Journal Website

Article history:
Received 26 July 2026
Revised 01 November 2026
Accepted 08 November 2026
Initial Publication 08 November 2026
Final Publication 01 January 2027

KMAN Counseling & Psychology Nexus

OPEN PEER-REVIEW

E-ISSN: 3041-9026

Comparison of the Effectiveness of Emotional Schema Therapy and Dialectical Behavior Therapy on Quality of Life and Metacognitive State in Individuals with Comorbid Depression and Anxiety

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the participant description, the sentence “According to information obtained from the participating psychological counseling centers, approximately 72 individuals meeting these general diagnostic characteristics were available during the study period” raises questions regarding recruitment flow. The manuscript should provide a transparent account of how the 72 potentially eligible individuals were screened, why 12 were not included, whether any declined participation or failed eligibility criteria, and whether all 60 randomized participants completed posttest and follow-up. A participant flow diagram or a detailed textual account would substantially improve methodological transparency.

In the Methods paragraph describing inclusion criteria, the phrase “having a confirmed diagnosis of comorbid depression and anxiety based on the clinical assessment of a psychiatrist and psychologist” is insufficiently specific for a clinical intervention study. The manuscript should identify the diagnostic framework used, such as DSM-5-TR or ICD criteria, specify whether a structured or semi-structured diagnostic interview was administered, and indicate whether diagnostic agreement between the psychiatrist and psychologist was required. Without this information, reproducibility and diagnostic validity cannot be adequately evaluated.

The exclusion criteria listed in the Methods are very limited. In the paragraph stating that exclusion criteria consisted of "unwillingness to continue participation in the study and concurrent use of complementary therapeutic approaches during the intervention period," the authors should consider whether other clinically important factors were assessed, including psychotic disorders, bipolar disorder, substance use disorders, severe suicidality, neurological conditions, unstable pharmacotherapy, or concurrent psychotherapy. If these were not assessed, this should be acknowledged as a methodological limitation because such factors may confound treatment effects.

In the assumptions paragraph, the manuscript reports that "the interaction between group and pretest score was nonsignificant ... indicating that the assumption of homogeneity of regression slopes was satisfied." This assumption is relevant to ANCOVA, not to a standard repeated-measures ANOVA in the form presented here. The authors should either remove this test if no ANCOVA was conducted or clearly explain why it was examined and how it relates to the chosen analytical model. Statistical assumptions should be reported only when they are pertinent to the inferential procedure actually used.

The assumptions section reports Shapiro–Wilk tests separately across time points, but no assessment of multivariate normality or influential multivariate outliers is described in the final manuscript despite mention of Mahalanobis distance in the source material. Because repeated-measures models are sensitive to unusual multivariate profiles, the authors should report the actual Mahalanobis criterion, critical value, and result if this analysis was performed. If not, the reference to multivariate outlier assessment should be removed.

In Table 2, the repeated-measures ANOVA presents corrected degrees of freedom for the within-subject effects, but the error terms and Greenhouse–Geisser epsilon coefficients are not reported. Since Mauchly's test was significant for both outcomes, the manuscript should report the corresponding epsilon values and clearly indicate that the F tests for time and Time × Group are Greenhouse–Geisser corrected. This would make the statistical reporting more complete and consistent with standard APA recommendations.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the intervention description for Emotional Schema Therapy, the sentence "The Emotional Schema Therapy intervention was implemented according to the therapeutic framework proposed by Leahy and colleagues and consisted of ten weekly sessions, each lasting approximately 120 minutes" should be expanded to address intervention fidelity. The authors should report who delivered the intervention, the therapist's qualifications and training, whether a treatment manual was used, whether sessions were supervised or recorded, and whether adherence to the protocol was independently checked. These details are necessary to establish internal validity and treatment reproducibility.

A parallel concern applies to the Dialectical Behavior Therapy protocol. In the paragraph beginning "The Dialectical Behavior Therapy intervention was conducted according to the protocol developed by Linehan," the manuscript should clarify whether the intervention represented full DBT, a DBT skills-training adaptation, or a shortened group-based protocol. An eight-session intervention focusing on selected mindfulness and distress-tolerance skills differs substantially from standard comprehensive DBT, and the terminology should accurately reflect the intervention actually delivered.

In the description of the control condition, the sentence "The control group received no intervention during the corresponding period" is methodologically important but insufficiently elaborated. The authors should specify whether the control group was a wait-list control, treatment-as-usual group, or no-treatment control, and whether participants continued receiving routine psychiatric care or medication. Any differences in concurrent care across groups could confound the treatment effect and should therefore be reported explicitly.

In the Data Collection Tools section, the manuscript identifies the quality-of-life measure as the "12-Item Short Form Health Survey (SF-12)," but the scoring description appears to use a simplified 12-to-48 raw-score interpretation. This requires careful verification because standard SF-12 scoring is generally based on weighted algorithms yielding physical and mental component

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summary scores rather than a simple raw total. The authors should verify the exact validated Persian scoring method used, cite the relevant validation study, and ensure that the reported score ranges and interpretation are psychometrically appropriate.

In the same instrument section, the statement that "Ware et al. reported satisfactory psychometric properties for the instrument, including a Cronbach's alpha coefficient of .89 and a construct validity coefficient of .78" should be checked against the original source. The SF-12 contains heterogeneous health domains and is not typically treated as a unidimensional scale for which a single Cronbach's alpha is necessarily the most appropriate reliability index. The authors should ensure that all reliability and validity coefficients are accurately attributed to the correct version, population, and scoring approach.

In Table 1 and its accompanying paragraph, the descriptive statistics for quality of life are incomplete because the control-group values are missing. This is a major reporting deficiency. Since the repeated-measures analysis compares three groups across three occasions, descriptive means and standard deviations for the control group at pretest, posttest, and follow-up are indispensable. The authors should provide these values and verify that they correspond exactly to the dataset used in the inferential analyses.

Response: Revised and uploaded the manuscript.

2. Revised

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

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E-ISSN: 3041-9026