

The Effectiveness of Emotional Schema Therapy on Executive Functions in Adolescents with Emotional Dysregulation in Isfahan

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E d i t o r	R e v i e w e r s
Anela Hasanagic ^{id} Full Professor, Department of Psychology, Faculty of Islamic Education, University of Zenica, Bosnia and Herzegovina anela.hasanagic@unze.ba	Reviewer 1: Azade Abooei ^{id} Department of Counseling, Faculty of Humanities, University of Science and Art, Yazd, Iran. Email: a.abooei@tea.sau.ac.ir . Email: Ali.darbani@iau.ac.ir Reviewer 2: Mehdi Rostami ^{id} Department of Psychology and Counseling, KMAN Research Institute, Richmond Hill, Ontario, Canada. Email: dr.mrostami@kmanresce.ca

1. Round 1

1.1. Reviewer 1

Reviewer:

In the paragraph beginning with “Among the various psychological processes affected by emotional dysregulation, executive functions occupy a particularly important position” (p. 3), executive functions are introduced broadly, but the manuscript does not clearly justify why each BRIEF subscale was expected to improve following Emotional Schema Therapy. The authors should provide a conceptual framework linking specific therapeutic components (e.g., emotional validation, acceptance, cognitive restructuring) to inhibition, working memory, planning, and monitoring.

The statement “Emotional Schema Therapy seeks to enhance emotional regulation and psychological flexibility” (p. 3) requires additional empirical support. The manuscript would benefit from a more detailed review of Emotional Schema Therapy literature, particularly randomized controlled trials and meta-analytic findings, to establish the evidence base of the intervention beyond a few cited studies.

In the final paragraph of the Introduction, the authors conclude that “interventions targeting emotional schemas may hold significant promise for promoting adaptive cognitive and emotional development” (p. 4). However, the specific research gap

remains insufficiently articulated. The authors should explicitly identify what is unknown in the existing literature regarding Emotional Schema Therapy and executive functioning and explain how the present study addresses this gap.

In the Methods section, the authors report that “those who obtained scores higher than 60 were considered eligible for participation” on the DERS (p. 4). The rationale for selecting this cutoff score is not provided. The authors should justify this threshold with reference to validation studies or normative data and explain whether it reflects clinically significant emotional dysregulation.

The manuscript states that “participants were allocated to groups through random assignment following the sampling process” (p. 4). More methodological detail is required regarding the randomization procedure. Specifically, the authors should clarify whether simple randomization, block randomization, or another method was used, who generated the allocation sequence, and whether allocation concealment was implemented.

In the Measures section, the authors indicate that the “Behavioral Rating Inventory of Executive Function (BRIEF), Teacher Form” was used (p. 5). However, it is unclear who completed the measure. Since the study was conducted in counseling centers rather than schools, the manuscript must specify whether teachers, parents, or adolescents themselves provided the ratings. This issue is critical because it directly affects the validity of the outcome assessment.

In the Discussion, the authors argue that “dysfunctional emotional schemas become modified, adolescents may experience reduced emotional burden and greater cognitive flexibility, thereby improving executive functioning” (p. 8). While theoretically plausible, this mechanism was not directly measured. The authors should acknowledge that mediation processes were inferred rather than empirically tested and avoid causal interpretations unsupported by the data.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The description of the BRIEF reports that “higher scores reflecting greater executive functioning difficulties” (p. 5). However, Table 1 and the discussion interpret increasing scores as improvements in executive functioning. This appears contradictory. The authors must clarify the scoring procedure and explain whether scores were reverse-coded. If higher scores indeed indicate greater dysfunction, the interpretation of the results would need substantial revision.

The Intervention section provides a narrative summary of the treatment but lacks sufficient detail for replication. For example, the manuscript states that “The middle phase of treatment focused on emotional expression, emotional processing through therapeutic writing, social support utilization, compassionate letter writing, and the development of interpersonal effectiveness skills” (p. 5). The authors should include a session-by-session treatment protocol, preferably in a table, detailing objectives, techniques, and homework assignments.

The manuscript does not report whether treatment fidelity was assessed. Given that Emotional Schema Therapy is a structured intervention, the authors should indicate whether therapists received training, whether adherence checklists were used, and whether any supervision procedures were implemented to ensure treatment integrity.

In the Data Analysis section, the authors state that “Inferential analyses were conducted using Multivariate Analysis of Covariance (MANCOVA)” (p. 6). However, no justification is provided for choosing MANCOVA rather than repeated-measures ANOVA or mixed-model analysis. The authors should explain the rationale for this analytical choice and discuss its assumptions.

Table 1 presents descriptive statistics indicating dramatic increases in executive function scores. For example, initiation increased from 6.13 to 15.06 in the experimental group (p. 6). Such large changes over a relatively brief intervention period raise questions regarding measurement sensitivity and possible ceiling effects. The authors should comment on the clinical plausibility of these improvements and report standardized effect sizes.

The manuscript states that “skewness and kurtosis values for all variables were within acceptable ranges” (p. 7). However, several values reported in Table 1 approach or exceed commonly recommended thresholds. The authors should specify the criteria used to evaluate normality and provide additional evidence such as Shapiro–Wilk statistics.

In reporting the assumption tests, the manuscript notes that “Box’s M test was conducted to assess the homogeneity of covariance matrices and yielded a non-significant result ($p = .674$)” (p. 7). The actual Box’s M statistic and degrees of freedom are not reported. For transparency and reproducibility, these values should be included.

Table 2 reports extremely large F-values (e.g., Monitoring: $F = 432.41$; Shift of Attention: $F = 414.53$) (pp. 7–8). Such values are unusual in psychological intervention studies with a sample size of only 30 participants. The authors should verify the calculations, report partial eta squared values, and provide confidence intervals to facilitate interpretation of the magnitude of effects.

The Results section states that “the intervention significantly improved all executive function subscales in the experimental group” (p. 8). However, no adjusted posttest means or estimated marginal means are reported. Because MANCOVA controls for pretest scores, adjusted means should be presented to allow readers to understand the nature of group differences.

Authors uploaded the revised manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.