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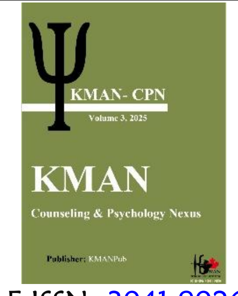

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Predicting the Quality of Life of Mothers of Children with Special Needs Based on Stress-Coping Strategies and Level of Acceptance

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the first paragraph of the introduction, the sentence “Quality of life is one of the most comprehensive indicators of psychological, social, emotional, and functional well-being” is conceptually acceptable but too general for a scientific introduction. The paragraph should define whether the study uses individual maternal quality of life, family quality of life, health-related quality of life, or caregiver quality of life as the primary construct. This distinction is essential because the instrument appears to assess family quality of life, whereas the manuscript repeatedly interprets the outcome as mothers’ personal quality of life.

In the measures paragraph for the Quality of Life Questionnaire, the manuscript reports prior reliability of 0.95 and current reliability of 0.89 but does not report reliability for the eight dimensions. Because the instrument includes several domains such as family interaction, parenting, emotional well-being, physical well-being, disability-related support, cultural and spiritual life, leisure time, and general awareness/reporting, the authors should provide either subscale reliabilities or justify using only the total score. This is particularly important if the total score is interpreted as a multidimensional construct.

In the measures paragraph for the Ways of Coping Questionnaire, several Cronbach’s alpha values are low, such as distancing = 0.546 and seeking social support = 0.567. These values fall below commonly accepted thresholds for internal consistency. The authors should address this psychometric limitation explicitly, report the reliability of the aggregated problem-

focused and emotion-focused scales in the present sample, and explain whether any subscales were excluded, retained, or combined. Without current-sample reliability, the regression estimates may be attenuated or unstable.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the introduction paragraph beginning "The concept of quality of life among mothers of children with special needs should be understood within the broader ecology of caregiving," the manuscript presents a broad ecological argument but does not specify an explicit theoretical model. I recommend grounding the study in a named framework, such as Lazarus and Folkman's transactional model of stress and coping, family systems theory, or the family quality-of-life model. The current theoretical rationale is descriptive rather than model-driven, which weakens the justification for selecting coping strategies and parental acceptance as predictors.

In the paragraph beginning "One of the most important psychological mechanisms influencing quality of life is coping," the manuscript divides coping into problem-focused and emotion-focused strategies, but the Ways of Coping Questionnaire includes eight subscales with mixed adaptive and maladaptive content. Please explain exactly which subscales were classified as problem-focused and which were classified as emotion-focused. Without this information, the negative association attributed to "emotion-focused coping" is difficult to interpret scientifically because positive reappraisal and emotional expression may have different implications than avoidance or distancing.

In the introduction paragraph stating that "emotion-focused coping involves attempts to regulate emotional distress," the discussion later treats emotion-focused coping primarily as maladaptive. This creates a conceptual inconsistency. The manuscript should distinguish adaptive emotion-focused coping, such as positive reappraisal and emotional processing, from maladaptive emotion-focused coping, such as denial, avoidance, and withdrawal. Otherwise, the conclusion that emotion-focused coping lowers quality of life may overgeneralize from a broad construct that contains both protective and risk-related components.

In the paragraph beginning "Evidence from different populations supports the central role of coping strategies," the manuscript relies on breast cancer patients and healthcare professionals to justify the maternal caregiving model. While these studies are relevant, the argument would be stronger if the authors prioritized evidence from parents of children with disabilities, developmental disorders, autism, intellectual disability, or chronic pediatric conditions. The cross-contextual comparisons should be explicitly framed as supplementary rather than primary evidence, because role demands and coping contexts differ substantially across these populations.

In the paragraph beginning "Spiritual and religious coping are additional dimensions of coping and acceptance," the manuscript introduces spiritual coping but does not measure it. This paragraph may distract from the empirical model unless it is clearly linked to the study variables. I recommend either shortening this section or adding a clear explanation that spiritual coping is a contextual factor beyond the scope of the present analysis. At present, the paragraph raises an important construct but leaves it untested, which may make the conceptual scope appear broader than the actual design.

In the final introduction paragraph, the sentence "The aim of the present study was to predict the quality of life…" should be accompanied by explicit hypotheses. For example, the authors should state that problem-focused coping and parental acceptance are expected to positively predict quality of life, whereas maladaptive emotion-focused coping is expected to negatively predict quality of life. A correlational-predictive study benefits from pre-specified hypotheses because it reduces the impression of post hoc interpretation and helps align the introduction, analysis, and discussion.

In the methods paragraph under "Study Design and Participants," the manuscript states that participants were mothers of children "under the age of 16 who had referred to a rehabilitation center affiliated with Tehran University of Medical Sciences." Please specify the recruitment period, location, type of rehabilitation center, diagnostic confirmation procedure, and whether

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children's diagnoses were based on clinical records, physician reports, educational classification, or maternal self-report. These details are essential for evaluating sample validity and replicability.

In the same methods paragraph, the sentence "The final sample size was calculated using Cochran's formula" requires more detail. Please report the formula inputs, including population size, confidence level, margin of error, estimated proportion or variance, and any adjustment for nonresponse. Since the accessible population was only 270 and the final sample was 159, readers need to understand why this sample size was statistically sufficient for a multiple regression model with three predictors and several possible control variables.

Response: Revised and uploaded the manuscript.

2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.

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