Author's response to reviews

Title: Comparing health-related quality of life of employed women and housewives: a cross sectional study from southeast Iran

Authors:

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Author's response to reviews: see over
Dear Wella,

Manuscript ID: 1342081064627085
Comparing health-related quality of life of employed women and housewives: a cross sectional study from southeast Iran
BMC Women’s Health

Thank you for your e-mail. We found the reviewers’ comments very helpful. Thus, we revised the manuscript as recommended. Please find the point-by-point responses as follows:

Reviewer: Dr. Pawel Zagozdzon
Major Compulsory Revisions:
The major methodological concern is related to lack of controlling for several important confounders in the analysis. There are significant differences in major characteristics between housewives and employed women (Table 1). Employed women were older and better educated. These differences were not addressed in the analysis of health-related Quality of life in this paper. It is well known that education positively affect quality of life. The authors fail to adequately characterized groups under the study. It would be interesting to see the differences regarding the income and number of children between the two groups as aforementioned characteristics may also influence quality of life in women. In order to correctly address the question posed in the paper some kind of approach to the multivariate analysis should be proposed to assess the role of employment independently of other variables.
The data were reanalyzed as recommended and was acknowledged in the Methods and Table 2 was revised:
a. Descriptive statistics were used to explore the data. Quality of life was compared between employed women and housewives using analysis of covariance (ANCOVA) while controlling for age, education and income as covariates. The data were analyzed using SPSS version 19.0.
b. The revised Table 2

<table>
<thead>
<tr>
<th></th>
<th>All (n = 220)</th>
<th>Employed women (n = 110)</th>
<th>Housewives (n = 110)</th>
<th>P*</th>
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<tbody>
<tr>
<td>Physical functioning</td>
<td>82.5 (20.3)</td>
<td>81.3 (20.2)</td>
<td>83.6 (20.4)</td>
<td>0.39</td>
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<td>Role physical</td>
<td>52.9 (39.7)</td>
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<td>Bodily pain</td>
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<tr>
<td>Social functioning</td>
<td>71.3 (20.9)</td>
<td>71.8 (20.6)</td>
<td>70.9 (21.2)</td>
<td>0.96</td>
</tr>
</tbody>
</table>
Minor Essential Revisions:
There are also a few minor issues that would need attention before it could be published and I have listed some comments below:

1. Page 2 line 6 and Page 4 line 7: authors use term “case-control study” for design and methods used in their work. From technical point of view this is not case-control study as there are no cases of any disease at the start of investigation but simple two groups comparison coming from survey and groups were distinguished according to employment status. Obviously, the data from survey can be analysed like in case-control study if definition of “cases” is applied. The cases would be those with worse health-related QoL.

   Thank you. This was corrected:

   This cross-sectional study was carried out during 2009-2010 to compare quality of life of housewives and employed women in Zaheden, Iran.

2. Page 3 line 16: data on Iranian women labor force participation need to have adequate reference.

   This was provided as recommended.

3. Page 5 line 1 to 7: it seems that the standard version of SF-36 was used in the standard way. Therefore it is not necessary to described it in details.

   The paragraph about the SF-36 was shortened as suggested.

4. Page 5 line 9: non-parametric version of ANOVA should be considered as the QoL data usually does not have normal distribution.

   In general this is true. However, we examined the data for normality and found that although the data was skewed but all were in the acceptable range except for physical functioning. Thus we used parametric tests.

5. Page 7 line 5: Authors refer to study limitation. They should emphasize cross-sectional character of this study and the lack of information about important confounders e.g. the income level of the participants.

   This was revised and the following section was added to the Discussion:

   Limitations

   This study had some limitations. Firstly it was a cross sectional study and thus making the findings limited. Secondly, there were some confounders for which we did not collect data for instance psychological status of the women at the time of completing the questionnaire. Thirdly, we did collect the data for illiterate women by interviews while for others we used self-completion method. This might be introduced a bias and should be noted in interpreting the results. Finally, social, economic and environmental issues may be influenced the results for which we were unable to control for.

   This is a informative article and will be an important contribution to the literature in documenting the impact of social participation and employment on women quality of life.

   Thank you.

Reviewer: Dr. Maryam Tajvar
Most of the following revisions are compulsory before I can make a decision on the publication of the manuscript titled “Comparison of health-related quality of life between
housewives and employed women in Zahedan City, Southeast Iran” in BMC Women’s Health. In general, I think the authors need to do some substantial improvements in the quality of their paper and presentation of their data, in addition to significant improvements in the English of the paper.

INTRODUCTION:

Paragraph 2: in sentence “The recent national census in Iran implies that Iranian women’s share of work force is 11%, and currently we have about 2.1 million employed women (10) and 14 million housewives nationwide.”, Ref. No 10 is related to the US not Iran. Also what is the Ref. for 14 million? There is a need for statistics of Iranian women from a sound relevant source. Also change “we” to “Iran”.

Thank you. This was corrected.

Paragraph 3: Ref 12 and 13 reports the information for Iran and Turkey only not everywhere. Please mention this in the text.

Done.

Studies from Turkey and Iran showed that employed women reported higher quality of life score than non-employed women in all aspects of quality of life [12-13].

Paragraph 3: what is the evidence for “Employed women are significantly happier and mentally healthier”? Please mention it.

This was deleted.

Paragraph 3: what do you mean by “the cultural differences”? Please explain it.

This was deleted.

Last sentence: you have claimed that “We wanted to find the different challenges the women of this city face in order to promote their overall health”. It seems that your study has not such an aim and so it is unrelated. I suggest removing this from your paper.

This was removed as recommended:

Considering the effect of women’s health on the overall family health and with regard to lack of coordination in shared responsibility of men and women in family, and considering women’s employment as a minor role alongside the major role of housekeeping, this study was designed to compare quality of life of the housewives with employed women in Zahedan, the capital city of Iranian southeastern Sistan and Baluchistan Province.

- Minor revision: change “third world countries” to “developing countries”.

This was corrected.

MATERIAL and METHODS:

Despite significant changes in the current version of the paper, the methods section still is not satisfactory. Please address the following issues:

- English of this section is particularly poor.

- Your study is not “case-control”. It is just a cross-sectional study and you have two groups of samples.

This was corrected.

- Provide information on the sample size calculation.

The following section was added to the Methods:

Sample size

The sample size used in this study was determined based on a sufficient statistical power (80%) to detect at least 20% differences in quality of life measures between the
two study groups at 5% significant level. As such a sample of 100 women for each group was estimated. However, the actual sample recruited for this study was 110.
- How 22 people from each centre have been selected? What was sampling method to choose these people for the study?
  This was further explained as recommended:
  This cross-sectional study was carried out during 2009-2010 to compare quality of life of housewives and employed women in Zaheden, Iran. A multiple random sampling methodology was used for the selection of women. Initially, the city was divided into five parts (north, south, center, west, and east). Then, two health care centers from each part were randomly selected (a total of 10 health care centers). All married women aged 14 to 45 years attending the centers were eligible to participate in the study.
  - You have initially selected 220 people for your study and it seems that all 100 of your samples have responded. This seems unrealistic. Please provide information on your non-respondents and the response rate of your study.
  There were no non-respondents. In fact we did not collected data on this.
- Paragraph 1: the sentence “The comparison group (housewives) was matched for age and gravity.” needs clarification. What do you mean by matching in one group only? Also what do you mean by gravity?
  This was deleted.
- Paragraph 2: your first inclusion criteria means that you have only studied healthy women, which is not sensible and distorts your study objective.
  This was revised.
- Paragraph 2: in the third inclusion criteria, why you have excluded “obese” women?
  There is evidence that obesity has significant influence quality of life.
- Which version of SF-36 has been used? The Iranian version is based on version 1 or 2?
  We used version 1 and this was indicated in the text.
- The method of data collection in your study for illiterate people was interview whereas for others it was self-completion method. This makes a bias in responses between two groups, i.e. the interviewees are subject to the interviewer bias. You have to justify this and at least report it as a limitation of your study in the discussion section.
  This was acknowledged as recommendation.
- Add the information about the ethical approval of your study to the main text.
  This was added as suggested:
  Ethics
  The ethics committee of Zahedan University of Medical Sciences approved the study. A written informed consent was obtained from all participants.
  - Minor revisions: 1) remove “Material” from the title and keep only “Methods”. 2) spell check is required 3) remove redundant words (Statistical methods were) in the last paragraph.
  Thank you. All were corrected.
RESULTS:
- Selected information of your Table 1 has to be reported in the text too.
  Done.
In all 220 women were studied. The mean (±SD) age of participants was 33.8± 8.9 years. Eighty women (36.4%) had a university degree with a mean (±SD) official
education of 10.8 ±4.9 years. The women's mean income was 1,640,000± ,  Rials per month (Rial is the unit of Iranian currency and officially 12,260 Rials = 1 US Dollar). Employed women were older than housewives (mean age 34.9 vs. 32.8, P = 0.07), and were better educated (mean education years 13.5 vs. 7.9, P < 0.0001). The results are shown in Table 1.

- Table 2 provides only a raw comparison between the two groups. The differences in the scores of SF-36 could be due to age or other potential confounders. It would be best if you could do additional analyses (multivariable) to control for the effect of these confounders.

The data were re-analyzed as suggested. And this was addressed both in the Methods and the Results.

a. Descriptive statistics were used to explore the data. Quality of life was compared between employed women and housewives using analysis of covariance (ANCOVA) while controlling for age, education and income as covariates. The data were analyzed using SPSS version 19.0.

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<td>63.1 (20.9)</td>
<td>66.2 (17.9)</td>
<td>60.0 (23.2)</td>
<td>0.67</td>
</tr>
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* Derived from analysis of covariance while job was treated as fixed factor and age, education and income as covariates.

- In Table 3, I am not clear why you have compared the HRQoL of your sample with that of Iranian women. I think this is totally senseless, because the information of Iranian women provided in your study is based on just one study (Montazeri) and the samples in two studies could have different characteristics and so not comparable. I suggest you remove all of this section but it worth you keep it for discussion of your findings.

This was removed.

DISCUSSION:
- The cross-sectional design of your study is its main limitation, because in cross-sectional studies you can not determine the cause-effect association. You have to clearly explain it in your study.

This was addressed as recommended:
This study had some limitations. Firstly it was a cross sectional study and thus making the findings limited.

CONCLUSION:
- As I said, because of the cross-sectional nature of your study you can not judge about the effectiveness of employment on HRQoL. At most you can claim that there may be
an association between the aspects of HRQoL and employment. Social participation is a different concept than employment. You can not recommend having more social participation to have better HRQoL based on your findings. Instead, you could have more relevant recommendations based on your findings.

The Conclusion was revised as suggested:
The findings from this study indicated that there were no significant differences in quality of life between employed women and housewives. However, employed women scored higher on the SF-36 especially on the role emotional, vitality, and mental health. The findings suggest that perhaps associations exist between the aspects of health-related quality of life and employment. Indeed improving health-related quality of life among housewives seems essential.

Reviewer: Dr. Andreja Barišin
This is an article on the effect of quality of life between housewives and employed women. The comparison between different social groups is interesting, but not new. Revisions are needed in some parts.

Major concerns:
1. Statistics: The authors have used data from a small sample (n=110) but sufficient for statistical analysis that is correct. Only one comment the authors use the p-level <0.02 as cut-off for statistical significance perhaps it is better <0.05.
   Thank you. This was corrected.
2. Interpretation in Discussion: at the beginning of the discussion is more involved and comments (concludes) the results of other studies and too little compared to this study and the results obtained. Some limitations could have avoided by separating the participants with mental health diagnoses and psychological condition (before completing the questionnaires)
   To comply with the recommendation, the Discussion was revised and we discussed further about the findings. We hope you find the revision satisfactory.
3. Conclusions: The conclusion of studies of ‘Comparison of health-related quality of life between housewives and employed women in Zahedan City, employment should be promoted among housewives in order to maintain a higher HRQoL in these conditions, it is correct.
   The Conclusion was revised:
The findings from this study indicated that there were no significant differences in quality of life between employed women and housewives. However, employed women scored higher on the SF-36 especially on the role emotional, vitality, and mental health. The findings suggest that perhaps associations exist between the aspects of health-related quality of life and employment. Indeed improving health-related quality of life among housewives seems essential.

Minor concerns:
1. Tables: after number put point instead of a colon (example Table 1.), and align results in tables (justify)
   Thank you this was corrected.
   Thank you. This was corrected.
I hope you find the revisions satisfactory.
Wish you all the best.
Kind regards
Ali Montazeri