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Development, Validation, and Effectiveness of an Integrative Counseling Package Based on Emotion-Focused Therapy and Positive Psychology for Trauma-Exposed Women Heads of Household

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Hussein OMAR Alkhozahe Professor, Department of Sociology, Al-Balqa' Applied University, Salt, Jordan huss1960@bau.edu.com	**Reviewer 1:** Nadereh Saadati Department of Couple and Family therapy, Alliant International University, California, United States of America. mdaneshpour@alliant.edu **Reviewer 2:** Masoud Asadi Assistant Professor, Department of Psychology and Counseling, Arak University, Arak, Iran. Email: m-asadi@araku.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

The manuscript should reconsider its description of blinding. The Methods section states that “participants were not informed of the exact intervention condition to which they had been allocated until completion of the pretest assessment” and that “the follow-up evaluator was unaware of group allocation.” Elsewhere, this procedure is described as both single- and double-blinding. In psychotherapy trials, participants necessarily become aware of the treatment they receive once sessions commence, and the therapist cannot be blinded to treatment condition. Therefore, describing the study as double-blind would be inaccurate. The authors should instead report that allocation information was withheld from participants until intervention initiation and that the follow-up assessor was blinded to group assignment. This terminology should be corrected because inappropriate claims of blinding may overstate the trial's protection against performance and detection bias.

The qualitative sample and saturation procedure need stronger reporting. The Findings section states that “no substantively new concepts emerged after approximately the twelfth interview; two additional interviews were conducted to verify saturation.” This is useful, but the manuscript should explain how saturation was operationalized and documented. For example, the authors should state whether coding occurred concurrently with data collection, whether a saturation grid or codebook was

maintained, whether meaning saturation or code saturation was targeted, and who determined that saturation had been reached. Since the qualitative phase was instrumental in constructing the intervention package, transparency regarding saturation is essential for assessing whether the identified themes adequately represented the target population's needs.

The rigor and trustworthiness of the thematic analysis should be expanded substantially. The manuscript currently states that interviews were analyzed "according to Braun and Clarke's thematic analysis framework using MAXQDA 2020," but does not report researcher reflexivity, coder characteristics, coding independence, consensus procedures, audit trail, member checking, peer debriefing, or other credibility strategies. Software use alone does not establish analytic rigor. The authors should specify whether one or more researchers coded the transcripts, how disagreements were handled, whether the coding framework was inductive, deductive, or hybrid, and what procedures were used to enhance credibility, dependability, confirmability, and transferability. If no second coder or participant validation was used, this should be reported transparently rather than retroactively implied.

The transition from qualitative themes to intervention components should be made more systematic and auditable. The manuscript states that "the qualitative results supported the decision to construct an intervention that simultaneously addressed emotional processing, self-perception, strengths, hope, meaning, and adaptive future orientation," and gives examples linking themes to techniques. However, the intervention-development process would be scientifically stronger if the authors explicitly described how each major qualitative theme informed specific intervention objectives, sessions, or techniques. For example, the inhibition–shame cycle appears to map onto emotional awareness, two-chair work, and emotional-expression training, whereas fear of the future maps onto hope and goal-setting. The authors should explain whether this mapping followed predefined theoretical criteria, researcher consensus, or expert judgment. Such information is critical to demonstrate that the qualitative phase genuinely shaped the package rather than merely serving as contextual justification for an intervention already derived from the literature.

The content-validity analysis requires methodological correction and more complete reporting. The manuscript reports that "the mean CVI for the complete intervention was 0.86" and that "a CVR of 0.62 or higher was considered acceptable for the 10-member expert panel." The authors should specify whether the reported CVI represents I-CVI averaged across items, S-CVI/Ave, or another index, because these statistics have different meanings. It would also be appropriate to report agreement at the item level and explain how ratings of relevance, clarity, and simplicity were combined. Most importantly, the critical CVR threshold should be verified against the specific Lawshe critical-value table adopted, because published revisions to critical values differ. The manuscript should cite the precise methodological source used to justify the chosen threshold and provide enough information to reproduce the calculations.

The manuscript should avoid inferring sustained treatment effects from descriptive follow-up values alone. The Findings state that "the maintenance of substantially improved scores at follow-up also indicated that the intervention effects, particularly those associated with the integrative package, were largely sustained over the three-month period." This conclusion requires inferential evidence comparing posttest with follow-up and examining group × time effects, not merely inspection of means. The authors should report whether posttest-to-follow-up changes were statistically significant within each active condition and whether the groups remained significantly different at follow-up. Bonferroni-adjusted comparisons across the three time points would allow a more defensible claim regarding durability. If such analyses were not conducted, the wording should be moderated to state only that descriptive patterns were consistent with maintenance.

The ANCOVA results require more complete assumption testing and interpretation. Table 4 reports statistically significant group effects after adjustment for pretest scores, but the manuscript does not report the homogeneity-of-regression-slopes assumption, which is fundamental to ANCOVA. The authors should test and report the group × pretest interaction for each dependent variable before interpreting adjusted group effects. In addition, because four separate ANCOVAs were performed, the manuscript should address familywise Type I error across outcomes or clearly explain why the multivariate model was treated as the primary test. The authors should also clarify whether the reported η^2 values are eta squared or partial eta squared; given the SPSS framework and the magnitude reported, they are likely partial eta squared and should be labeled ηp^2 consistently.

The Bonferroni post hoc table appears incomplete and its derivation should be clarified. Table 5 presents only four comparisons per outcome—for example, "Integrative > EFT," "Integrative > Positive Psychology," "Integrative > Control," and "EFT > Positive Psychology"—although four groups yield six possible pairwise comparisons. Comparisons involving EFT

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versus control and positive psychology versus control are omitted, yet the manuscript states that "all three active interventions were effective compared with the control condition." The authors should either provide all six pairwise comparisons per outcome or explicitly state that only hypothesis-driven comparisons are shown and report the omitted comparisons in the text or supplementary material. Furthermore, the stated Bonferroni threshold of .0083 corresponds to six comparisons and therefore reinforces the expectation that all six pairwise tests should be transparently reported.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The reporting of the intervention protocols should include treatment-fidelity procedures. The Methods section notes that "all intervention sessions were conducted by the researcher," but there is no indication that adherence to the EFT, positive-psychology, or integrative manuals was independently assessed. This omission is particularly important because the same therapist administered three theoretically distinct active conditions, creating a risk of cross-condition contamination or therapist allegiance effects. The authors should report whether structured session checklists, audio/video recordings, independent supervision, fidelity ratings, or protocol-adherence forms were used. If no formal fidelity assessment was conducted, this limitation should be acknowledged explicitly because differences between groups cannot otherwise be attributed with confidence to the intended intervention components.

The rationale for matching the three active interventions on treatment dose should be articulated more clearly. The manuscript states that "each intervention consisted of 10 group sessions lasting approximately 90 minutes, delivered twice weekly over five weeks." Although equal session duration helps control for treatment exposure, the integrative condition appears to contain substantially more therapeutic content per session than either single-modality condition. This creates a possible treatment-intensity confound: superior outcomes may partly reflect exposure to a greater number or diversity of techniques rather than integration itself. The authors should discuss how equivalence of treatment intensity was addressed, whether the amount of therapist interaction and homework was comparable across conditions, and whether the integrative protocol required compression of techniques that could alter treatment depth. This issue should also be recognized in the Discussion.

The attrition analysis is insufficiently developed. The manuscript reports that "12 participants were lost during the intervention and follow-up process" and that the final analyses included "60 participants, with 15 women in each group." The authors should report how attrition was distributed across the four study conditions, the reasons for dropout by condition, and whether completers and non-completers differed on baseline demographic or psychological variables. It is statistically unusual for a total loss of 12 participants to yield exactly 15 completers in each group unless attrition was precisely three participants per group; if this occurred, it should be stated explicitly. Furthermore, the manuscript appears to use a complete-case/per-protocol analysis rather than intention-to-treat analysis. This analytic choice should be justified and discussed as a potential source of attrition bias.

The manuscript should provide a full demographic and baseline-comparability analysis for the quantitative sample before reporting intervention effects. Table 3 shows broadly similar pretest means for the psychological outcomes, but no demographic characteristics are reported for the four groups. Variables such as age, reason for household headship, number of children, duration of household headship, education, employment, socioeconomic status, time since divorce or bereavement, and baseline trauma severity could plausibly affect treatment response. The authors should report and statistically compare these characteristics across groups. Demonstrating baseline comparability is particularly important in a study with only 15 analyzed participants per condition, where random allocation may not guarantee balance across clinically meaningful covariates.

The primary statistical model described in the Methods is not sufficiently represented in the Findings. The manuscript states that "the principal intervention effects were evaluated using repeated-measures multivariate analysis of variance, with group as the between-subjects factor and time as the within-subjects factor," yet the reported inferential tables emphasize ANCOVA at posttest and Bonferroni comparisons. The Results should provide the actual repeated-measures MANOVA findings,

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including the multivariate test statistic selected, such as Wilks' Lambda, Pillai's Trace, Hotelling's Trace, or Roy's Largest Root, together with F, degrees of freedom, p values, and effect sizes for the main effects of time and group and, most importantly, the group × time interaction. Without those results, the central longitudinal hypothesis and the maintenance of effects at follow-up are not formally demonstrated.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor’s decision: Accepted.
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