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Determining the Effectiveness of Hypnotherapy in Reducing Loneliness and Grief Reactions among Individuals Bereaved by COVID-19

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the Methods section, the sentence “Thirty eligible volunteers were recruited from the target population through convenience sampling” requires greater procedural detail regarding recruitment. The authors should specify how potential participants were identified by Bahman, Atiyeh, and Parsian hospitals, whether hospital staff referred participants directly, whether advertisements or telephone invitations were used, how many individuals were screened, how many declined participation, and how many were excluded before random allocation. A participant-flow description is necessary to assess possible selection bias and the feasibility of reproducing the study.

In the Study Design and Participants paragraph, the statement “participants were randomly assigned to either the experimental group or the control group” is insufficiently detailed for a study that relies on group allocation to support causal inference. The manuscript should report the randomization procedure, such as a random-number table, computerized sequence, sealed envelopes, or another allocation method; who generated the sequence; who enrolled participants; and whether allocation

was concealed. Without this information, it is difficult to distinguish true random assignment from informal or quasi-random allocation.

The manuscript refers to participants being excluded for "absence from more than three intervention sessions," which would theoretically allow a participant attending only five of eight sessions to remain in the study. The authors should report actual attendance, the number of participants who completed all eight sessions, whether any participants missed one to three sessions, and how missing sessions were handled. It would also be useful to justify the threshold of more than three absences because such a degree of incomplete treatment exposure could materially affect intervention fidelity and outcome estimates.

The statistical analysis strategy requires clearer specification. The Methods state that repeated-measures analysis of variance was used, but the Results also report Box's M, Mauchly's test, Greenhouse-Geisser corrections, Wilks' Lambda, between-subject effects, and pairwise comparisons. The authors should clearly identify the statistical model for each outcome: whether a two-way mixed repeated-measures ANOVA was performed separately for each grief subscale and loneliness, whether the six grief dimensions were first tested within a multivariate repeated-measures framework, and what correction was used for multiple hypothesis testing. Without a clearly specified analytic hierarchy, the risk of inflated Type I error is substantial because numerous dependent variables and comparisons were examined.

The statement regarding Box's M for loneliness contains a direct statistical contradiction: "Box's M test was significant, M = 8.655, F(6, 5680.302) = 3.121, p = .005, indicating that the equality of variance-covariance matrices assumption was not satisfied; therefore, the multivariate Wilks' Lambda statistic was considered." When homogeneity of covariance matrices is violated, Wilks' Lambda is not generally the most robust multivariate statistic; Pillai's Trace is typically preferred under violations of multivariate assumptions, especially with small samples. The authors should re-evaluate this analysis and either report a more robust statistic or provide a methodological justification for retaining Wilks' Lambda.

The interpretation of effect sizes should be revised. The Results describe some treatment effects as "low" or "moderate," but values such as partial η^2 = .607, .450, .434, and .405 are substantial by commonly used benchmarks. The authors should apply a consistent convention for interpreting partial eta squared and avoid qualitative labels that understate the magnitude of the observed effects. At the same time, because the sample is small, confidence intervals around effect sizes should ideally be reported or the Discussion should acknowledge that effect estimates may be unstable and potentially inflated.

The pairwise-comparison results should clarify the adjustment method used. The manuscript states that comparisons were generated "using the Syntax tool in SPSS software," but this is not a statistical procedure and does not indicate whether Bonferroni, Sidak, least significant difference, or another correction was applied. Because multiple comparisons were conducted across seven outcomes and three assessment occasions, the authors should explicitly state the multiplicity-adjustment procedure and report adjusted p values if applicable. If no correction was applied, this should be acknowledged and preferably corrected through reanalysis.

In the Discussion, the sentence "The significant time × group interactions across the grief-reaction components indicate that the observed changes were not simply the result of the passage of time but were associated with participation in the intervention" is reasonable but should be expressed more cautiously. Given the small sample, convenience recruitment, lack of an active control condition, and probable absence of assessor blinding, the study cannot fully separate specific hypnotic effects from expectancy, therapeutic attention, group participation, relaxation, or cognitive-behavioral components. The Discussion should consistently describe hypnotherapy as being "associated with" improvement rather than implying definitive specific efficacy attributable to hypnosis alone.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The sample-size justification requires substantial revision. The manuscript currently states that "a minimum sample size of approximately 15 participants per group is commonly regarded as acceptable for quasi-experimental studies," but this is not a

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statistically adequate justification. A priori power analysis should ideally be reported based on the expected time × group interaction, number of repeated measurements, alpha level, desired power, and anticipated effect size. If no prospective power analysis was conducted, this should be acknowledged explicitly, and the authors should avoid presenting 15 participants per group as an established methodological minimum.

The demographic paragraph reports that "the mean age of participants was 41.60 ± 15.50 years in the experimental group and 42.15 ± 67.42 years in the control group." The standard deviation of 67.42 years is highly implausible for a group with a mean age of 42.15 and strongly suggests a transcription or data-entry error. This value must be verified against the original dataset before publication. Because descriptive errors can cast doubt on the accuracy of the inferential analyses, the authors should systematically recheck all reported means, standard deviations, test statistics, and confidence intervals.

The eligibility criterion stating that participants had "no debilitating physical or psychiatric disorder as determined by the researcher through a clinical interview" needs operational clarification. The manuscript should specify whether a structured, semi-structured, or unstructured clinical interview was used, who conducted the interview, what qualifications that person had, and which psychiatric or physical conditions were considered exclusionary. In a grief intervention study, screening for major depression, psychosis, substance use disorders, posttraumatic stress disorder, and suicide risk is particularly important and should be explicitly described.

The manuscript should report the interval between the COVID-19-related death and enrollment in the study. The current criterion only specifies that participants "had lost a loved one as a consequence of COVID-19," but grief reactions vary markedly depending on whether the loss occurred weeks, months, or more than a year earlier. Time since bereavement is therefore a potentially important confounder. The authors should report this variable descriptively and, if substantial variability existed, consider whether it should have been controlled statistically or discussed as a limitation.

The relationship between the participant and the deceased is not reported, although this variable is likely to have considerable clinical relevance. Losing a spouse, child, parent, sibling, or more distant relative can produce substantially different patterns of grief intensity, loneliness, attachment disruption, and personal growth. The Methods and Findings sections should therefore report the distribution of relationship-to-deceased categories and, if possible, assess whether the experimental and control groups were comparable on this characteristic before intervention.

In the description of the UCLA Loneliness Scale, the reported scoring appears inconsistent with the descriptive data in Table 1. The Methods state that total scores range from 20 to 80, yet the Findings report loneliness means such as 0.907, 0.89, and 0.956, while the pairwise comparisons later report differences of approximately 4 to 5 points. These metrics cannot all refer to the same scoring system. The authors must verify whether the loneliness variable was reported as a standardized score, transformed score, mean item score, or raw total score. The scale scoring, descriptive statistics, and inferential results must be placed on one internally consistent metric.

The Hogan Grief Reaction Checklist terminology should be checked carefully. The manuscript uses "Denial" as one subscale, whereas the standard Hogan Grief Reaction Checklist is commonly described using specific established subscale labels. The authors should verify the exact version and validated translation administered in this study and use the official subscale names consistently throughout the Methods, Findings, Discussion, tables, and abstract. If a Persian adaptation uses a modified factor structure or translated label, the source and psychometric justification for that version should be reported.

The intervention paragraph states that the protocol was "developed based on the principles and procedures presented by Elkins" and that its content was "reviewed and approved by relevant academic experts," but this does not provide sufficient evidence of treatment standardization. The authors should report who delivered the intervention, the therapist's professional qualifications, hypnosis training and clinical experience, whether a treatment manual was followed, whether all participants received the same scripted hypnotic suggestions, and whether treatment fidelity or adherence was independently assessed. These details are necessary to determine whether the intervention can be replicated and whether treatment effects can reasonably be attributed to a standardized hypnotherapy protocol.

Response: Revised and uploaded the manuscript.

2. Revised

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