Author's response to reviews

Title: Reproductive health of women with and without disabilities in South India, the SIDE Study (South India Disability Evidence) Study: A Case Control Study

Authors:

Venkata S Murthy Gudlavalleti (gvsmurthy1956@gmail.com)
Neena John (neena.sushil@iiphh.org)
Jayanthi Sagar (jayanthi.sagar@iiphh.org)

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Author's response to reviews: see over
Reviewer’s Comments

Reviewer 1: Nadine Seward

Reviewer’s report:
The manuscript entitled “Reproductive health of women with and without disabilities in South India, the SIDE Study (South India Disability Evidence) Study: A Case Control Study” was an interesting piece of work and read well.

We thank the reviewer for the comments.

However I still feel that overall, the manuscript will require considerable major compulsory works before it is fit for publication. Importantly, the author does not state the main objective of the paper and how this will be assessed for, only that the reproductive health of women will be described.

As suggested by the reviewer we have revised the manuscript thoroughly. The objective has now been clearly mentioned – Page 6, line 2-4.

I feel this is a decent first attempt, but a paper that looks at a specific outcome that is of public health interest would be more beneficial to the reader, in addition to the univariable analysis. As an example, a paper that looked at predictors of having a live birth in the past 2 years in women with a disability compared to women without a disability. I’m not an expert in disability by any means, so I’m not sure about topics of public health importance.

The suggestions are valuable and we have taken on board all suggestions which could be incorporated based on the data collected. Wherever the collected data is inadequate to answer some of the queries we are not in a position to make any changes post facto.

I also feel the authors would benefit from hiring an epidemiologist or statistician to help with some of the more technical aspects of the paper.

We would like to clarify that the authors include experienced epidemiologists and additional statistical inputs have been obtained wherever they were required.

The report I detail below is based as my experience as an epidemiologist/statistician and not as a clinician. Although many corrections are required, I have only presented a few general comments below that will hopefully be of some value to the authors:

1. Is the question posed by the authors well defined?
   • The abstract does not specifically state what the objective of the study was, only that they would look at reproductive health experiences of women with disability in South India. How will the authors attempt to do this? What is the purpose of having the case control study? Are the authors comparing women with disability to women without disability? If so, this should be clearly stated in the objective.

Though we have considered the comments of the reviewer carefully, we are also constrained by the word limit for the abstract. We have made changes wherever possible in the abstract,
including providing an objective. Yes, we are comparing women with and without a disability and we believe that we have brought this out very clearly.

• Although a vague objective is proposed, authors still need to state the main question of the study. The following excerpt illustrates how vague the authors have been: “There is a complete lack of published literature in peer reviewed journals on the reproductive health status of women with disabilities in India. The present study was therefore conducted to bridge this evidence gap to enable need-based appropriate reproductive health services for women with disability in India.”

Text in the introduction section has been modified (line 2-4, page 6) to include a specific objective as suggested by the reviewer.

• Authors compare reproductive health of women with disability to women without disability. Is this how authors which to look at reproductive health experiences? If so, this should be stated in a clear objective.

We have now specified what we included in the reproductive health experiences (line 4-6, page 6).

2. Are the methods appropriate and well described?
• The statistical methods were not described in sufficient detail. The authors state a chi-squared test was used for associations and authors do not state associations for what. It was also state logistic regression was used to determine odds for associated variables, but does not state associated variables of what.

This has now been included in the text. More details have been incorporated to reflect the statistical analysis undertaken.

Furthermore, logistic regression presents odds ratios, not odds.

We have corrected this error.
• Although the univariable analysis provides an initial understanding of the situation, further work is required to understand if there is truly no difference/or a difference between these two populations. There was no attempt to account for clustering, nor were there an attempt to create a multivariable model whilst adjusting for appropriate confounders with an appropriate outcome measure.

Since it was a case control design, we did not specifically look for clustering. However as we have now mentioned, adjusting the effect of other independent variables when looking at the association of a specific factor would help in addressing some of the concerns raised by the reviewer. The methods section now details the procedure followed for calculating odds ratios.

• The results are not presented in a way that is considered appropriate. For example, for a chi square test, it is only necessary to present the p value and not the chi square. Also, the mean age
of women in the two separate groups is presented, but authors do not state if there is a difference or not.

We have now modified the text based on the suggestions of the reviewers.

• Authors should provide a reference for the large study (i.e. South India Disability Evidence Study) that the present study is a part of.

This has now been done.

• Tables mention that some statistics are age adjusted, but the methods on how this was done were not reported.

This has now been included in the section of methods under the sub heading of statistical analysis.

3. Are the data sound?
• Impossible for me to tell from the information provided.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
• Results have not been appropriately stated. If authors state something is “significantly higher”, then a p value should be reported.

We have now incorporated the p-values as suggested.

• Presenting results as “x” times higher is not appropriate. Authors should state if this is an odds ratio, rate ratio, etc.

We have now mentioned all results as odds ratios.

• Results tables need some work. As an example, there is no need to report the number and percent in two separate columns. One column will do!

In many previous publications, we have been specifically been advised by the editors to include them in separate columns. However, we have accepted the suggestions of the reviewer and reworked the tables accordingly.

• If authors are comparing two groups, a statistic to describe the associations should also be stated.

We believe that this has now been incorporated both in the text and the tables.

• These are only a few of the many corrections that are required with the information in the results section.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
• Discussion and conclusion requires considerable work

We have modified the discussion and conclusions based on the results of the present study.

• Authors state reproductive health indicators are comparable between these two populations. What are the reproductive health indicators? These should be clearly stated in the discussion when the authors state there are no differences between the two groups.

This has been explicitly stated in the introduction now.

• Limitations are not discussed.

A section detailing the limitations has now been included in the discussion.

• Discussion and conclusions are not adequately separated out.

Discussion and conclusions are mentioned in separate sub heads. We are not clear what the reviewer wants additionally.

• Authors state there are no significant differences on most parameters of reproductive health. The results section to not support this statement.

We disagree with the reviewer on this aspect. We had expected differences in most of the reproductive health parameters but results did not show this.

• Future research is not discussed

This has now been specified in the section on conclusions.

6. Are limitations of the work clearly stated?

• Limitations of the study are not discussed
  These have now been incorporated.

• Recall bias? Women report on reproductive health and outcomes of any pregnancy over the past 2 years

• Have controls been appropriately selected? They are age and sex matched, but authors do not state why they think this was important.

• Other potential bias?

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
• This has been adequately completed.

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

• The study findings are adequately described in the title and abstract

9. Is the writing acceptable?
• Writing is acceptable English

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare I have no competing interests

Reviewer 2: Pat Doyle

Reviewer's report:
This study describes a case-control study of reproductive health in women with and without disabilities in South India, building on an earlier study of the prevalence of disability in the population. It is a novel study addressing an important, and neglected, public health issue in India.

I have several comments on the paper, mostly relating to more technical aspects of the case-control design.

Major Compulsory Revisions
Methods
1. I found the Methods section a little muddled. It would be useful to re-order the text to clarify the definition and sources of cases, the definition and sources of controls, data collection methods, and analysis.

We value the comments of the reviewer and have modified the methods section to address the concerns raised by the reviewer.

2. The methods section had very little detail on the method of selection of the controls. More detail is needed to be able to assess the likelihood (or not) of selection bias.

We have now added more details on selection of controls.

3. The Statistical Analysis section is too brief. More needs to be added on the method used for adjustment of potential confounding factors in logistic regression. also, was a power calculation done?

We have revised the statistical analysis section to address concerns raised by the reviewer.
Results
4. Table 1. The vast majority of cases (72%) were described as “other”. More detail is needed here—perhaps as grouped categories—as the reader has no idea what type of disability is included.

We have now revised Table 1 to incorporate the valuable suggestions of the reviewer.

5. Table 3. In the calculation of the mean number of births per woman and a history of miscarriage, it appears that the denominator is the total number of women (247 cases and 324 controls). This is incorrect. The denominator needs to be the number of women with a history of one or more pregnancies. This may change the conclusion that cases have more living children than controls.

We have revised the table to address the concerns of the reviewer. The denominator was number of women with a history of one or more pregnancies. This has now been mentioned in the text and the table.

6. Table 3. The way odds ratios have been presented in the table is non-standard. The authors need to (i) include 95% confidence intervals, (ii) provide a footnote to indicate what variables have been adjusted for in the models, and (ii) consider presenting crude and adjusted OR for all the variables in Table 4 (and leave out chi-square statistics) to make the table more readable.

We have revised the tables based on the valuable inputs of the reviewer.

Discussion
7. Reference to use of contraception by cases and controls is speculative and should be mentioned more cautiously (and possibly changed considering the denominator issue mentioned in point 4 above).

We agree with the reviewer. We have therefore revised the sentence accordingly.

8. The limitations of the study should be mentioned, especially the low numbers for some analyses and resulting low power to detect smaller differences.

We have now included a section on limitations as suggested by the reviewer.

Minor Essential Revisions

This has now been deleted


This has now been revised as suggested.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests