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Comparison of the Effectiveness of Compassion-Focused Therapy and Mindfulness-Based Cognitive Therapy on Sexual Function and Quality of Life in Women with Hypothyroidism

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1. Round 1

1.1. Reviewer 1

Reviewer:

The introductory paragraph appropriately contextualizes the issue of hypothyroidism and its psychological effects. However, the statement, "Hypothyroidism is the second most common endocrine disorder after diabetes," could benefit from more specificity. It would be helpful to provide a citation or a clearer reference to the source of this statistic to ensure that the claim is well-supported. A more robust explanation of the ranking of hypothyroidism compared to other endocrine disorders would strengthen this point.

The claim that hypothyroidism is linked to “limitations in daily activities and diminished occupational performance” is well-supported, but the authors should consider referencing studies that quantify the extent of occupational impact, such as absenteeism or reduced productivity. Specific data would provide a stronger empirical foundation for these statements.

The introduction to the therapeutic interventions (Mindfulness-Based Cognitive Therapy (MBCT) and Compassion-Focused Therapy (CFT)) is helpful, but it would be useful to provide a brief overview of how these therapies specifically address hypothyroidism-related symptoms. Currently, the explanation is more general. A more direct connection between the therapies and hypothyroidism would strengthen the rationale for the study.

The study design is clearly described, but there is some ambiguity around the criteria for participant inclusion. Specifically, the statement "diagnosis of hypothyroidism by an endocrinologist or through a blood TSH test (≤ ng/ml00/10)" is unclear. Please revise to clarify the exact reference range for TSH (e.g., what TSH value indicates hypothyroidism).

The authors mention using “accessible sampling,” which can be prone to selection bias. It is recommended that the authors discuss how they minimized potential biases due to this sampling technique, or consider a more rigorous sampling method (e.g., random sampling) if feasible.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The assertion that “symptoms such as depression, mood changes, and decreased cognitive functioning can impact the patient’s daily activities and affect various aspects of the individual’s life” would benefit from clarification. It is recommended to expand on how these symptoms specifically interact with the patients' activities of daily living. For example, how might cognitive impairment manifest in daily life or how depression affects productivity?

The section on the impact of hypothyroidism on sexual function in women is insightful, but the transition from discussing general symptoms to sexual health could be smoother. To improve clarity, I suggest explicitly linking the psychological effects (depression, mood changes) to sexual dysfunction before moving into the literature review on this topic.

The citation of Roa Duenas et al. (2024) on sexual dysfunction improvements after thyroid-stimulating hormone and thyroid hormone adjustments is relevant. However, the authors should specify the type of "sexual domain scores" that were measured and how this improvement was quantified. Providing these details would make the argument more scientifically precise and clearer to the reader.

The authors discuss "quality of life" from both a psychological and physical perspective, citing multiple sources. It may be beneficial to define "quality of life" more precisely at the beginning of this section, as its meaning may vary depending on the context (e.g., clinical vs. psychological outcomes). The inclusion of the WHO’s definition is helpful but should be followed by a brief explanation of its relevance to the hypothyroidism context.

It would be beneficial to include a more detailed description of the control group. Specifically, how was the control group treated during the study? Were they given any placebo or standard care, and how was this managed to avoid confounding factors that might affect the outcomes?

The authors claim a significant difference between the post-test scores for sexual function between the experimental groups and the control group. However, the authors should report the statistical tests used to confirm this difference, including p-values and effect sizes, for full transparency and scientific rigor.

The interpretation of the greater effectiveness of MBCT compared to CFT for improving sexual function is interesting, but the explanation involving "imagery visualization techniques" could be expanded. A brief citation or explanation of how these techniques specifically work within MBCT to improve sexual function would add depth to the argument.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor’s decision: Accepted.

Editor in Chief’s decision: Accepted.

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