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Comparison of the Effectiveness of Acceptance and Commitment Therapy and Logotherapy on Guilt, Parenting Stress, and Psychological Burnout in Mothers of Children with High-Functioning Autism Spectrum Disorder

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1. Round 1

1.1. Reviewer 1

Reviewer:

The Methods section reports that "the final sample size was to be determined through an a priori statistical power analysis," yet the Findings section presents a final sample of 69 participants, with 23 participants in each group. The actual power analysis used to justify this number must be reported rather than described prospectively. The authors should specify the statistical test used for the calculation, assumed effect size, alpha level, desired power, number of groups, number of repeated measurements, assumed within-person correlation, nonsphericity correction, expected attrition, software used, and resulting minimum sample size. Because the manuscript emphasizes that an arbitrary value such as 15 participants per group should not be used, omission of the actual calculation is particularly important to correct.

The intervention section states that "the logotherapy intervention was delivered… through 10 structured group sessions," whereas "the Acceptance and Commitment Therapy intervention was delivered… in eight structured group sessions." This

difference in treatment dose is a potentially important confound. A comparison of two interventions with different numbers of sessions does not isolate therapeutic orientation from treatment exposure, therapist contact, group support, and expectancy effects. The authors should report the duration and frequency of each session and either justify why unequal treatment doses were considered acceptable or explicitly acknowledge treatment-dose inequality as a design limitation. Ideally, comparative efficacy trials should match active conditions on total therapist-contact time.

The paragraph describing the logotherapy protocol requires substantial conceptual clarification. Several reported components—such as identification of negative thoughts, rumination, problem solving, avoidance, activity monitoring, and relapse prevention—appear more characteristic of cognitive-behavioral or behavioral activation protocols than classical logotherapy. The authors should identify the exact source or manual from which the 10-session logotherapy protocol was derived and demonstrate how each session corresponds to core logotherapeutic principles such as meaning discovery, freedom of attitude, responsibility, self-transcendence, dereflection, or paradoxical intention. Otherwise, the intervention cannot confidently be interpreted as logotherapy, which directly affects the validity of the study's main comparison.

The row reporting the main effect of group on parental guilt appears statistically inconsistent: the manuscript gives $F = 3.366$ but reports $p < .001$. With degrees of freedom of approximately 2 and 66, an F value of 3.366 would not ordinarily correspond to $p < .001$. Similar inconsistencies may exist elsewhere. The authors should not manually reconstruct inferential tables from intermediate calculations; instead, all values should be transcribed directly from the statistical software output and independently checked. This is a major issue because erroneous p values can change whether an effect is interpreted as statistically significant.

The post-hoc section contains an important internal inconsistency. The original table reports the posttest ACT-versus-control comparison for parental guilt as a mean difference of -3.73 with a 95% confidence interval from -6.69 to -0.78 but an adjusted p value of .08; similarly, logotherapy versus control is reported as -3.47 with a confidence interval from -6.43 to -0.52 but $p = .16$. Confidence intervals excluding zero are incompatible with nonsignificant two-sided comparisons at the .05 level. The manuscript should return to the original Bonferroni output and verify whether the correct values were .008 and .016, whether the confidence intervals were adjusted, and whether the reported p values lost a decimal place. This must be corrected before publication.

The interpretation of the post-hoc findings should be aligned with the actual inferential results. The manuscript initially states that “both interventions were associated with significant improvements relative to the control condition” across the three outcomes, yet Bonferroni-adjusted longitudinal comparisons show that logotherapy did not differ significantly from control for parental burnout at posttest ($p = .063$) or follow-up ($p = .193$). The Discussion appropriately acknowledges this inconsistency, but the Abstract and Results should do the same. The authors should distinguish between the significant baseline-adjusted ANCOVA effect of logotherapy on posttest burnout and the nonsignificant conservative longitudinal pairwise comparisons rather than presenting logotherapy as unequivocally effective for burnout.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the ACT protocol, the manuscript appropriately mentions “acceptance, cognitive defusion, contact with the present moment, self-as-context, values clarification, and committed action,” but the session-by-session description does not clearly demonstrate adequate coverage of all six core ACT processes. In particular, self-as-context and acceptance appear underdeveloped relative to values, cognitive defusion, and mindfulness. The authors should specify which exercises, metaphors, homework assignments, and experiential techniques were used to operationalize each process. This is especially important because treatment integrity depends not simply on labeling a program ACT but on demonstrating fidelity to the ACT model.

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The manuscript notes that "therapeutic adherence was monitored using a structured fidelity checklist," but no methodological details or results of this assessment are presented. The authors should report who rated treatment fidelity, whether sessions were observed live or reviewed from recordings, the number or percentage of sessions evaluated, the checklist domains, scoring procedure, inter-rater reliability if multiple raters were involved, and mean fidelity scores for ACT and logotherapy. Reporting treatment fidelity is essential in a comparative psychotherapy study because differences or similarities in outcomes cannot be meaningfully attributed to the protocols if treatment delivery was not verified.

The measurement paragraph for the Parenting Stress Index–Short Form requires correction and verification. The earlier description of the instrument states that "higher total and subscale scores represent greater levels of parenting stress," whereas some source material supplied for the study stated that higher scores indicate lower stress. The inferential and descriptive results clearly interpret lower scores as improvement, which is consistent with the conventional PSI-SF interpretation. The authors should ensure that the scoring direction is correctly described throughout the manuscript, verify whether any items were reverse-scored, report the possible total and subscale score ranges, and state which validated Persian version and scoring manual were used. A scoring-direction error would fundamentally alter interpretation of the results.

The psychometric reporting for the Parental Guilt Scale should be expanded. The manuscript states that the measure contains nine items and three dimensions and reports previously published Cronbach's alpha coefficients, but it does not provide reliability coefficients from the present sample. Because reliability is sample dependent, the authors should report Cronbach's alpha or preferably omega for the total score and relevant subscales at least at baseline. Furthermore, if the instrument has a proposed cutoff that was used for participant eligibility, the exact cutoff, its source, validation evidence, sensitivity/specificity where available, and rationale for its use in mothers of children with ASD must be reported.

The manuscript should reconsider the analysis strategy because two partially overlapping inferential frameworks are currently used: repeated-measures mixed ANOVA for the principal longitudinal analysis and numerous pairwise ANCOVAs for individual hypotheses. The statement "the principal inferential analyses were based on linear mixed-effects models" in the Methods is also inconsistent with the Findings, which actually report repeated-measures ANOVA and ANCOVA. The authors should select and consistently report the analysis that was truly performed. Given three groups and three assessment points, a linear mixed-effects model would provide a coherent primary analysis, accommodate individual trajectories, avoid the sphericity requirement, and reduce the need for multiple separate ANCOVAs.

The manuscript reports that Mauchly's test was significant for all three outcomes and therefore uses Greenhouse–Geisser corrections. This is appropriate for repeated-measures ANOVA, but the reporting in Table 2 and the associated text contains several numerical inconsistencies. For example, the table gives partial $\eta^2 = .668$ for the time effect on parental guilt, whereas the narrative reports .686; for parenting stress, the table reports partial $\eta^2 = .370$ for the group × time interaction, whereas an earlier narrative value is .317. All inferential statistics should be audited against the original software output, and the manuscript should use one definitive value for each F statistic, degree of freedom, p value, and effect size.

Response: Revised and uploaded the manuscript.

2. Revised

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

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