

# Comparison of the Effectiveness of Gestalt-Based Play Therapy and Parent-Centered Play Therapy on Academic Self-Efficacy among Female Students in the Second Cycle of Elementary School


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## 1. Round 1

### 1.1. Reviewer 1

Reviewer:

The statement “children who obtained the cutoff score in all three assessments” in the Participants section is methodologically ambiguous because, at the recruitment stage, posttest and follow-up assessments had not yet occurred. It appears that the authors intended to refer to the screening phase or baseline measurement only. This wording raises concerns about procedural clarity and potential retrospective selection bias. The sentence should be revised to explicitly state how participants were screened and at which time point the cutoff criterion was applied.

The authors state that participants scoring “lower than 60” on the Academic Self-Efficacy Questionnaire were included, yet no empirical or normative justification for this cutoff is provided. Since the scale ranges from 30 to 120, the rationale for selecting 60 as the threshold should be supported either through prior literature, psychometric norms, percentile distributions, or clinical interpretation. Without justification, the criterion appears arbitrary and weakens methodological rigor.

In the Methods section, the sampling strategy is described simultaneously as “convenience sampling” and “two schools were randomly selected.” The manuscript should clarify the multistage sampling process because the current wording creates

confusion regarding whether the study used random cluster selection followed by convenience sampling or entirely non-random recruitment. This distinction is important for evaluating external validity and sampling bias.

The manuscript does not report whether participants were randomly assigned to the three groups after recruitment. Given the quasi-experimental design, it is essential to explicitly describe the allocation procedure. If random assignment was used, the manuscript should specify the randomization method. If not, the authors should discuss the resulting threats to internal validity, including selection bias and baseline nonequivalence.

The manuscript repeatedly uses the phrase “academic self-efficacy develops partly through supportive interpersonal experiences,” but the study did not assess mediators such as parental attachment, emotional support, or family functioning. Including validated measures of these constructs would substantially strengthen the explanatory value of the study and clarify the mechanisms underlying the superiority of parent-centered play therapy.

The study exclusively included female students aged 9–11 years from public schools in one district of Tehran, yet the Discussion occasionally generalizes findings broadly to “children” and “students.” The authors should narrow their claims and explicitly acknowledge gender-, culture-, and context-specific limitations throughout the interpretation of findings.

The manuscript would benefit from inclusion of a CONSORT-style participant flow diagram showing the number of students screened, excluded, allocated to groups, lost to follow-up, and analyzed. This omission limits transparency regarding participant retention and attrition, particularly in a longitudinal intervention design with follow-up assessment.

Response: Revised and uploaded the manuscript.

## 1.2. Reviewer 2

Reviewer:

In Table 1, the descriptive results indicate that the parent-centered play therapy group demonstrated substantially larger gains in academic self-efficacy than the Gestalt-based group (70.20 vs. 63.86 at posttest), yet the Discussion initially emphasizes Gestalt-based mechanisms more extensively and with greater theoretical detail. The interpretive emphasis should be better aligned with the actual statistical findings, particularly because the superior efficacy of the parent-centered intervention represents the most clinically meaningful outcome of the study.

The manuscript reports extremely large effect sizes for the repeated-measures ANOVA results ( $\eta^2 = .811, .967, \text{ and } .940$ ), which are unusually high for psychosocial intervention research involving behavioral outcomes. The authors should verify these calculations and clarify whether partial eta squared or another effect size index was used. Additionally, confidence intervals for effect sizes should be reported to improve interpretability and transparency.

The assumption testing procedures are incomplete. Although the authors report the Shapiro–Wilk normality test, the manuscript does not provide results for Mauchly’s test of sphericity, Levene’s test of equality of variances, or Box’s M test where applicable. Since repeated-measures ANOVA assumptions directly influence the validity of F statistics, these analyses should be explicitly reported, including any corrections such as Greenhouse–Geisser adjustments.

The sentence “because the Bonferroni test calculates the combined means of the three groups, the graph of the main effects of group and time should be considered” is statistically unclear and conceptually inaccurate. Bonferroni adjustments do not “calculate combined means”; rather, they correct pairwise comparisons for inflated Type I error. The authors should revise this explanation using accurate statistical terminology.

The intervention descriptions are highly detailed procedurally but insufficiently standardized scientifically. For example, in the Gestalt-based protocol, the description “children preferred to play the Zip-Zap game and the musical chair activity” introduces unnecessary anecdotal detail, while critical methodological information such as therapist qualifications, treatment fidelity procedures, supervision protocols, and adherence monitoring is absent. Intervention fidelity is essential for establishing replicability and treatment integrity.

The manuscript does not report whether the same therapist administered both interventions or whether different therapists conducted each treatment arm. Therapist effects may significantly influence psychological intervention outcomes, especially

in play therapy research. The absence of this information limits the ability to determine whether differences between interventions were attributable to treatment modality or therapist characteristics.

The Results section lacks baseline equivalence testing between groups. Before interpreting treatment effects, the manuscript should statistically demonstrate that groups did not significantly differ at pretest on academic self-efficacy or demographic characteristics. Without this analysis, it is difficult to determine whether post-intervention differences reflect treatment effects or pre-existing group disparities.

In the Discussion, the paragraph beginning with “The improvement in academic self-efficacy observed in the Gestalt-based play therapy group...” contains strong causal interpretations regarding emotional regulation and self-awareness mechanisms, yet these variables were not directly measured in the study. The authors should avoid overextending causal interpretations beyond the collected data or alternatively include mediation-related measures in future studies.

Response: Revised and uploaded the manuscript.

## 2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.