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The Effectiveness of a Social-Emotional Learning Program on Social Acceptance and Perceived Competence in Adolescents with Specific Learning Disorder

Roya. Yadkar[1], Jahangir. Karami[2*], Kamran. Yazdanbakhsh[3]

[1] Master of Educational Psychology, Department of Psychology, Faculty of Social Sciences, Razi University, Kermanshah, Iran
[2] Associate Professor, Department of Psychology, Faculty of Social and Educational Sciences, Razi University, Kermanshah, Iran
[3] Associate Professor, Department of Psychology, Faculty of Social Sciences, Razi University, Kermanshah, Iran

*** Corresponding author email address**: j.karami@razi.ac.ir

Editor	Reviewers
Seyyed Mohsen Asgharinekah Associate Professor, Department of Counseling and Educational Psychology, Faculty of Education and Psychology, Ferdowsi University of Mashhad, Mashhad, Iran asghari-n@um.ac.ir	**Reviewer 1:** Sara Nejatifar Department of Psychology and Education of People with Special Needs, Faculty of Educational Sciences and Psychology, University of Isfahan, Isfahan, Iran. Email: s.nejatifar@edu.ui.ac.ir **Reviewer 2:** Kamdin. Parsakia Department of Psychology and Counseling, KMAN Research Institute, Richmond Hill, Ontario, Canada. Email: kamdinarsakia@kmanresce.ca

1. Round 1

1.1. Reviewer 1

Reviewer:

The participant paragraph contains an important inconsistency between the target population and the inclusion criteria. The manuscript initially states that the population consisted of students “enrolled in the second stage of elementary education,” whereas the inclusion criteria specify only students in the fifth grade and aged 11–13 years. The authors should clarify whether all eligible participants were fifth-grade students or whether other grades were initially considered. In addition, the age range of 11–13 years should be justified because fifth-grade students may ordinarily have a narrower age distribution. Reporting the final sample’s exact age mean, standard deviation, range, sex distribution, grade distribution, and type of specific learning disorder would substantially improve the description of the sample.

The intervention paragraph beginning with “The social-emotional learning intervention was administered by the researcher in 12 sessions” should include more information about treatment fidelity. Although the session content is described in considerable detail, the manuscript does not indicate how adherence to the intervention manual was monitored. The authors

should state whether a session checklist was used, whether any sessions were observed or rated by an independent evaluator, whether the facilitator received specific training, and whether all planned components were delivered consistently. In behavioral intervention research, intervention fidelity is essential for determining whether observed effects can reasonably be attributed to the intended protocol.

The same intervention paragraph should address the possibility of facilitator and expectancy effects. Because "the intervention was administered by the researcher," the person responsible for the study may also have been responsible for delivering treatment and potentially collecting outcome data. The authors should clarify whether the posttest assessments were administered or scored by someone blinded to group assignment. If blinding was not feasible, this should be acknowledged as a potential source of bias. The manuscript should also explain whether the control group received usual educational services, no additional activity, or delayed intervention, because this affects interpretation of the observed between-group effects.

The Results section reports that all assumptions for MANCOVA and ANCOVA "were evaluated" and "supported," but no numerical assumption-test results are presented. The paragraph referring to the Shapiro–Wilk test, Mahalanobis distance, Mardia's statistic, Bartlett's test, Box's M test, Levene's test, scatterplots, and homogeneity of regression slopes should report the relevant statistics and probability values. Simply stating that assumptions were satisfied does not allow readers to independently assess model adequacy. At minimum, Box's M, Levene's tests for each outcome, the group-by-pretest interaction tests for homogeneity of regression slopes, and the multivariate outlier assessment should be quantitatively documented.

The sentence stating that "baseline group differences were assessed using one-way analysis of variance" raises a methodological concern because Table 1 shows substantial pretest differences between the groups, particularly for total perceived competence, behavioral competence, and social competence. For example, total perceived competence was 63.22 in the control group but 51.50 in the experimental group at pretest. Such large baseline differences appear inconsistent with the assertion that there were no meaningful pre-intervention differences. The authors should report formal baseline comparisons, discuss these imbalances explicitly, and explain how they may affect interpretation despite covariance adjustment. These differences may also indicate that random allocation did not achieve baseline equivalence, which is particularly relevant given the small sample size.

The statistical model used for the ANCOVA on social acceptance should be reported more completely. Table 2 includes the group effect, error term, and corrected total but omits the pretest covariate effect. Because the principal purpose of ANCOVA is to adjust posttest scores for baseline values, the table should include the pretest covariate with its sum of squares, degrees of freedom, mean square, F statistic, probability value, and effect size. The same recommendation applies to the ANCOVA table for total perceived competence and to the univariate analyses of the competence subscales. Omitting the covariate makes the model specification incomplete and prevents readers from evaluating the contribution of baseline scores.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The sample-size justification in the paragraph stating that "a minimum sample size of 15 participants per group has been recommended for experimental studies" is not sufficiently rigorous for the statistical analyses employed. A generic rule of thumb based on a minimum number per group does not ensure adequate power for ANCOVA and MANCOVA. The authors should conduct and report an a priori or sensitivity power analysis specifying the anticipated effect size, alpha level, desired statistical power, number of groups, number of dependent variables, and covariates. Given the final sample of 36 participants and the use of multivariate analyses across four competence dimensions, statistical power is a particularly important concern and should be addressed explicitly.

The manuscript should provide a participant-flow description within the Methods section. At present, the text reports that 36 students were recruited, but it does not indicate how many students were initially approached, screened, excluded, declined participation, randomized, completed the intervention, or were included in the final analysis. Because absence from more than

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two sessions and incomplete questionnaires were defined as exclusion criteria, readers need to know whether any participants met these criteria after allocation. The authors should report attrition separately for the experimental and control groups and clarify whether analyses followed a complete-case approach or an intention-to-treat principle.

The paragraph describing the Marlowe–Crowne instrument requires major conceptual clarification. The manuscript refers to it as the "Marlowe-Crowne Social Desirability Scale" while simultaneously interpreting scores as an indicator of "social acceptance." These are not equivalent psychological constructs. The original Marlowe–Crowne scale measures socially desirable responding, specifically the tendency to present oneself in culturally approved ways, rather than peer acceptance or interpersonal social acceptance. The authors must provide a strong psychometric and conceptual justification for treating this instrument as a measure of social acceptance, or preferably reconsider the operationalization of the dependent variable. Without such clarification, the interpretation that the intervention improved "social acceptance" may constitute a construct-validity problem.

Relatedly, the sentence stating that individuals scoring "between 20 and 33" demonstrate behavior that is highly consistent with social rules and norms should be reconsidered. High scores on the Marlowe–Crowne scale may indicate a stronger social-desirability response tendency rather than objectively greater social adaptation. The current interpretation risks conflating socially desirable self-presentation with actual acceptance by peers. The authors should revisit the scoring interpretation using the validated version employed in the study and clearly state whether the Persian adaptation has demonstrated criterion, construct, or convergent validity specifically for social acceptance. If the instrument was adapted or reinterpreted in Iranian research, that validation process should be documented in detail.

The reliability paragraph for the Marlowe–Crowne measure states that "In the present study, the internal consistency of the scale, as assessed by Cronbach's alpha, was .89," but it is unclear whether this coefficient was calculated using pretest data, posttest data, or combined observations from both measurement occasions. Reliability should ideally be reported separately at each assessment point, particularly in an intervention study where score distributions may change. The same issue applies to the perceived competence measure. The authors should report internal consistency estimates for the current sample for total perceived competence and, preferably, for each subscale at both pretest and posttest.

The paragraph describing the Perceived Competence Scale requires greater precision regarding the exact instrument used. The manuscript states that the measure contains 27 items and assesses behavioral, social, academic, and physical competence, yet Harter's self-perception instruments have appeared in several versions with different numbers of items and subscales. The authors should identify the exact version, describe the number of items assigned to each dimension, explain whether any items are reverse-coded, specify how total and subscale scores are calculated, and provide an example of the structured-alternative response format if that format was retained. This is essential for replication and for evaluating whether the total perceived competence score is psychometrically justified.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor's decision: Accepted.
Editor in Chief's decision: Accepted.

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