Author’s response to reviews

Title: A quantitative exploration of the sociocultural context of teenage pregnancy in Sri Lanka

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Author's response to reviews: see over
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Many thanks for the very useful comments from the three reviewers. We have taken all of these comments on board and made extensive changes and substantial improvements to the manuscript as detailed below.

Reviewer 1: Kabiru Salami

Major Compulsory Revisions:

1. Since the title of the study indicates “A quantitative exploration of …………”, your readers would be willing to see results presented with some statistics where applicable, rather than being described purely qualitatively as done in the abstract. This is more important at abstract section.

   ❍ Statistics have been inserted into the abstract.

2. Following up on the three categories of respondents at different stage is a big task that this study has taken up; however, there is little information about the process of following up, the challenges and difficulties faced in the process. These should be properly addressed to pave ways for future researchers who are also intending to follow up on the approach in their own study.

   ❍ It appears our method was not clearly explained. Questionnaires were administered once only. This has been made clear in the section on ‘data collection’.

3. On page 7, it was stated that ethical approval was taken from Ethics committee of the SLMA, the University and relevant authorities. I feel the authors should highlight specifically, the referred ‘relevant authorities’ in Sri Lanka apart from the ethics committee of SLMA and the University. This information may help future researchers too.

   ❍ It has now been mentioned that approval was obtained from the line Ministry of Health, Provincial Ministry of health and Provincial Director of Education.

4. It is not ideal to start sentences with figures. This is common in this paper especially at results section of the paper. It should be avoided. Serious editorial is necessary not only at results section but in the overall manuscript.

   ❍ We agree. Sentences beginning with numbers have been edited. The manuscript has been proof read and edited to improve readability.
5. On page 8, 2nd paragraph, 2nd line “……of other pregnant teenagers”. Please give detail of other pregnant teenagers; are they pregnant but not recruited in the study?

Ł This has now been clarified.

6. On page 7, Authors should insert “the local languages” after “….Tamil and Sinhala” on line 1.

Ł Yes, we have done as suggested.

7. Also there is concern whether the partners reported the reactions of the family to their own career after their families have accepted to support their pregnant wives. It is necessary to know whether the parents only agreed to support partner’s wives in taking care of the pregnancy at the expense of the boy/partner’s career. It is important know if the parent supported the pregnancy and at the same time the partner's career.

Ł We agree that these are important questions but they were beyond the scope of the structured questionnaire survey that we used in this study. We have added a comment in the discussion to highlight these areas for future investigation.

8. Page 12, 1st paragraph is too clumsy with the questions raised in the paragraph, therefore needs reconstruction.

Ł We agree - this paragraph has been re-written

9. Also page 12, last paragraph: Interpretation of data on ‘condom and pills as best known’ has to come with caution since respondents were recruited at clinics.

Ł The word ‘respondent’ has been inserted to make it clear that we are referring to our sample and not to people outside of the sample.

10. Page 13, 1st paragraph, subheading ‘factors associated with unplanned teenage pregnancy’ is too clumsy to understand. Please reconstruct.

Ł We agree - page 13 para 1 has been rewritten
11. Page 17-18, most of the information provided as strength and limitation in this paper could fit in more perfectly in the methodology section. They are information already known before data collection while some of them are not even necessary.

Ł We suggest that it is acceptable and common practice to keep the strengths and limitations section where it is currently positioned and have retained it in its original position. We seek editorial guidance on whether this suggested change is deemed necessary.

Minor Essential Revisions:

12. Authors should crosscheck for omission of words which can hamper the meaning of key and relevant issues raised in the paper.

Ł The paper has been carefully edited to improve readability and clarity.

Reviewer 2: Sunita Bandewar

Reviewer’s report:

1. Is the question posed by the authors well defined?

Yes, the research question is relevant. However, I would like to indicate two areas that require attention. It would help to strengthen the piece and enhance the relevance of the topic of enquiry (research question). They are:(a) the data collection since took place between the years 2007 (July) and 2009 (June), it warrants reporting of relevant empirical and theoretical work in the recent past. A quick review references reveals that the most recent publication quoted/sourced is from the year 2011. I suggest that authors respond to this gap by updating the literature or stating in response to the editor that there is nothing relevant to source from the year 2011 onwards to date on the topic of enquiry and allied themes.

Ł We agree - a number of newer references have been added and we have now discussed our findings in light of these.

Malin Jordal, Kumudu Wijewardena and Pia Olsson Unmarried women’s ways of facing single motherhood in Sri Lanka – a qualitative interview study BMC
Women's Health 2013, 13:5


(b) Authors justify the relevance of the work by saying it would help feed into the state policies or by saying “…teenage pregnancy is attracting increasing policy attention in Sri Lanka because of the risks it poses to maternal and infant health as well as to social and economic well-being.”. However, there is almost nothing sourced that would shed light on the current policies and approaches of the Shri Lankan government, and as to how it is constrained in absence of either no or limited empirical evidence on the topic. The reference sourced is that of the WHO (Ref no 1) but not that of the actual govt policies even if they might be only in making.

Thank you for pointing out this important oversight. The lead author is a government employee and references to relevant policies that are in place and in development have now been added to the introduction section.

2. Are the methods appropriate and well described?

The ‘method’ section is rather ambiguous. It requires clearer presentation relating to some of the key aspects as below:

(a) It would be helpful to state up front and at the outset that it is a clinic based study (which has inherent limitations of its own)

It is now clearly stated that the study sample was drawn from a population health register and conducted in health clinics. Limitations of the sample are reported in the discussion section.

(b) It is not clear how the 50% estimated figure about the teenage pregnancy amongst married girls is arrived at. Sourcing any documents/reports that mentions these estimates will be necessary.
A 50% estimate was used because, in the absence of information on the actual percentage, this approach results in a conservative sample size (i.e. the largest sample size) and therefore more precision in the estimates produced. This has now been made clear.

(c) Does this mean that only married girls were included in the study? It seems so. Again, if so, it will be helpful to state it upfront.

No, all registered pregnant teenagers were included regardless of marriage status. This has been made clear in the methods section.

(d) Not clear if the clinics (179 - area + 21 – estate) included in the study to draw the sample are private or public or both. Please state it clearly given the international audience of the journal.

The study context now clearly states that these were public funded community maternity services. Free health care at the government clinics is available to all.

(e) Also, since it is a clinic based study, it is logical to talk about how these 200 clinics were selected? What is the universe, that is, how many clinics in the district of study?

We apologise for the fact that the methods section was not completely clear. It is important to note that the sampling frame for the study was the antenatal registers maintained by the community midwives. While acknowledging that some adolescent girls who become pregnant will inevitably have been overlooked, antenatal care coverage is extremely high in the area and these registers are well maintained, meaning that they provide a good sampling frame from which to achieve a random, representative sample. The questionnaires were administered to adolescent girls while they were in attendance at their clinic appointment, for convenience. All adolescents who are registered attend the clinics for antenatal care. The section of the manuscript that describes the sampling approach has been completely rewritten to make it clearer to the reader. The sample was drawn from 179 non-plantation midwives and 21 plantation out of a total of 249 midwives.

(f) It appears that about 1 - 3 women per clinic were picked up to be included in the study. If so, it would be appropriate to describe how the clinics were selected from the universe of clinics (area + estate)? And what was the procedure to pick up women/teen age pregnant women to be included in the study? It is not clear how women from these clinics were selected?

Please see our response below.
(g) It is stated that the upper limit of eight women to be recruited from any one clinic was set. Once again, not clear if one goes by the numbers above (200 clinics, and 450 sample size) as how was this done. If this has been the case, it would mean the number of clinics actually included in the study would be much much less. Please clarify.

We agree that this is confusing to the reader. This upper limit was set as a precaution against recruiting a large number of the sample from one clinic visit but in practice was only reached in 4 visits, with most visits yielding just 1 or 2 completed questionnaires. We have clarified this in the manuscript.

(h) It is also not clear as to how random sampling was achieved. It is essential to clarify else the current articulation is suggestive of ‘purposive sampling’ and will have bearing on the appropriateness of statistical tests applied.

The points raised above (e) to (h) all relate to the sampling procedures and we apologise for the lack of clarity in the original version. These details have been completely re-written to clarify the important issues raised by the reviewer. The new section appears on pages 6-7.

(i) Consent seeking: It is stated that girls/teenage pregnant girls were approached via midwives. Does this mean service providers (doctors) did not have any role to play in the consent seeking processes?

Verbal consent was initially sought by the clinic midwives. If a positive response was received the adolescent girl was then approached by one of the data collectors who explained the study in more detail and sought written consent prior to administering the questionnaire. The data collectors were two Public Health Nursing Sisters and a Medical Assistant. No doctors who were providing services had a role in this study.

(j) MOH Records: are they robust enough? And most importantly, needs to be mentioned the year of MoH records used for the sampling purposes.

The previous year's MOH records provided the sampling frame and this has been noted. The MOH records are extremely robust and cover almost all pregnancies. It has been noted in the discussion section that a limitation of our approach is that a small number of concealed pregnancies may have been missed, but these are also very unlikely to be picked up by any other quantitative sampling approach.

(k) The description of methods probably doesn't befit the ‘random sampling’ of women at the clinic.
The revised and expanded section on our sampling approach illustrates more clearly how a random and representative sample was achieved.

(l) Data analysis: It is stated that the responses to the open ended and direct questions were categorised for the analysis purposes. It will be helpful to clearly state as what guided this categorisation; if any seeming contradictions, if any, in the data set were handled; and if data were revisited and/or handled in an iterative manner as a way of making the data analysis robust and validated internally.

Thank you for pointing this out. We agree that the original detail included here is insufficient. The dataset was extensively examined and variables cross-checked to ensure consistency and credibility of the data. We have added this information (page 8-9).

(m) Analysis restricting to 409 cases only of the total 450: It is not clear as to why analysis of 409 cases for further analysis justified on the grounds that they were the cases of first pregnancy. If so, this should have been the eligibility criteria? In any case, it is not clear enough the rationale behind this rationale. It also suggests that data collected from other 41 girls was only marginally utilised.

The 41 pregnant adolescents who were not in their first pregnancy were excluded from some of the analyses presented in the current paper. Pilot work suggested that asking adolescent women who were pregnant for their second or third time to recall the circumstances of their first pregnancy would not deliver robust data. At the same time, it was clear that factors such as reproductive health knowledge and relationship status were likely to have changed considerably over time. Thus, while this group was included in the general description of the sample of pregnant adolescents, since they do make up part of this group (and indeed it was of interest to know what proportion of pregnancies were first or subsequent), it was appropriate to exclude them from the analysis of factors describing the context of adolescent pregnancy to provide a more homogenous sample. We have now made it clearer that sections of the analysis relate to first pregnancy (around 90% of the sample).

3. Are the data sound?

Yes with a caveat that stems from the aforesaid clarifications sought relating, particularly to the sampling.

All addressed as detailed above.
4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

Yes

Ł No response required.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

Yes

Ł No response required

6. Are limitations of the work clearly stated?

Yes.

Ł No response required.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?

To a great extent except that it will be appropriate and essential, as mentioned earlier in response to Q no 1 above, to update the work/lit review over these past three years, and sourcing material relating to the current policy status and overall trends of thinking amongst policy makers over the past couple of decades has reflected in the policies relating to teen age pregnancy. This might have been reflected in the overall SRRH framework or other relevant policies of the Shri Lankan government.

Ł Addressed as detailed above.

8. Do the title and abstract accurately convey what has been found?

Yes.

Ł No response required

9. Is the writing acceptable?

A minor point: Please edit sentences those begin with numbers. In such instances, numbers are to be written in words, as a norm.
We agree - all such sentences have been edited.

Major Compulsory Revisions

The points I made relating to methods and suggested revisions are essential.

 All these points have been addressed as detailed above.

Reviewer 3 : Jinseok Kim

Major compulsory revisions

1. Socio-demographic characteristics of overall sample should be summarized in a table.

 This has been done – new Table 1 has been added.

2. Description of Tamils and Moors distribution of the sample would be more informative if the authors provide population distribution of Tamils and Moors.

 The data for Badulla District taken from the DHS have been added and this comment adjusted.

3. Description of questions should be included in the methods section, not in the results section.

 This has been adjusted. No questions now appear in the results section.

4. When reporting statistical test results, relevant statistics and p-values should be reported in the text.

 All mentions of statistical tests have been checked and p values inserted where previously omitted.

5. Developing logistic regression model seemed very exploratory and data driven and no relevant theory or conceptual framework were involved; more theoretical and conceptual justification of regression model should be provided.

 This is an important point. The study was preceded by a careful review of prior theoretical and empirical literature. However, this literature relates almost entirely to settings outside Sri Lanka and raises a host of potentially important factors to be considered. The study was therefore intentionally exploratory in
nature and set out to examine a wide range of potential variables. We have now included some references to the prior theory and empirical work that guided the questionnaire development and subsequent analysis.

It has been made clearer that the selection and testing of different independent variables in the regression models was informed by this prior work.

6. Implication of the study needs to be firmly based on the findings from the study; specifically, while I concur that extrapolation should be done with special care, the authors didn't seem to provide enough empirical evidence why that's necessary; it would be much better for the readers to understand if the authors present the analysis results comparing with the equivalent numbers in, say, Western countries.

Additional comments have been added to the discussion to illustrate how Sri Lanka differs importantly from other settings.

Minor essential revisions

7. On page 6, “The content of the questionnaires was largely similar… and empirical work” should have been accompanied by specific references.

   Yes, we agree -- these have now been added as this section has been slightly expanded in response to point 6 above.

8. Overall organization of the manuscript needs to be rearranged; as mentioned earlier, parts included in the results section should be moved to methods section such that results section includes only the analysis results and findings.

   Questions have been removed from results section as requested.