PUBLISHING INSTITUTE
Journal Website

Article history:
Received 30 December 2025
Revised 11 May 2026
Accepted 18 May 2026
Initial Published 24 May 2026
Final Publication 01 November 2026

Psychology of Woman Journal

Open peer-review report

PWJ
Psychology of Woman Journal
Publisher: IEMANPub

E-ISSN: 3041-8615

The Effectiveness of Cognitive Behavioral Therapy on Psychological Flexibility and Suicidal Ideation in Women with Experiences of Spousal Infidelity

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1. Round 1

1.1. Reviewer 1

Reviewer:

The sentence “Psychological flexibility has gained substantial attention in recent years because of its important role in mental health and emotional resilience” introduces the construct appropriately, but the authors do not adequately explain why cognitive behavioral therapy, rather than Acceptance and Commitment Therapy, was selected as the intervention despite psychological flexibility traditionally being associated with ACT literature. This theoretical inconsistency should be explicitly addressed in the Introduction and Discussion sections.

In the paragraph discussing suicidal ideation, the sentence “Women who experience marital infidelity may be especially vulnerable to suicidal ideation” would be stronger if supported by empirical prevalence data or epidemiological statistics. Currently, the argument is largely theoretical. Including quantitative evidence regarding suicide risk among betrayed spouses or women experiencing relational trauma would substantially improve the justification for the study.

The literature review heavily relies on descriptive summaries of previous findings but lacks critical synthesis. For example, the paragraph beginning with "Research findings have consistently demonstrated…" summarizes multiple studies without identifying gaps, contradictions, methodological limitations, or unresolved issues in the literature. The Introduction should move beyond narrative description toward analytical integration of prior evidence.

The authors repeatedly refer to "women with experiences of spousal infidelity," but the operational definition of this phrase is absent. In the Methods section, the manuscript should clearly define how infidelity was identified or verified. Was infidelity self-reported, emotionally perceived, legally confirmed, or clinically assessed? Without operational clarity, the inclusion criteria remain ambiguous.

In the Methods section, the sampling strategy is insufficiently justified. The sentence "Participants were selected through convenience sampling" raises concerns regarding selection bias. The authors should explain how participants were approached, screened, and recruited, and whether any individuals declined participation. Reporting recruitment flow would improve transparency and methodological credibility.

The effect sizes reported in Table 2 (η^2 = .59 and .56) are extremely large for psychosocial interventions involving brief CBT protocols. While large effects are possible, the manuscript should discuss the magnitude of these effects more critically and consider whether small sample size inflation may have contributed to exaggerated effect estimates.

In the Discussion section, the authors repeatedly restate findings before interpretation, which creates redundancy. For example, the paragraph beginning "Another important finding of the present study…" reiterates results already presented in previous paragraphs. The Discussion would benefit from more analytical depth and less repetitive restatement of statistical outcomes.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The sample size of 28 participants is relatively small for ANCOVA analyses involving psychological variables with potentially high variance. The manuscript does not report any a priori power analysis or sample size justification. The authors should calculate and report statistical power to demonstrate whether the study was adequately powered to detect the reported effects.

The randomization procedure is inadequately described. The sentence "randomly assigned into experimental and control groups" lacks methodological detail. The manuscript should specify the randomization method (simple randomization, block randomization, computerized assignment, sealed envelopes, etc.) and clarify who performed the allocation and whether allocation concealment was implemented.

The description of the intervention protocol is too general for replication purposes. In the Intervention section, the authors mention "cognitive restructuring techniques, emotional regulation strategies, problem-solving skills, and behavioral activation exercises," but no session-by-session structure is provided. The article would benefit significantly from a detailed treatment protocol table summarizing session objectives, therapeutic techniques, homework assignments, and targeted cognitive distortions.

The manuscript does not report therapist qualifications or treatment fidelity procedures. It is essential to specify who delivered the intervention, their level of CBT training, years of clinical experience, and whether adherence to the therapeutic protocol was monitored or supervised. Without fidelity assessment, it is difficult to determine whether the intervention was implemented consistently.

The use of the Suicide Ideation Questionnaire requires further clarification. The manuscript states that the instrument contains 13 dichotomous items with a Cronbach's alpha of 0.65. This reliability coefficient is relatively low for clinical research involving suicidality. The authors should discuss the implications of limited internal consistency and justify continued use of the measure despite this limitation.

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The psychometric description of the Psychological Flexibility Questionnaire is incomplete. The manuscript reports Cronbach's alpha values but does not discuss construct validity, factor structure, or cultural adaptation procedures in the Iranian context. Given the cultural sensitivity of psychological constructs, more detailed validation information is necessary.

The Results section lacks sufficient reporting of assumption testing for ANCOVA. The authors state that assumptions were satisfied but do not provide actual statistics for homogeneity of regression slopes, Levene's test, or normality indices. Merely stating that assumptions were met is insufficient for rigorous statistical reporting.

Table 2 contains a potential statistical inconsistency. Under the "Group" source, the degrees of freedom are reported as "2" for ANCOVA, whereas in a two-group design, group df would generally equal 1. The authors should carefully review the ANCOVA table because this issue raises concerns regarding statistical accuracy and reporting validity.

Authors revised the manuscript and uploaded the document.

2. Revised

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

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