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# Comparison of the Effectiveness of Cognitive-Behavioral Couple Therapy and Intensive Solution-Focused Couple Therapy on Intolerance of Uncertainty and Generalized Anxiety in Infertile Women

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## 1. Round 1

### *1.1. Reviewer 1*

Reviewer:

The Generalized Anxiety Disorder Scale paragraph should use the instrument's standard name consistently, preferably the Generalized Anxiety Disorder-7 (GAD-7). The statement that the instrument was used for "identifying probable cases of generalized anxiety disorder and assessing the severity of associated clinical symptoms" is appropriate, but the manuscript should clarify that the GAD-7 is a screening and symptom-severity measure rather than a diagnostic interview. If the authors use the term "generalized anxiety disorder" throughout the manuscript, they should avoid implying that participants received a formal DSM-based diagnosis unless a structured clinical interview was actually administered.

The psychometric reporting for the Intolerance of Uncertainty Scale should be checked carefully against the original source. The manuscript describes a 12-item version developed by Carleton et al. (2007), which is presumably the IUS-12, but terminology and scoring anchors should be standardized according to the validated version actually administered. Please state the full scale name, whether the total score or subscale scores were analyzed, whether any items were reverse-scored, and which

Persian validation version was used. These details are essential because different versions of the Intolerance of Uncertainty Scale have appeared in the literature and should not be treated interchangeably.

The intervention description requires information regarding therapist qualifications and treatment fidelity. The paragraph beginning "The cognitive-behavioral couple therapy intervention was implemented according to the protocol proposed by Dattilio (2009)" describes session content in detail but does not indicate who delivered the intervention, the therapist's academic degree, clinical training, experience in couple therapy, or supervision arrangements. The same problem applies to the solution-focused intervention. Please report whether the same therapist administered both interventions, whether treatment manuals or session checklists were used, whether sessions were independently reviewed, and whether adherence/fidelity ratings were obtained. These factors are crucial when comparing two active psychotherapies.

Table 3 should be carefully checked because some pairwise mean differences do not correspond to the raw descriptive means in Table 1. For example, the pretest intolerance-of-uncertainty means reported in Table 1 are 34.95 for cognitive-behavioral couple therapy and 31.79 for intensive solution-focused couple therapy, implying a raw difference of 3.16, whereas Table 3 reports a difference of 5.87. Similar discrepancies appear elsewhere. The note indicating that pairwise comparisons were based on adjusted inferential analyses is helpful but insufficient. Please specify whether these are estimated marginal means from transformed data, adjusted means, or another quantity. If transformed scores were analyzed, presenting mean differences in the original measurement metric may be misleading unless back-transformed appropriately.

The interpretation of generalized anxiety should be made more statistically precise. The Discussion states that "intensive solution-focused couple therapy produced a significant reduction in generalized anxiety at posttest compared with the control group," which is supported by the post hoc comparison ($p = .036$). However, the two active interventions did not significantly differ from one another at posttest ($p = 1.000$). Therefore, the results do not demonstrate that intensive solution-focused couple therapy was superior to cognitive-behavioral couple therapy. The manuscript should consistently distinguish "significant versus control" from "significantly different from the other active treatment" and avoid interpreting one significant comparison and one nonsignificant comparison as evidence of a significant difference between the two therapies.

Authors revised the manuscript and uploaded the document.

### *1.2. Reviewer 2*

Reviewer:

There is a significant dose imbalance between the two treatments that needs to be addressed explicitly. Cognitive-behavioral couple therapy consisted of ten 2-hour sessions, whereas intensive solution-focused couple therapy consisted of six 90-minute sessions. Thus, the interventions differed substantially in total therapist contact time. The manuscript currently interprets outcome differences primarily as differences between therapeutic models, but treatment duration represents a potential confound. The authors should discuss why unequal treatment doses were used, whether they correspond exactly to established protocols, and how this difference affects causal interpretation of any apparent superiority of one intervention over the other.

The description of the intensive solution-focused protocol includes content that appears conceptually misaligned with the stated primary outcomes. For example, the intervention paragraph refers repeatedly to "self-efficacy, emotional regulation, and dyadic adjustment," whereas the study outcomes are intolerance of uncertainty and generalized anxiety. This raises concern that the protocol may have been adapted from a different study or population. Please explain the theoretical rationale linking each major intervention component to intolerance of uncertainty and generalized anxiety, and indicate whether the protocol was formally adapted for infertile couples. If adaptation occurred, the process and content-validity procedures should be reported.

The statistical assumptions paragraph raises an important methodological concern. The authors state that "a rank-based transformation was applied to variables that violated the normality assumption… Following transformation, the distributions were considered acceptable for parametric analysis." This procedure requires much greater methodological transparency. Please identify the exact transformation used, provide a recognized statistical rationale or reference, explain whether analyses were conducted on ranks while descriptive statistics remained in raw-score units, and clarify how effect sizes and post hoc



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comparisons were derived. A rank transformation combined with parametric repeated-measures ANOVA can alter interpretation and may not necessarily resolve violations appropriately. A robust mixed-effects model or another method less sensitive to distributional departures may be preferable.

The handling of sphericity should be reported more precisely. The manuscript states that "Mauchly's test of sphericity indicated that the sphericity assumption was violated; therefore, epsilon-adjusted degrees of freedom were used," but it does not identify which correction was applied. Please specify whether Greenhouse–Geisser, Huynh–Feldt, or another correction was used and report the epsilon value for each outcome. The fractional degrees of freedom in Table 2 strongly suggest an adjustment, but readers should not be required to infer the method from the df values.

The manuscript should reconsider whether repeated-measures ANOVA is the optimal statistical method for this design. With three groups, three measurement occasions, a relatively small sample, some non-normal distributions, and potentially correlated observations if recruitment occurred at the couple level, a linear mixed-effects model may offer a more flexible and defensible analytical framework. In particular, mixed models can handle within-subject correlation directly, allow more flexible covariance structures, and provide clearer estimation of group-by-time contrasts. At minimum, the authors should justify their selection of repeated-measures ANOVA and explain how missing values, if any, were handled.

Table 2 contains an important inconsistency between the reported generalized-anxiety effect sizes and the narrative interpretation. The table reports partial $\eta^2 = .04$ for Group and partial $\eta^2 = .43$ for Time, whereas the original narrative states that "43%" of change was attributable to group membership and "4%" to time. The revised text appears to have corrected this, but the authors should verify all reported effect-size interpretations against the table and original SPSS output. In addition, it would be preferable to interpret partial eta squared explicitly as the proportion of variance attributable to an effect conditional on other modeled effects rather than as a literal percentage of "change caused by" group membership.

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## 2. Revised

Editor's decision: Accepted.
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