






# Effectiveness of Group Attachment Based Intervention (GABI) on Improving Emotional Safety and Perceived Stress of Vulnerable Children

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E d i t o r	R e v i e w e r s
Ghorban Hemati Alamdarloo  Associate Professor, Department of Exceptional Children, Shiraz University, Shiraz, Iran ghemati@shirazu.ac.ir	<b>Reviewer 1:</b> Ali Akbar Soleymanian  Associate Professor of Counseling Department, Bojnord University, Iran. Email: Soleymanian@ub.ac.ir <b>Reviewer 2:</b> Hajar Torkan  Assistant Professor, Department of Psychology, Islamic Azad University, Isfahan Branch (Khorasgan), Isfahan, Iran. h.torkan@khuisf.ac.ir

## 1. Round 1

### 1.1. Reviewer 1

Reviewer:

The description of nine categories of vulnerability lacks a clear source citation or explanation of how these categories were operationalized for this study. The authors should indicate whether these categories were adopted from an existing framework (e.g., UNICEF, WHO).

The argument about stress exposure is strong but would be improved by distinguishing acute versus chronic stressors, aligning with psychobiological research on vulnerability trajectories.

The source Ghanbari et al. is cited for the training model, but the adaptation process is not described. Please clarify whether any cultural or contextual modifications were made for the Tehran Welfare population.

The intervention description is comprehensive, yet some sessions (e.g., Session 6: empathy-building) would benefit from specifying therapeutic techniques (e.g., guided imagery, role-play, or emotion labeling). Adding fidelity measures would strengthen methodological rigor.

The reliability indices are reported, but the validity evidence (construct, convergent, or discriminant) is not discussed. The reviewer recommends including prior factor analysis results or citing Iranian adaptation studies in more detail.

The Shapiro–Wilk test p-values indicate some violations of normality (e.g.,  $p=0.007$  for follow-up). The authors should discuss how this affects the validity of ANCOVA and whether nonparametric corrections were considered.

This section is conceptually rich but lacks references to empirical studies that have measured emotion regulation changes in vulnerable youth. Including recent meta-analyses (2023–2025) on attachment-based or emotion regulation programs would strengthen the argument.

Authors revised the manuscript and uploaded the document.

## 1.2. Reviewer 2

Reviewer:

The discussion of attachment theory is insightful, but the authors should expand on how group formats modify individual attachment processes—citing mechanisms such as co-regulation, observational learning, and peer mirroring.

The aim statement, “The purpose of the current study was to evaluate...”, would be more powerful if phrased as a clear research question or hypothesis (e.g., “It was hypothesized that GABI would significantly increase emotional safety and decrease perceived stress compared with a control group”).

Although the F and p values are correctly presented, reporting partial eta squared interpretations (e.g., small, medium, large) would help readers evaluate effect size magnitude.

The interpretation, “indicating stable effects,” could be misleading because a significant difference between post-test and follow-up ( $p=0.010$ ) actually suggests change, not perfect stability. Clarify whether this indicates sustained improvement or slight variation over time.

The first line merely repeats the study objective. The reviewer recommends condensing this and moving directly to the interpretation of findings, integrating them with the theoretical rationale of attachment-based group work.

Authors revised the manuscript and uploaded the document.

## 2. Revised

Editor’s decision: Accepted.

Editor in Chief’s decision: Accepted.