KMAN PUBLISHING INSTITUTE
Journal Website

Article history:
Received 14 July 2026
Revised 21 October 2026
Accepted 27 October 2026
Initial Publication 27 October 2026
Final Publication 01 January 2027

KMAN Counseling & Psychology Nexus

OPEN PEER-REVIEW

E-ISSN: 3041-9026

Structural Modeling of High-Risk Sexual Behaviors Based on Misophonia and Sensation Seeking: The Mediating Role of Childhood Trauma in Individuals with Psychopathic Traits

Parham. Fonoudi[1], Negar. Akbari Zargar[2*], Ali. Farahani[3]

[1] Master's Degree, Department of Psychology, WT.C., Islamic Azad University, Tehran, Iran
[2] Assistant Professor, Department of Psychology, WT.C., Islamic Azad University, Tehran, Iran
[3] Assistant Professor, Department of Psychology, Raja University, Qazvin, Iran

*** Corresponding author email address**: negar_akbari20@yahoo.com

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:** Masoud Asadi Assistant Professor, Department of Psychology and Counseling, Arak University, Arak, Iran. Email: m-asadi@araku.ac.ir **Reviewer 2:** Mahdi Khanjani Associate Professor, Department of Psychology, Allameh Tabataba'i University, Tehran, Iran. Email: khanjani_m@atu.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

The proposed temporal ordering of the mediation model requires reconsideration. Table 3 specifies the pathways "Misophonia → Childhood trauma" and "Sensation seeking → Childhood trauma," yet childhood trauma refers to adverse experiences that occurred during childhood and therefore temporally precedes current adult misophonia symptoms and sensation-seeking scores. The Discussion itself acknowledges this problem in the sentence, "Childhood trauma necessarily precedes current adult misophonia symptoms chronologically." This creates a fundamental inconsistency between the statistical model and the developmental interpretation. The authors should either reverse the theoretically implausible paths and test childhood trauma as an antecedent variable, or provide a compelling theoretical rationale for modeling retrospective trauma scores as a statistical mediator while explicitly avoiding temporal or causal terminology.

Relatedly, the manuscript repeatedly describes childhood trauma as a "mediator," but the study is cross-sectional and all variables were measured concurrently. For example, the Findings state that "childhood trauma functioned as a partial mediator

in both relationships." Statistical indirect effects can be estimated cross-sectionally, but temporal mediation cannot be demonstrated without temporal separation of predictor, mediator, and outcome. The manuscript should consistently use more cautious terminology such as "statistical indirect effect" or "cross-sectional mediation model," and causal expressions such as "through which," "developmental pathway," and "explains how" should be moderated unless supported by longitudinal evidence.

The High-Risk Sexual Behaviors Questionnaire is currently the least adequately documented measure in the manuscript. The text states that it contains 10 items but does not provide the response format, scoring range, dimensions, representative content, interpretation of high versus low scores, or validation characteristics. Because high-risk sexual behavior is the principal outcome of the study, the psychometric and operational description of this instrument should be substantially expanded. The authors should also clarify exactly which behaviors are captured by the scale, such as unprotected intercourse, multiple partners, substance-related sexual activity, or other behaviors, rather than relying on a broad conceptual description that may not correspond to the actual questionnaire items.

The use of the Amsterdam Misophonia Scale requires stronger methodological justification. The manuscript notes that the A-MISO-S consists of six items and was modeled partly on the Yale–Brown Obsessive Compulsive Scale, but it does not address the limitations of using a brief, early-generation measure of misophonia as the principal predictor. The authors should explain whether the Persian version has undergone formal validation in an Iranian population, including evidence for construct validity, reliability, and appropriate scoring. Given recent developments in misophonia assessment, the manuscript should also acknowledge that A-MISO-S scores represent symptom severity rather than a diagnostic determination and should avoid language implying that participants necessarily met criteria for a misophonia disorder.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The sample-size justification is absent and should be added to the Study Design and Participants section. The manuscript reports a final sample of 240 participants but does not explain how this number was determined. Because path analysis was the principal analytic technique, the authors should provide an a priori power analysis, Monte Carlo power analysis, or another defensible sample-size justification based on the number of estimated parameters, anticipated effect sizes, significance level, and desired statistical power. Merely obtaining 240 usable cases after screening 320 individuals does not establish that the sample was adequately powered for the direct and indirect effects examined.

The recruitment procedure requires considerably more detail. The sentence stating that participants "were identified between 2025 and 2026 through psychiatric centers, psychological clinics, specialized counseling centers, and Qazvin Central Prison" combines markedly different recruitment settings. The manuscript should report the number recruited from each setting, explain whether identical screening procedures were applied across sites, and clarify whether prisoners participated under conditions that adequately protected voluntariness. Recruitment source should also be considered analytically because correctional and clinical participants may differ systematically in psychopathy severity, trauma exposure, sexual behavior, substance use, and social circumstances. At minimum, the authors should examine whether recruitment setting is associated with the principal variables or discuss this source of heterogeneity as a limitation.

The ethical reporting is incomplete for a study involving prisoners, sensitive sexual behavior, childhood maltreatment, and psychiatric characteristics. The Methods section states that written informed consent, confidentiality, withdrawal rights, and coding were implemented, but no ethics committee name, approval number, or institutional authorization is reported. The authors should provide the full institutional review board or research ethics committee information and describe safeguards specific to potentially vulnerable participants, particularly prisoners. It should also be clarified whether prison authorities had access to individual responses, whether participation had any bearing on privileges or treatment, and how privacy was maintained during completion of questionnaires.

KMAN-CPN KMAN-Counseling & Psychology Nexus
E-ISSN: 3041-9026

The description of the Childhood Trauma Questionnaire–Short Form should be revised to distinguish clearly between instrument properties reported in previous validation studies and reliability obtained in the current sample. The sentence "Based on the information available for the present study, Cronbach's alpha coefficients for its subscales ranged from .81 to .94" leaves the source of these coefficients ambiguous. The same problem occurs for the other measures. For every instrument, the authors should explicitly report prior psychometric evidence separately from reliability calculated using the present 240 participants. Ideally, current-sample reliability should be provided for each CTQ-SF subscale and, where appropriate, the total score used in the structural model.

The operationalization of childhood trauma in the structural analysis is insufficiently specified. The CTQ-SF comprises emotional abuse, physical abuse, sexual abuse, emotional neglect, and physical neglect, yet Table 1 reports only one aggregate "childhood trauma" score. The authors should state precisely how this total score was calculated, whether all 25 clinical items were summed, and whether the Minimization/Denial items were excluded. More importantly, the justification for collapsing five potentially distinct trauma domains into a single observed variable should be provided. Given that different forms of maltreatment may have different relationships with sexual risk and emotional reactivity, supplementary analyses using the CTQ dimensions could materially strengthen the manuscript.

Response: Revised and uploaded the manuscript.

2. Revised

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

KMAN-CPN
KMAN-Counseling & Psychology Nexus
E-ISSN: 3041-9026