the age range affects external validity, the rationale should be scientifically defensible rather than simply presented as a strategy for achieving homogeneity.

The treatment of normality assumptions requires methodological revision. The manuscript states that “Data normality was assessed using the Kolmogorov–Smirnov test” and that normality was established because $p > .05$, while skewness and kurtosis were also within ± 1 . In a sample of 250 participants, significance tests of normality can be overly sensitive and should not serve as the sole or principal basis for assessing distributional assumptions. Moreover, maximum likelihood path analysis requires consideration of multivariate normality rather than only separate univariate distributions. The authors should report multivariate normality diagnostics, such as Mardia’s coefficient or an equivalent index, and explain whether conventional maximum likelihood or a robust/bootstrap procedure was employed if multivariate normality was questionable.

The paragraph assessing “the assumption of independence of errors” through the Durbin–Watson statistic appears to import a diagnostic from ordinary least-squares regression without sufficiently explaining its relevance to the reported path model. Durbin–Watson primarily evaluates serial correlation of residuals in ordered observations and is most commonly relevant in regression involving potentially autocorrelated data. Because the present data appear to be cross-sectional and independently sampled, the authors should explain why this test was considered necessary and which regression equation it was applied to. More importantly for path analysis, the manuscript should address whether residual covariances were specified, whether exogenous predictors were allowed to covary, and whether the covariance structure was theoretically justified.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The Method section does not provide sufficient information on clinically important exclusion criteria. Since the study examines predictors of obsessive–compulsive symptom severity, the authors should clarify whether participants with major psychiatric comorbidities, neurological disorders, substance-use disorders, psychotic disorders, bipolar disorder, or severe depressive episodes were included. Medication use and concurrent psychotherapy are also potentially important because both may influence obsessive–compulsive symptom severity and rumination. Even if these factors were not used as exclusion criteria, they should ideally be recorded and reported as sample characteristics or acknowledged as potential confounders.

The paragraph describing the Obsessive–Compulsive Inventory should verify the precise instrument name and scoring procedure. The manuscript states that the “Obsessive–Compulsive Inventory” contains 18 items and six subscales and cites

Foa et al. (2002); this appears to correspond specifically to the Obsessive–Compulsive Inventory–Revised (OCI-R), rather than the original longer inventory. The exact validated instrument name should be used consistently throughout the manuscript, and the authors should clarify whether the total score or subscale scores were entered into the path model. Because the outcome is repeatedly described as “symptom severity,” the manuscript should also explain whether the OCI-R total score is being interpreted as a global symptom severity index and justify this operationalization.

In the instrument descriptions, Cronbach’s alpha coefficients are presented for all four measures, for example, “In the present study, the reliability of the instrument, assessed using Cronbach’s alpha, was .88.” Reporting internal consistency is appropriate, but reliability evidence should be complemented by information regarding construct validity of the Persian versions in the target cultural context. If confirmatory factor analyses were not conducted in the present sample, this should be stated. Furthermore, because multidimensional instruments such as the perfectionism scale contain theoretically distinct subscales, reporting only an overall alpha may obscure differential reliability across dimensions. The authors should clarify whether total scores were psychometrically justified and ideally provide subscale reliability coefficients where appropriate.

The measurement of perfectionism requires stronger conceptual justification. The manuscript states that the Hewitt and Flett scale assesses “self-oriented perfectionism, other-oriented perfectionism, and socially prescribed perfectionism,” yet the path model uses a single observed variable labeled “Perfectionism.” The authors should explain why these theoretically distinct dimensions were collapsed into one total score. A total score may conceal different or even opposing relationships with obsessive–compulsive symptoms. The analysis would be considerably more informative if the authors either justified the use of a composite score psychometrically or tested the perfectionism dimensions separately, particularly because the Introduction and Discussion frequently emphasize maladaptive perfectionism rather than perfectionism in general.

There is a notable inconsistency in the statistical software versions reported across sections. The Abstract and Results state that “SPSS version 27” was used, whereas the Method section states that “SPSS version 26 and AMOS were used for data analysis.” This discrepancy must be corrected throughout the manuscript. The authors should also specify the version of AMOS employed. Reproducible reporting requires a single, accurate account of all statistical software and versions used in the analyses.

The paragraph beginning “Prior to performing path analysis, univariate outliers were examined using boxplots, while multivariate outliers were identified using Mahalanobis distance” is incomplete from a reproducibility standpoint. The authors should report how many cases were identified as univariate or multivariate outliers, the criterion used for Mahalanobis distance, the associated degrees of freedom and significance threshold, and how many observations were removed. This issue is particularly important because the Abstract reports a sample of 250 participants; if cases were excluded during data screening, the effective analytical sample may have been smaller than 250 and should be reported explicitly.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.