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Comparing the Effectiveness of a Sexual Intelligence Educational–Counseling Package and Johnson's Emotionally Focused Therapy on Sexual Relationships Among Women Experiencing Marital Conflict

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1. Round 1

1.1. Reviewer 1

Reviewer:

The sample-size description in the sentence "The required sample size was estimated using Cohen's sample size criteria, assuming a medium effect size of .05, a statistical power of .75, and a significance level of .05" is statistically problematic and should be revised. An effect size of .05 would ordinarily be considered very small rather than medium, depending on the metric used, and the manuscript does not specify whether the effect size refers to Cohen's f, partial eta squared, or another index. Moreover, statistical power of .75 is lower than the conventional minimum of .80 used in most intervention studies. The authors should report the exact effect-size metric, analytical test assumed in the calculation, number of groups and repeated measurements, anticipated correlation among repeated observations, nonsphericity correction if relevant, software or Cohen table used, and whether inflation for potential attrition was considered.

In the participant selection paragraph, the sentence "women who obtained a total score greater than 126 were considered eligible for participation" requires stronger justification. The manuscript should provide an empirical or psychometric basis for the cutoff score of 126 on the Sanaei Marital Conflict Questionnaire. It is currently unclear whether this threshold represents a validated clinical cutoff, a normative percentile, the theoretical midpoint, or a cutoff selected by the investigators. Because this

criterion directly determined eligibility and therefore the clinical characteristics of the sample, the authors should provide a citation or validation evidence supporting this threshold and clarify whether the same cutoff has been used in previous Iranian studies of marital conflict.

The inclusion criterion stating that participants had "no acute or chronic psychiatric disorder as confirmed by a psychiatrist or clinical psychologist" is insufficiently operationalized. The authors should specify how psychiatric conditions were assessed, whether a structured or semi-structured diagnostic interview was used, whether diagnoses were based on DSM criteria, and whether all participants were formally evaluated by the same type of clinician. If eligibility was determined from medical records or clinical judgment rather than a standardized diagnostic instrument, this should be explicitly stated. The absence of a clearly defined psychiatric screening procedure raises concerns regarding reproducibility and potential selection bias.

The intervention section should address treatment fidelity. The manuscript provides a detailed summary of the 10 sessions but does not report who delivered each intervention, the therapist's professional qualifications, training in sexual counseling or Emotionally Focused Therapy, years of clinical experience, or whether the same therapist delivered both interventions. The authors should also describe whether manuals or session checklists were used, whether any sessions were observed or recorded, and whether adherence to the intervention protocol was independently evaluated. Without treatment-fidelity information, it is difficult to determine whether differences between interventions reflect the theoretical approaches themselves or variation in therapist delivery.

The implementation of "Johnson's Emotionally Focused Therapy" requires particular clarification because the sample consisted of women rather than couples. In the intervention description, several procedures explicitly refer to "interactions between spouses," "the couple relationship," and participants' "engagement with their spouses," yet it is unclear whether husbands attended any sessions. If only women participated, the intervention should not be described uncritically as standard Emotionally Focused Couple Therapy because the canonical model is dyadic and typically involves both partners. The authors should explain how the original EFT model was adapted for individual or group delivery to women, specify whether spouses participated in any exercises, and discuss the implications of this adaptation for treatment fidelity and interpretation of the nonsignificant EFT effect.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The Methods section states that participants were excluded if they were taking "psychiatric medications and medications used for the management of hypertension or diabetes" because of possible effects on sexual functioning. This criterion should be refined scientifically because these medication classes are heterogeneous and do not uniformly impair sexual function. The authors should identify the specific medication categories considered exclusionary, such as selective serotonin reuptake inhibitors, certain antihypertensive agents, or other drugs with established sexual side effects, and explain whether medication status was self-reported, clinically verified, or assessed from prescriptions. Broad exclusion of all antihypertensive and antidiabetic medications may unnecessarily restrict the sample and reduce external validity.

The manuscript should provide substantially more information on randomization. The phrase "allocated through simple randomization, using a random-number table" does not indicate who generated the allocation sequence, who enrolled participants, whether allocation was concealed, or whether the person conducting outcome assessment was aware of group assignment. These procedural details are important because inadequate allocation concealment can introduce selection bias even when a random-number sequence is used. The authors should therefore report the sequence-generation procedure, allocation ratio, method of concealment, person responsible for allocation, and whether outcome assessors or data analysts were blinded to intervention condition.

The description of the Hurlbert Index of Sexual Assertiveness contains a conceptual inconsistency that should be resolved. The manuscript describes the primary outcome as "sexual relationship quality" while using selected items from an instrument explicitly developed to assess sexual assertiveness. The authors should explain why the selected items are valid indicators of

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"sexual relationship" rather than one dimension of sexual assertiveness and should provide evidence supporting the factorial interpretation of Items 7, 10, 13–20, and 22–24 as a distinct Sexual Relations subscale. Without such evidence, there is a risk of construct mislabeling, which could affect the validity of both the results and the study title.

In the Data Collection Tools section, the reliability information for the HISA appears internally inconsistent with the source text. The original material reports both "the reliability of the Sexual Relations subscale... was .81" and subsequently states that "the alpha value in the present study was .76," whereas the English version currently presents only a general statement that internal consistency was acceptable. The authors must resolve this discrepancy and report one correct Cronbach's alpha coefficient for the analyzed subscale in the current sample. If .81 and .76 refer to different scales, assessment points, or samples, this distinction should be explicitly explained. Reliability estimates should be reported transparently because they directly affect interpretation of measurement precision.

The description of the Sanaei Marital Conflict Questionnaire also contains conflicting reliability coefficients that require clarification. The source material reports one set of Cronbach's alpha coefficients from Dehghan and another set with notably low subscale values, including coefficients of .16, .20, .27, and .30. These values would indicate unacceptable internal consistency for several subdimensions. The manuscript should identify the source of each reliability estimate, explain whether they refer to different studies or samples, and avoid presenting weak reliability coefficients without discussion. Most importantly, the authors should report the reliability coefficient obtained for the total marital conflict score in the present sample because this instrument was used as the principal screening measure.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor's decision: Accepted.
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