


# The Effectiveness of Resilience-Based Group Training on Uncertainty Intolerance, Future Orientation, and Stress Appraisal in High School Students

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## 1. Round 1

### 1.1. Reviewer 1

Reviewer:

The introduction extensively discusses resilience, intolerance of uncertainty, and stress appraisal, yet it lacks a sufficiently explicit theoretical integration among these constructs. For example, the paragraph beginning with “Resilience refers to the dynamic process...” discusses resilience broadly, but the manuscript does not clearly articulate a conceptual mechanism explaining how resilience training is expected to simultaneously influence future orientation and stress appraisal. A more coherent theoretical model or conceptual framework diagram is recommended.

The manuscript repeatedly references post-pandemic and crisis-related literature; however, the study itself was conducted in a “normal educational environment.” The paragraph beginning “The psychological disruptions caused by social crises...” overemphasizes COVID-19-related evidence without sufficiently justifying its relevance to the current sample. The authors should better explain why pandemic-era findings remain theoretically applicable to contemporary Iranian adolescents in 2025–2026.

The rationale for selecting the three dependent variables appears underdeveloped. Specifically, the manuscript does not sufficiently explain why “future orientation” was selected instead of closely related constructs such as hope, optimism,

academic engagement, or psychological capital. The paragraph beginning “In addition to uncertainty intolerance...” would benefit from stronger justification grounded in developmental psychology literature.

The authors should provide more detail regarding ethical procedures involving adolescent participants. The sentence “obtaining parental consent” is insufficient because the manuscript does not indicate whether assent from students themselves was obtained, whether ethics committee approval was secured, or whether an institutional review board code existed.

The control group received “no intervention during the study period,” which raises concerns regarding attention bias and placebo effects. Because the experimental group received structured interpersonal interaction and facilitator attention, improvements may partially reflect nonspecific therapeutic factors rather than resilience-specific mechanisms. The use of an active control group would strengthen causal inference.

Authors uploaded the revised manuscript.

## 1.2. Reviewer 2

Reviewer:

The study design is labeled “quasi-experimental,” yet the manuscript states that participants were “randomly assigned” into groups. This creates methodological ambiguity because random assignment typically characterizes a true experimental design. The authors should clarify whether the quasi-experimental designation resulted from non-random sampling, lack of full control, or practical constraints.

The inclusion criterion “elevated levels of uncertainty intolerance and maladaptive stress appraisal” is insufficiently operationalized. The manuscript does not report the cutoff scores, percentile thresholds, or screening procedures used to determine eligibility. Without clear criteria, the sample selection process cannot be adequately evaluated for validity or bias.

The intervention description lacks procedural specificity necessary for replication. In the paragraph beginning “The resilience-based group training program was implemented...,” the authors mention cognitive restructuring, relaxation, role-playing, and emotional regulation techniques, but they do not provide session-by-session content, therapeutic exercises, or facilitator protocols. Including a structured intervention table or supplementary appendix would substantially strengthen methodological transparency.

The manuscript does not specify the professional qualifications or training of the group facilitator(s). Since intervention effectiveness may depend heavily on therapist competence, the absence of information regarding facilitators’ credentials, supervision, or adherence monitoring represents a notable methodological limitation.

Although the authors state that assumptions for MANCOVA were examined using Shapiro–Wilk, Levene’s, and Box’s M tests, there is no mention of testing the homogeneity of regression slopes assumption, which is essential in covariance analysis. The omission raises concerns regarding the validity of the ANCOVA/MANCOVA results.

The reporting of statistical significance throughout Tables 2 and 3 should be revised to conform to APA standards. Specifically, p-values are reported as “0.001” instead of “ $p < .001$ .” In addition, the manuscript inconsistently uses decimal formatting across text and tables. Greater consistency in statistical reporting is needed.

The manuscript reports extremely large effect sizes ( $\eta^2$  values ranging from .53 to .71) for a relatively brief eight-session intervention. Such unusually high effects warrant deeper discussion regarding possible inflation due to small sample size, expectancy effects, or shared method variance. The discussion currently interprets these values positively without critically evaluating their plausibility.

The demographic information provided in the Findings section is limited. The manuscript reports age, gender, and grade level, but it omits potentially important contextual variables such as socioeconomic status, parental education, academic performance, or family structure, all of which could influence resilience and stress appraisal outcomes among adolescents.

Authors uploaded the revised manuscript.

## 2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.