

Comparison of the Effectiveness of Emotional Freedom Technique (EFT) Training and Self-Compassion Training on Resilience, Anxiety, and Parent–Child Relationships in Parents of Children with Autism

Shirin. Allaf Sharifi¹, Elham. Hakimi Rad^{2*}, Leila. Kashani Vahid³

¹ PhD Student, Department of Psychology of Exceptional Children, SR.C., Islamic Azad University, Tehran, Iran

² Assistant Professor, Department of Psychology, Faculty of Educational Sciences and Psychology, Shahid Beheshti University, Tehran, Iran

³ Department of Psychology of Exceptional Children, SR.C., Islamic Azad University, Tehran, Iran

* Corresponding author email address: e_hakimirad@sbu.ac.ir

E d i t o r	R e v i e w e r s
Monika Szczygiel  Department of Psychology, Jagiellonian University, Krakow, Poland monika.szczygiel@uj.edu.pl	Reviewer 1: Abolghasem Khoshkanesh  Assistant Professor, Counseling Department, Shahid Beheshti University, Tehran, Iran. Email: akhoshkonesh@sbu.ac.ir Reviewer 2: Thseen Nazir  Professor of Psychology and Counseling Department, Ibn Haldun University, Istanbul, Turkey. Email: thseen.nazir@ihu.edu.tr

1. Round 1

1.1. Reviewer 1

Reviewer:

In the Methods section, the eligibility criteria are too limited for a clinical intervention study. The sentence indicating that inclusion required “having a child diagnosed with autism, providing informed consent, and committing to regular attendance” should be expanded to specify how the child’s ASD diagnosis was established and verified, the age range of the children, whether parents had to be the primary caregiver, and whether concurrent psychotherapy, psychiatric medication, or participation in other parent-training programs was permitted. These variables could materially influence anxiety, resilience, and parent–child relationship outcomes and therefore require explicit control or documentation.

In the Methods section, the sample-size justification based on a general recommendation of “at least 15 participants” per quasi-experimental group is methodologically weak. The manuscript should replace or supplement this heuristic with an a priori statistical power analysis based on the primary outcome, intended ANCOVA model, number of groups, expected effect

size, alpha level, desired power, and anticipated attrition. Given the observed effect sizes ranging from approximately partial $\eta^2=.111$ to $.352$, a formal power calculation would provide much stronger justification for the adequacy of a sample of 20 participants per group.

In the Findings section, Table 1 contains descriptive statistics for pretest, posttest, and follow-up, but the control group is also assigned follow-up values even though the subsequent description of follow-up testing refers only to the two intervention groups. Please clarify whether the control group was actually assessed at follow-up. If not, its follow-up means and standard deviations should not be presented. If it was assessed, those data should be included in the longitudinal inferential analysis rather than restricting maintenance analyses to the two active interventions.

The assumption-testing paragraph states that normality was supported because Shapiro–Wilk p values were greater than $.05$. This should be interpreted more cautiously. A nonsignificant Shapiro–Wilk test does not demonstrate a “greater than 95% probability of normality”; it merely indicates insufficient evidence to reject normality. The manuscript should remove any probabilistic interpretation of normality and preferably supplement the test with examination of skewness, kurtosis, Q–Q plots, and potential outliers, especially given the relatively small group sizes of $n=20$.

The ANCOVA model requires further statistical clarification. The manuscript reports $F(2,52)$ for the group effect after adjustment for pretest scores, although a conventional one-way ANCOVA with 60 participants, three groups, and one covariate would ordinarily produce a different residual degrees of freedom unless additional covariates or exclusions were involved. Please provide the full model specification and explain how the reported $df=52$ was obtained. This is especially important because the preceding multivariate analysis lists different degrees of freedom, and the current presentation makes it difficult to reconstruct the analysis.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the Data Collection Tools paragraph for the Connor–Davidson Resilience Scale, the manuscript presents inconsistent scoring descriptions. The underlying Persian text alternates between a five-point scale scored “1 to 5” and another description suggesting scoring from “0” to “5,” whereas the standard CD-RISC-25 is conventionally scored using a 0–4 response format. Please verify the exact version administered in this study, provide the actual response anchors and total possible score range, and ensure that all scoring information is internally consistent. The psychometric coefficients should also be attributed to the precise validation study used for the Persian version.

In the DASS-21 paragraph, the manuscript states that “the total anxiety score was calculated by summing the responses to the seven relevant items.” Please clarify whether the summed DASS-21 anxiety score was multiplied by two to permit comparison with the original 42-item DASS severity thresholds. The descriptive means presented for anxiety suggest that raw seven-item scores may have been analyzed, but this should be stated explicitly. In addition, please report the internal consistency of the anxiety subscale within the present sample rather than relying exclusively on previously published reliability coefficients.

In the Parent–Child Relationship Scale paragraph, the manuscript describes a 33-item, three-dimensional instrument comprising conflict, intimacy, and dependency, yet it also refers to previous confirmatory-factor findings supporting a “two-factor structure.” This is conceptually inconsistent and requires correction. Please identify the exact Persian version used, explain whether all three subscales were retained, and report psychometric evidence corresponding specifically to that version. Reliability coefficients for conflict, intimacy, and dependency should ideally be calculated and reported for the current study sample.

The intervention protocol descriptions are currently too generic to permit replication. For example, the EFT paragraph states that participants “were taught to focus on a specific emotionally salient problem while simultaneously applying the characteristic EFT tapping procedure,” but the manuscript does not specify the number of sessions, session duration, frequency,

therapist qualifications, exact tapping points, sequence of procedures, use of Subjective Units of Distress ratings, homework assignments, or treatment-fidelity monitoring. These details are essential and should be reported systematically.

The self-compassion intervention paragraph likewise requires considerably greater procedural detail. The sentence stating that the program focused on “self-kindness, recognition of common humanity, and mindful awareness” explains the conceptual basis but not the actual intervention delivered. Please specify the intervention source or manual, number and duration of sessions, session content, exercises, homework assignments, therapist training, and adherence assessment. Without these details, it is impossible to determine whether the intervention corresponds to a recognized self-compassion protocol or an investigator-developed adaptation.

The manuscript should reconsider its terminology concerning Emotional Freedom Technique and emotion-focused therapy. In the introduction and discussion, EFT is at times theoretically supported using literature on emotionally focused therapy, including Johnson’s emotionally focused couple therapy framework. These are distinct interventions with different theoretical origins and procedures. The sentence suggesting that “emotionally focused therapeutic models regard emotional processes as central mechanisms” does not constitute direct theoretical evidence for Emotional Freedom Technique. Please clearly distinguish Emotional Freedom Techniques/tapping from Emotion-Focused Therapy or Emotionally Focused Couple Therapy and avoid using evidence from one approach as if it directly validates another.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.