

Comparison of the Effectiveness of Parent Management Training and Life Therapy on Feelings of Shame and Guilt Among Mothers of Children with Autism in Sari

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E d i t o r

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R e v i e w e r s

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1. Round 1

1.1. Reviewer 1

Reviewer:

The participant paragraph reports that “A total of 45 eligible mothers were selected through purposive sampling... and were subsequently assigned by simple random allocation to three equal groups.” The sampling and allocation procedures should be described in greater methodological detail. The authors should specify how the 45 participants were identified from the three clinics, whether all eligible mothers were approached consecutively, how many were screened, how many declined participation, and how the random allocation sequence was generated. It should also be stated whether allocation concealment was implemented and whether any participant withdrew after randomization. A participant-flow description would improve transparency, particularly because the final sample is relatively small.

The inclusion and exclusion criteria in the Study Design and Participants section are insufficiently comprehensive for an intervention trial. The paragraph currently mentions age, educational level, absence of severe physical illness, willingness to participate, and attendance requirements, but it does not specify how the child’s autism spectrum disorder diagnosis was established or confirmed. The authors should indicate whether diagnosis was based on medical records, psychiatric or

psychological assessment, DSM criteria, or standardized instruments. In addition, maternal psychiatric conditions, concurrent psychotherapy, psychotropic medication, participation in other parenting programs, and major recent life events could substantially influence shame and guilt and should either be controlled or acknowledged as potential confounders.

The manuscript states that ethics approval and clinical trial registration were obtained, but the Methods section does not report the actual ethics approval code or trial registration number. The sentence beginning “Before recruitment and implementation, the required institutional permissions and research ethics approval were obtained...” should therefore be revised to include the full name of the approving ethics committee, ethics code, registry name, registration identifier, and, where applicable, registration date. These details are essential for research transparency and are generally required for publication of intervention studies.

The Data Collection Tool section identifies the instrument as the “Guilt and Shame Proneness Scale (GASP) developed by Cohen, Wolf, Panter, and Insko (2011),” but the manuscript should verify that the reported scoring and construct labels exactly correspond to the validated version used in the study. Some terminology used elsewhere in the manuscript, particularly “negative evaluation” and “reparative actions,” should be standardized with the accepted GASP terminology. The authors should also report whether an officially validated Persian translation was used, how the translation was obtained, and whether evidence of construct validity has been established in Iranian samples. Reporting only Cronbach’s alpha coefficients does not sufficiently establish the appropriateness of the instrument for this population.

The paragraph describing reliability reports Cronbach’s alpha coefficients of “.82 for guilt, .79 for shame, and .86 for the total scale.” This reporting should be expanded because the statistical analyses are conducted on four subscales rather than only the two broad domains and total score. Internal consistency should therefore ideally be reported separately for negative self-evaluation, withdrawal behavior, negative behavior evaluation, and reparative behavior. Furthermore, the authors should consider whether a total GASP score is psychometrically appropriate, since shame and guilt are theoretically distinct constructs and the subscales may not necessarily justify aggregation into a single total score. This issue should be addressed explicitly.

The results in Table 2 require careful statistical verification, particularly the effect-size values. For negative self-evaluation, the group effect is reported as $F = 6.413$ with an effect size of .694, whereas for withdrawal behavior the group effect is $F = 15.105$ with an effect size of .418. These values appear unusual if the reported effect size is partial eta squared, especially given the relative F statistics and sample size. The authors should specify exactly which effect-size statistic is reported, preferably partial η^2 , and verify all values against the statistical output. The table heading should not simply state “Effect Size” without identifying the metric.

The post hoc analysis strategy requires stronger justification. Table 3 appropriately uses Bonferroni-adjusted pairwise comparisons for repeated measurement occasions, but Table 4 reports Tukey post hoc comparisons only between the two experimental groups. Because the design contains three groups, the authors should report all relevant pairwise group comparisons, including PMT versus control, Life Therapy versus control, and PMT versus Life Therapy, with multiplicity adjustment. Reporting only the comparison between the two active treatments does not demonstrate directly that each treatment differed significantly from the control group, even though the narrative implies this conclusion. Full pairwise comparisons are necessary to support claims of intervention effectiveness.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The Parent Management Training protocol contains a potentially serious conceptual and ethical inconsistency in the original intervention description. The seventh session was described as involving “the use of physical punishment,” whereas the English version interprets this session as correcting mothers’ reliance on physical punishment and replacing punitive practices with evidence-based strategies. The authors must verify the original treatment manual and make the intended content unambiguous. If the intervention discouraged physical punishment, this should be explicitly stated. If any physical punishment was actually

instructed or practiced, this would raise substantial ethical concerns and would require justification, although such an approach would not be consistent with contemporary evidence-based behavioral parent training.

The Life Therapy protocol is described in considerable detail, but its fidelity and standardization remain unclear. The paragraph beginning “The second experimental group participated in an eight-session Life Therapy program...” should specify whether the therapist followed a manual, whether session checklists were used, whether supervision occurred, and whether treatment fidelity was assessed. Since both interventions were delivered by the same psychologist or by psychologists with potentially different allegiances to the interventions, therapist effects may influence the findings. The authors should explicitly state who delivered each intervention, the therapist’s qualifications, whether the therapist was trained equally in both approaches, and whether adherence to the protocols was independently monitored.

The statistical analysis section states that a “mixed-design repeated-measures analysis of variance was conducted,” but the Results table appears to report the measurement-occasion effect with only 1 degree of freedom despite three assessment occasions: pretest, posttest, and follow-up. Under a standard repeated-measures ANOVA with three time points, the uncorrected within-subject time effect would ordinarily have 2 degrees of freedom. The reported $df = 1$ therefore requires explanation. The authors should carefully recheck the statistical output, specify whether a particular contrast was analyzed, or determine whether the table contains transcription errors. This issue is critical because it directly affects the validity and reproducibility of the inferential findings.

The assumption-testing paragraph also contains a statistical inconsistency in the sentence stating that “The results of Box’s M test indicated that... the significance level was .092; therefore, the assumption of equality of covariance matrices was supported.” The authors should report the actual Box’s M statistic, associated F approximation, relevant degrees of freedom, and exact p value in standard format. More importantly, the description in the Persian source appears to characterize .092 as “significant,” which would be incorrect under conventional thresholds. The authors should re-examine the original SPSS output and ensure that the interpretation is based on the actual p value rather than a translation or transcription error.

The reporting of Mauchly’s test of sphericity should be revised substantially. The manuscript currently states only that the test was not significant and that the assumption was therefore satisfied, but no Mauchly’s W, chi-square statistic, degrees of freedom, or p value is provided. For a repeated-measures analysis involving three measurement occasions, these values should be reported. In addition, the authors should specify whether Greenhouse–Geisser or Huynh–Feldt corrections were considered if sphericity was violated for any variable. This is particularly important given the unexplained degrees of freedom reported for the within-subject effects in Table 2.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.