KMAN PUBLISHING INSTITUTE
Journal Website

Article history:
Received 14 December 2025
Revised 04 March 2026
Accepted 12 March 2026
Initial Published 04 June 2026
Final Publication 10 June 2026

Journal of Adolescent and Youth Psychological Studies

Open peer-review report

E-ISSN: 2981-2526

# Development of a Model of Obsessive–Compulsive Symptoms Based on Thought–Action Fusion and Emotion Regulation: The Mediating Role of Intolerance of Uncertainty among University Students in Kermanshah

Sara. Mortazavi[1], Kamran. Yazdanbakhsh[2*]

[1] MA, Department of Clinical Psychology, Ker.C., Islamic Azad University, Kermanshah, Iran
[2] Associate Professor, Department of Psychology, Razi University, Kermanshah, Iran

*** Corresponding author email address**: K.yazdanbakhsh@razi.ac.ir

| Editor | Reviewers |
|---|---|
| Gholamreza Rajabi<br>Professor of Counseling Department, Shahid Chamran University, Ahvaz, Iran<br>rajabireza@scu.ac.ir | **Reviewer 1:** Mehdi Rostami<br>Department of Psychology and Counseling, KMAN Research Institute, Richmond Hill, Ontario, Canada.<br>Email: dr.mrostami@kmanresce.ca<br>**Reviewer 2:** Maasuomeh Behboodi<br>Assistant Professor, Counseling Department, Roudehen Branch, Islamic Azad University, Roudehen, Iran. Email: masomehbehbodi@riau.ac.ir |

## 1. Round 1

### *1.1. Reviewer 1*

Reviewer:

The paragraph defining thought–action fusion (TAF) clearly distinguishes moral and likelihood components. Nevertheless, the sentence “TAF refers to a cognitive bias in which an individual conflates thoughts with actions” would benefit from explicitly clarifying whether TAF is conceptualized here as a belief system, a trait-like cognitive style, or a state-dependent appraisal, as this distinction has implications for model interpretation.

While the discussion of emotion regulation is theoretically sound, the paragraph beginning “In parallel, emotion regulation has become a central explanatory axis in OCD research” would benefit from a clearer justification for focusing exclusively on reappraisal and suppression, rather than broader constructs such as emotion regulation difficulties or emotional avoidance.

In the paragraph starting “IU is another construct increasingly regarded as a cognitive vulnerability for OCD”, intolerance of uncertainty is described as a vulnerability factor. The authors should clarify whether IU is treated as transdiagnostic or OCD-specific within the current model, particularly given the emphasis on student (nonclinical) samples.

The paragraph beginning "From a model-building standpoint, IU is conceptually positioned to operate as a mediator" provides a strong conceptual rationale. However, the authors should explicitly state whether alternative models (e.g., IU as a moderator or antecedent) were theoretically considered and empirically ruled out.

Table 1 is comprehensive, but effect size interpretation is absent. The authors might consider briefly commenting on whether observed mean levels (e.g., IU total score) are low, moderate, or high relative to normative data.

In the paragraph beginning "Measurement model fit entails evaluating the reliability and convergent validity", the authors accept AVE values above 0.30. This threshold is unconventional and should be explicitly justified with methodological references.

The calculation of the Goodness-of-Fit (GoF) index is reported, but the formula itself is not shown. Including the explicit formula would improve reproducibility and transparency.

Figure 1 presents standardized path coefficients but does not indicate significance levels visually (e.g., via asterisks or line thickness). Adding these cues would enhance interpretability.

Authors uploaded the revised manuscript.

### *1.2. Reviewer 2*

Reviewer:

The final sentence of the Introduction clearly states the study aim. Nonetheless, it would be helpful to explicitly mention the analytical approach (PLS-SEM) in this sentence to align the aim more closely with the methodological strategy.

In the paragraph describing sample size calculation using Tabachnick and Fidell (2013), the justification for applying this rule within a PLS-SEM framework is not fully explained. Given that PLS-SEM often relies on different heuristics (e.g., 10-times rule), the authors should clarify why this formula was preferred.

The use of convenience sampling is acknowledged, but the paragraph does not discuss how sampling bias might influence parameter estimates in SEM. A brief reflection on this limitation would improve methodological transparency.

The AVE value reported for obsessive–compulsive symptoms is 0.464, which is below the commonly recommended threshold of 0.50. Although the authors state this is acceptable, a more explicit justification or citation supporting this decision in PLS-SEM contexts is necessary.

The reported Cronbach's alpha for TAF ($\alpha = 0.672$) is relatively low. In the paragraph describing this construct, the authors should clarify whether this reflects cultural adaptation issues, multidimensionality, or item heterogeneity, and whether any items were considered for removal.

In the paragraph describing expressive suppression, the Cronbach's alpha ($\alpha = 0.683$) is marginal. The authors should discuss whether this reliability level may attenuate path coefficients and potentially explain the non-significant direct effect observed later.

The AVE value for IU (0.930) is exceptionally high. The authors should comment on whether this may indicate item redundancy or overly homogeneous indicators, and how this might affect discriminant validity.

The sentence "Because the data distribution was non-normal, the study hypotheses were tested using linear regression within a path-analytic framework in SmartPLS" conflates regression and SEM terminology. The authors should rephrase this to more accurately reflect PLS-SEM estimation procedures.

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