


# The Effectiveness of Acceptance and Commitment Therapy on Mood Status in Women with Postpartum Depression


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## 1. Round 1

### 1.1. Reviewer 1

Reviewer:

In the Introduction, the statement “Postpartum depression, whose prevalence has been reported in 5% to 20% of mothers” is too broad and outdated without a nuanced epidemiological discussion. The authors should provide recent global and regional prevalence estimates, particularly from Middle Eastern or Iranian populations, because prevalence rates vary substantially depending on diagnostic criteria, screening tools, and postpartum timing.

The paragraph beginning with “One of the underlying characteristics that can be examined in this regard is mood status” lacks conceptual precision. “Mood status” is repeatedly treated as both a broad emotional construct and a measurable psychometric outcome without defining its theoretical framework. The authors should clarify whether mood status refers to transient affective states, mood dysregulation, or overall emotional functioning, and connect this construct more explicitly to postpartum depression pathology.

The literature review does not sufficiently justify why Acceptance and Commitment Therapy (ACT) was selected over other empirically supported interventions for postpartum depression such as CBT, IPT, mindfulness-based cognitive therapy, or pharmacotherapy. The manuscript would benefit from a dedicated paragraph discussing ACT’s unique mechanisms, especially psychological flexibility, experiential avoidance reduction, and value-based action, within postpartum populations.

The assumptions underlying ANCOVA and MANCOVA are insufficiently reported. Although the Kolmogorov–Smirnov test is mentioned, there is no information regarding homogeneity of regression slopes, equality of covariance matrices (Box’s *M*), or homogeneity of variances (Levene’s test). These assumptions are essential for the validity of covariance analyses and should be explicitly reported.

In the paragraph beginning “According to the results of multivariate analysis of covariance,” the manuscript refers to “Table 5,” although only Tables 4 and 6 are shown. This indicates either a missing table or numbering error. All tables and in-text references should be carefully reviewed for consistency.

The Discussion section relies heavily on general ACT literature but insufficiently interprets findings within the unique psychosocial context of postpartum women. Variables such as maternal role transition, sleep disruption, breastfeeding stress, marital support, hormonal fluctuation, and infant-care burden should be integrated into the interpretation to strengthen clinical relevance.

Authors revised the manuscript and uploaded the document.

## 1.2. Reviewer 2

Reviewer:

The sentence “to date, the effectiveness of acceptance and commitment therapy on variables such as mood status, cognitive emotion regulation, and anxiety sensitivity in women with postpartum depression has not been assessed” overstates the novelty of the study. However, anxiety sensitivity and cognitive emotion regulation were not actually analyzed in the reported findings. This creates a major inconsistency between the study aims and the presented results. The authors should either remove these variables from the study objectives or provide the missing analyses and corresponding measures.

In the Methods section, the manuscript inconsistently reports the study year as both 2023 and 2024. For example, the Methods section refers to “psychological clinics in Tehran during 2023,” while the Procedure section states “in 2024.” This inconsistency raises concerns about data accuracy and must be corrected throughout the manuscript.

The sampling strategy requires further clarification. The authors describe the use of “purposive sampling” followed by random assignment, but the recruitment process remains vague. The manuscript should specify how participants were initially approached, how postpartum depression was screened before enrollment, and whether recruitment occurred through clinician referral, advertisement, or systematic screening procedures.

The inclusion criterion “obtaining a score above 40 on the Beck Depression Inventory” appears unusually high and likely indicates severe depressive symptomatology. The authors should justify this cutoff with references and explain whether women with severe depression, suicidality, or psychiatric comorbidities were clinically evaluated before participation. Ethical considerations surrounding treatment of severe postpartum depression should also be discussed.

The description of the ACT intervention protocol is insufficiently standardized for replication. Although Table 1 summarizes sessions, critical details are missing, including therapist qualifications, treatment fidelity procedures, supervision structure, adherence checks, and whether the protocol was culturally adapted for postpartum Iranian women. These details are essential for intervention reproducibility.

The manuscript reports that sessions lasted 120 minutes weekly for eight weeks, yet no information is provided regarding attendance rates, participant adherence, homework completion, or dropout frequency. Since postpartum women often face childcare and fatigue-related barriers, adherence data are clinically important and should be reported.

The psychometric description of the Profile of Mood States (POMS) contains several inconsistencies. The manuscript states that the short form includes 37 items, yet the listed subscale totals exceed this number when summed. Additionally, the scoring explanation appears internally inconsistent. The authors should carefully revise the instrument description and provide accurate citations for the exact version used.

The statement “the researcher used content validity to determine the validity of the questionnaire” is methodologically vague. Content validity is typically established through expert panel review, CVR/CVI calculations, or adaptation procedures. The authors should explicitly describe how validity was assessed and by whom.

The Results section reports demographic characteristics descriptively, but no statistical comparison between the experimental and control groups at baseline is provided. The manuscript should include baseline equivalence testing for age, education, childbirth order, depression severity, and other relevant variables to ensure randomization effectiveness.

Table 3 presents a highly implausible F statistic ( $F = 6095.615$ ) alongside a relatively modest eta squared value ( $\eta^2 = 0.205$ ). These values appear statistically incompatible and suggest either reporting errors or analytical problems. The ANCOVA calculations should be carefully rechecked, preferably with independent statistical review.

The manuscript repeatedly uses the phrase “mood status significantly decreased,” although lower POMS scores represent improvement rather than deterioration. This wording may confuse readers. The authors should revise the phrasing throughout the paper to clearly indicate “improvement in negative mood symptoms” or “reduction in mood disturbance.”

Authors revised the manuscript and uploaded the document.

## 2. Revised

Editor’s decision: Accepted.

Editor in Chief’s decision: Accepted.