Author's response to reviews

Title: Socio-economic inequalities in curative health-seeking for children in Egypt: Analysis of the 2008 Demographic and Health Survey

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Response to reviewers

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Dear Professor Arah,

We would like to express our appreciation for the opportunity to revise our manuscript. We thank the reviewers for their insightful comments and feedback, which contributed significantly to improving the quality and clarity of the manuscript. Please find below our responses to all their comments.

In addition, you had asked that the overlap in phrasing between this manuscript and a previous article “A mediation approach to understanding socio-economic inequalities in maternal health-seeking behaviours in Egypt” be minimised. I therefore propose to change the title of the current article to “Socio-economic inequalities in curative health-seeking for children in Egypt: Analysis of the 2008 Demographic and Health Survey”. Please let me know if you have any further questions or comments.

Yours Sincerely,

Lenka Benova, on behalf of the author team
Response to reviewers

Reviewer #1

Comment 1: This article deals with socioeconomic predictors of health-seeking behaviour for children in Egypt. From Public Health view, the topic is of relevance, and the study is based on data from a large survey that was conducted in 2008. However, since 2008, the Egyptian society may have experienced major changes (in 2011, the Arab Spring started in Egypt) and the results may not exactly reflect the current situation. The research question is well defined, the methods are described in detail, the data are sound. The whole manuscript is written well, and all tables/figures were thoroughly designed.

Response: We thank the reviewer for the thorough assessment of our work and the detailed feedback. We greatly appreciate the questions and suggestions that have improved this manuscript.

Comment 2: Minor essential revision Fifth paragraph (Page 5 Line 20) “… 7.8% of children were reported to have had ARI and 8.5% diarrhea.” I suppose this should be 7.6% and 8.4%, respectively, since these values are given in the abstract and in table 2.

Response: We thank the reviewer for noticing this mistake, which has been corrected in the manuscript.

Comment 3: Discretionary revision First paragraph (Page 6 Line 15) You mention that more than one child per family were included in your analysis, if there lived 2 or more children in the households. According to table 1, 60.5% of the families had 2, 3, or 4 children. I am not sure, whether these observations are really independent (I suppose they are not), and whether it would have been better to consider only 1 child per family (e.g., only the youngest of those for whom diarrhea and/or ARI was reported), since the mothers’ behaviour in case of illness may be very similar for all her children. On the other hand, only a few families may have reported diarrhea/ARI for more than one child within the observation period of 2 weeks. You may just report, how many mothers reported diarrhea and/or ARI for 2 or more of their children. If this number should be considerable, you may further conduct your analysis by including only one child per family, and compare the findings with the results from the current manuscript.

Response: We thank the reviewer for this comment. A large proportion of children in the initial sample of all children <5 lived in households with one or more children of the same age. However, the proportion of children in the samples of ill children (ARI or diarrhea) living in the same household as other sick children was much lower (12% for diarrhea and 19% for cough). The purpose of adjusting for the number of children in the household (all children, not ill children) was to adjust for the element of adults’ availability for health-seeking. In the analysis of health-seeking, we acknowledge that clustering of health-seeking behaviours may occur in instances where more than one ill child per household contributed to the analysis. However, we were reluctant to exclude children from the analysis sample – systematically excluding older ill children would result in health-seeking behaviours, particularly the “sought treatment” outcome, to be overestimated, because the proportion of younger children for whom treatment was sought was higher than older children. We could have also chosen a random child from households where more than one child was reported ill, yet this would have resulted in a reduction of sample size and therefore in our power to detect differences. In order to address this issue in our original analysis, we conducted sensitivity analyses where instead of setting clustering at the primary sampling unit level, we set the clustering effect to occur at the household level. The results of the sensitivity analysis (not shown in the manuscript) were not different from the findings from the main findings as presented in the manuscript, and we are therefore confident that the presented findings are not influenced by this phenomenon.

Comment 4: DR Second paragraph (Page 6 Line 17) Measure of SEP: The quoted source (Reference 14) explains which variables were used for constructing socio-cultural capital and economic capital, but not how both variables are calculated. Evidently, both variables can also have negative values (e.g., in table 1, the mean for children with diarrhea and ARI). You could e.g., add a reference that explains how the variables are calculated.
Response: We thank the reviewer for this suggestion and have added a reference that addresses the methodology of calculating the main SEP exposures in full detail.

Comment 5: MER Third paragraph (Page 7 Lines 3-5) “The first step in assessing health-seeking behaviours for curative care to analyse the reporting of diarrhea and ARI in the two-week period preceding the survey among children under-five years (0-59 months old).” Is there a word missing in this sentence, e.g., “was” ? … was to analyse the reporting….

Response: We thank the reviewer for noticing this omission. It has now been corrected in the manuscript.

Comment 6: MER Second paragraph (Page 10 Lines 19/20) “Among the 1,952 children for whom one or more illness was reported, 28.7% had only fever. Among the sample of 1,390 children with diarrhea or ARI…” I suppose, 562 had only fever (1,952-1,390), thus 562/1952 = 28.79% à better round it to 28.8%.

Response: We thank the reviewer for this suggestion and have corrected this in the manuscript.

Comment 7: Paragraph ARI health-seeking (Page 12 Line 25/26) “socio-cultural capital was marginally associated with lower odds of reporting ARI (p=0.066).” This p-Value is reported with 0.061 in Table 4. Please check/correct.

Response: We thank the reviewer for noticing this typo and have corrected the manuscript.

Comment 8: Paragraph Mediation analysis ARI health-seeking (Page 14 Line 13) “Table shows that neither…” à the number of the table (Table 5) is missing.

Response: We thank the reviewer for noticing this omission. It has now been corrected in the manuscript.

Comment 9: MER Page 15 Line 7 “… to determine whether this difference in period prevalence is a result …” à supposably, one “is” should be deleted.

Response: We thank the reviewer for noticing this typo and have corrected the manuscript.

Comment 10: DR I would suggest to add in the limitations that the findings are based on data from 2008, and that owing to the political events/changes the prevalence rates/predictors may vary now.

Response: We thank the reviewer for this suggestion and have added a statement to this effect in the limitations section.

Comment 11: Table 2 (DR) I noticed that the item for whether treatment was sought is named “sought treatment” for diarrhea (table row Nr 2) and “received treatment” for ARI (Table row Nr 6). It might be better to use the same term for both diseases.

Response: We thank the reviewer for noticing this discrepancy and agree that consistency in terminology is necessary. We prefer the term “sought treatment” as this more accurately describes the health-seeking action (the term “received treatment might imply that the correct clinical action was taken, which is not the type of data we had available for this analysis). This discrepancy has been corrected in Table 3 and in Table 2.

Comment 12: Reference list (MER)

<table>
<thead>
<tr>
<th>Comment</th>
<th>Response</th>
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<tbody>
<tr>
<td>I noticed that some Journals are abbreviated (e.g., Nr 3, Nr 22 while for other references the journal is given with the full name (e.g., Nr 4, Nr 7)</td>
<td>These have now been corrected, except in reference number 3, as BMJ Open is the full title of the journal - <a href="http://bmjopen.bmj.com/">http://bmjopen.bmj.com/</a>).</td>
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<tr>
<td>The journal is given with “Lancet” in Ref 28 , and with “The Lancet” in Ref 8.</td>
<td>This has been corrected.</td>
</tr>
<tr>
<td>Reference 9: please check whether the link still works. I tried, but did not get to the page.</td>
<td>We thank the reviewer for noticing this website no longer exists. We have replaced this reference with a more updated report/website.</td>
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Reviewer #2

Comment 1: Discretionary Revision: 1. Evidence showing the relation socio-economic position affects the child health seeking behaviour must be explored in the introduction part. Only wealth as a predictor was explained in the introduction part. Although authors had mentioned that there is no studies in Egypt to substantiate the relation between SEP and health seeking behaviour but studies from different countries with same economic development as Egypt can be used to quantify such relations.

Response: We thank the reviewer for the thorough review of our work and the positive feedback. We also appreciate the insightful suggestions and comments that have enriched this article. This comment related to the reviewer’s feedback in Comment 5 below, where we address this point.

Comment 2: Discretionary Revision: More over a logical sequence should maintained for clear understanding of the readers. 2. Page 9, First paragraph (statistical analysis): How the categorical and continuous variables were presented and which statistical tests were used to determine association in the results may be included for clear picture. (it can be ignore as in the tables these are explained in the foot notes)

Response: We thank the reviewer for this suggestion and have added a description of the presentation and testing of differences in the Statistical Analysis section of the Methods.

Comment 3: Minor Essential Revisions: Page 4, line 11: Abbreviation "HIO" should be expanded.

Response: We thank the reviewer for this suggestion and have clarified this in the manuscript (Health Insurance Organisation).


Response: We thank the reviewer for this suggestion and a reference to the 2008 Egypt DHS report has been added.


Response: We thank the reviewer for this suggestion and added references to support our statement and to point readers to evidence about the phenomenon of socio-economic inequalities in health-seeking behaviours for child illness in other countries.

Comment 6: Minor Essential Revisions: Page 6, line 23: Authors have written ten binary variables but mentioned only five. The variable under "utilities" and "household asset ownership" should be included for clear understanding of methodology.

Response: We thank the reviewer for this suggestion and have expanded this section to list the ten component variables in detail.

Comment 7: Minor Essential Revisions: Page 12, line 26: p value mentioned does not match with the figure mentioned in the table 4.

Response: We thank the reviewer for noticing this typo, which had been corrected.

Comment 8: Minor Essential Revisions: Page 24, line 24: This study is about child health seeking behaviour, sudden comparison with maternal health seeking behaviour seems inappropriate. Author can rephrase the sentence.
Response: We thank the reviewer for this suggestion, which seems to refer to the first sentence of the Conclusions. This sentence has been rephrased to focus on the findings of the child health-seeking analysis.

Comment 9: Minor Essential Revisions: Discussion: Study findings were not well compared with other studies/evidences. Although authors mentioned this is first study in Egypt using DHS data but they can compare with studies from different countries.

Response: We thank the reviewer for this comment. We agree with the reviewer that a broader comparison with determinants of child health-seeking in other countries might be desired. However, we feel that due to the study methodology employing two SEP measures specifically constructed to Egypt, differentiation of the various steps of health-seeking and mediation analysis, comparisons with other studies would be uninformative. Much of the literature on socio-economic gradients focuses on describing coverage and content of care; we went beyond this analysis in the particular environment of Egypt’s health system to provide an in-depth perspective of determinants for health-seeking. We have added a discussion point to this effect in the Discussion section (page 16).

Comment 10: Minor Essential Revisions: Conclusion: Conclusion section should be short, explaining the implications of the study. But Authors mentioned many of their study finding in the conclusion section, which should placed in discussion section.

Response: We thank the reviewer for this suggestion. We have moved several paragraphs with interpretations from the Conclusion section to the Discussion section to make the Conclusion section short and concise.