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Comparison of the Effectiveness of Short-Term Psychodynamic Therapy and Cognitive Behavioral Therapy on Improving Emotion Regulation Strategies in Mothers of Adolescents with Congenital Heart Disease

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1. Round 1

1.1. Reviewer 1

Reviewer:

The text introduces ISTDP but uses the acronym inconsistently. Earlier, the article defines “short-term psychodynamic therapy (STPT),” while here it shifts to “ISTDP.” Consistency in terminology is required throughout the manuscript.

The phrase “Participants were randomly assigned to the groups using block randomization” should be expanded to explain block size, concealment procedures, and whether allocation was blinded to reduce bias.

The CERQ is described thoroughly, but the authors should include internal consistency indices (Cronbach’s alpha) from their sample, not only from prior studies.

The description is detailed, but the sentence “By the end of the program, participants were expected to demonstrate improved emotional flexibility...” reads like an outcome rather than a procedural description. It should be rephrased in methodological terms.

Homework assignments are mentioned, but adherence rates are not reported. Please clarify how homework compliance was monitored and whether it influenced outcomes.

The text claims all assumptions of ANOVA were met, but it would be more rigorous to include test statistics in a supplementary appendix. For example, exact p-values for Shapiro–Wilk should be reported, not just "ranging from .13 to .28."

In Table 1, some means (e.g., self-blame follow-up for psychodynamic group: 8.91) suggest regression compared to posttest (8.37). This should be noted in the text, as it may indicate partial relapse.

The comparison between STPT and CBT is sound, but the authors should caution that equivalence may be due to insufficient power rather than true similarity. Explicitly mentioning sample-size limitations here would improve critical balance.

The authors mention "self-report bias" and "sample from Tehran," but they also incorrectly list "cross-sectional design" when the study is actually longitudinal (pretest–posttest–follow-up). This is contradictory and must be corrected.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The authors cite multiple studies on CBT but do not clearly distinguish whether the cited populations are comparable to mothers of chronically ill adolescents. The applicability of CBT evidence from adolescents or clinical disorders to caregiver mothers should be more explicitly justified.

The authors state: "This represents a critical gap in the literature..." but do not provide global prevalence data of mothers of CHD patients or highlight the clinical urgency. Adding epidemiological context would strengthen the rationale.

The table shows large η^2 values (.59–.66). The authors should discuss the possibility of inflated effect sizes due to small sample size (n = 15 per group).

The authors report "no significant differences" between interventions with all ps > .68. Instead of only stating non-significance, it would be better to report effect size estimates (e.g., Cohen's d) to evaluate practical equivalence.

The sentence "The increased reliance on adaptive strategies... highlights how therapeutic input can foster resilience" could benefit from integrating cultural factors. For Iranian mothers, resilience may be influenced by religious coping or social norms, which are not discussed.

Authors uploaded the revised manuscript.

2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.

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