



## **Academic Review**

**Date:** 29 July 2024

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**Expert:** Dr. Dorit Naot

**Managing Editor:** Dr. Meredith Armstrong

**Short Title:** Postpartum Depression's Impact on Maternal Quality of Life

**Details:** The study investigated the relationship between postpartum depression symptoms and quality of life in the first year after childbirth and compared between Arab and Jewish participants. The research question is interesting and important and could inform future interventions in the two ethnic groups. The questionnaires used in the study are well-validated, and the large number of participants is a strength of the study. In General, the manuscript is structured well.

### **Comments**

#### **a. Content Review**

The main concern is that not all the conclusions are backed by the evidence.

- Based on the adjusted logistic regression model (Table 4), the authors state that 'Arab mothers were 1.62 times more likely to have lower QoL.' However, the P for this OR is

0.090; therefore, there is no evidence that ethnicity affects the QoL. This should be corrected throughout the manuscript.

- Based on the analysis presented in Table 3, the authors state that ‘The impact of PPD on QoL dimensions indicated significant differences in physical health, mental health, and social relationships, with Jewish mothers generally faring better across these dimensions than Arab mothers, particularly among those with EPDS  $\geq 13$ .’ However, no statistical test was used to compare the two ethnic groups, and no conclusions can be drawn about differences between Arab and Jewish mothers in the various QoL dimensions. It would be best to consult a statistician and use a statistical test to analyze the ethnic differences. In addition, the test used in the current version of Table 3 is not specified, but I assume (according to the Methods section) that it was a t-test. Was there any correction for multiple comparisons?

#### **b. Clarifying Arguments and Definitions**

- Because of the study's cross-sectional design, no causal relationship can be determined between the variables. The authors refer to this fact in the study limitations paragraph. I think the expressions describing the relations between variables should be used more carefully throughout the manuscript. ‘The impact of PPD on QoL’ implies causation. Measuring the effect of PPD on QoL can describe a specific analysis used in the study, but the outcomes should be stated more cautiously. The argument can be reversed to hypothesize that women with lower QoL are at risk of developing PPD, and therefore, ‘association’ is probably more accurate to describe the relation between the two variables.
- The definition of QoL is not entirely clear. Two measures of QoL were used in the study: the continuous variable of QoL score, and the categorical variable of ‘low (or lower) QoL,’ which was defined by dividing the study population into two groups. I have added the following sentence to the Methods section: ‘We defined the categorical variables

'lower QoL' and 'higher QoL,' using the mean of the WHOQOL-BREF score in the entire study population (87) as the cut-off point.' Although I generally try to avoid abbreviations, I suggest you consider naming the variables 'L-QoL' and 'H-QoL' to avoid confusion between the continuous QoL variable and the categorical variable. (There is less confusion with 'PPD' because the cutoff separates the group to women with/without PPD symptoms, and unlike 'lower/ higher,' it is clearly categorical).

- The scale and cutoff point for high/ low QoL is unclear. As in my previous communication:

*'According to the Methods section and Table 3, only a partial set of 16 questions were used from the WHOQOL-BREF questionnaire. With a 5-point Likert scale, should the scores be between 16 and 80? A range of 26–130 and a cutoff point of  $\geq 87$  between lower and higher QoL are mentioned in the methods, and in Table 3, 93–94 is the cutoff. Could you please clarify if the scale is 26–130 or 16–80 and what the cutoff point was?'*

In response to my question, you suggested removing Table 3. That does not solve the problem. What scale and what cutoff point were used for the analyses?

### **c. Section-Specific Comments**

#### ***Introduction***

I have modified the Introduction structure slightly to create a better flow and progress from general to more specific facts that lead better to the scientific question of the current study.

The revised Introduction follows this outline:

- PPD - definition, symptoms, and effects on mother and child
- QoL – definition, relevance to the postpartum period
- The association between PPD and QoL
- PPD prevalence – globally and in Middle Eastern countries

- PPD prevalence in Israel and the disparity between the Arab and Jewish populations.

## **Methods**

This section is structured well. However, two of the subsections are unclear.

- The sample size calculation (please see comment in the manuscript).
- I have revised the statistical methods subsection; please check the new version.
- The following sentence (under statistical methods) requires your attention: 'All independent variables associated ( $p < 0.05$ ) with PPD in the bivariate analyses were included in the multivariate analysis.' Please see my comment in the manuscript.

## **Results**

This section is generally structured well. I have suggested changes that aim to present the tables more accurately.

- **Table 1** Includes continuous and categorical variables, but the statistical test the P value refers to is not specified. For example, for each of the following variables: age, number of children, and level of education, the P value is presented in the row of the continuous variable analysis: was it calculated by a t-test or the Chi-square presented in the following rows? I had mentioned this point in my previous communication, but I found the response unsatisfactory ('...we do not write below the table...'). Please see, for example, Tables 1 and 2 in the paper by Shwartz et al. (Midwifery 70 (2019) 54–63).
- **Table 2**  
I would like to suggest an alternative arrangement for the data presented in this table. I would split the table into three parts: the mean scores of the questionnaires compared between Arab and Jewish women (2a, t-test), and contingency tables presented in 2b and 2c. Contingency tables usually present the exposure in rows and the outcome in

columns. Tables 2b and 2c are organized to answer the questions: what is the odds ratio between Arab and Jewish women experiencing PPD / low QoL?

**Table 2a**

	EPDS score Mean (SD), range	QoL score Mean (SD), range	P
Jewish women (N=478)	8.1 (5.7), 0–30	95 (16.0), 41–130	?
Arab Women (N=123)	9.7 (5.4), 0–25	87 (22), 33–130	
Total (N=601)	8.5 (5.7), 0–30	93 (17.9), 33–130	

**Table 2b**

	EPDS<13	EPDS≥13	OR (95% CI), P
Jewish women			
Arab Women			

**Table 2c**

	Low QoL	High QoL	OR (95% CI), P
Jewish women			
Arab Women			

- **Table 3** is missing the information that the values are means (SD).

**Conclusions** (including the conclusion section in the abstract)

These sections will require revision after all the suggestions have been considered. The main issues are (1) responses to specific QoL questions/dimensions were not compared between Arab and Jewish participants, and (2) ethnicity had no significant effect on QoL in the adjusted logistic regression model.

**d. An Additional Comment regarding the Research Question**

In my previous communication (copied below), I suggested that the study question is somewhat ambiguous.

## *Research questions*

*The data collected in the study includes:*

- 1. Ethnicity: Arab or Jewish (and other sociodemographic characteristics)*
- 2. PPD score*
- 3. QoL score*

*The exact study questions need to be clarified. The possible questions are:*

- 1. Is PPD associated with ethnicity (different between Arab and Jewish women)*
- 2. Is QoL associated with ethnicity (different between Arab and Jewish women)*
- 3. Is QoL associated with PPD?*
- 4. Is the association between QOL and PPD different between Arab and Jewish women (is there interaction)?*

I am not sure I have made my point clear. I looked for a similar paper that formed the research question clearly to serve as an example. The publication 'The Relationship Between Satisfaction with Life and Depression Symptoms by Gender' by Gigantesco et al.

(10.3389/fpsy.2019.00419) has a similar structure. I think the way these authors framed the research question works better as an example for you to give it some thought.

*'In the light of the ascertained relationships of life satisfaction with depression and other indicators of poor mental health (28) and of the uncertainty about gender differences in this relationship, we designed this study to investigate whether satisfaction with life was a useful proximal indicator of depressive condition among both male and female individuals. Specifically, the objective of the present study was to investigate whether satisfaction with life was related to depressive symptoms depending on gender, in a sample consisting of women and men, some of whom screened positive for a depressive disorder.'*