Author's response to reviews

Title: Household headship and child death: Evidence from Nepal

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Author's response to reviews:

Dear Editor,

We have incorporated all the comments given by editor and the reviewers. Attached please find a copy of the revised version of my manuscript entitled "Household headship and child death: Evidence from Nepal"

Below please find our point-by-point responses of the comments.

Responses to the editor's comments

The revised manuscript has edited again by a native English speaking professional editor.

Repose to Sarah Saleem's comments

Thank you so much for the recommendation for publication.

Repose to Margaret Elizabeth Kruk's comments

1. The first stage of DHS sampling is random sampling with probability proportional to size NOT systematic (that is at a later stage).

Response: In the report of Nepal Demographic health survey, it is written that "The sample for the survey is based on a two-stage, stratified, nationally representative sample of households. At the first stage of sampling, 260 PSUs (82 in urban areas and 178 in rural areas) were selected using systematic sampling with probability proportional to size". Please suggest me whether I have to correct it or not.

2. The Methods section needs to include the theory-based explanation for inclusion of the explanatory variables, citing appropriate literature. For example, care-seeking variables are not necessarily a measure of the status of women as the authors state in their response to reviewers. This section need not be long.

Response: We have added one paragraph regarding it. We agree that care seeking variable is not necessarily a measure of the status of women. We have added it because many literatures showed that care-seeking behavior are related to morbidity and mortality. So authors included this variable in the analysis.
3. The term “child death experience” should be replaced everywhere with “experience of child death”
Response: We have replaced accordingly.

4. The reason for the association between parity and mortality is primarily that women with more children are simply more likely to have one of them die (higher exposure). This is not due to repeat childbearing. The more relevant variable here would be birth spacing, where there is clear evidence that births less than two years apart confer higher risk of child death.
Response: In the revised manuscript, authors have added the variable 'Previous birth interval'.

Once again, thank you very much for your valuable comments.

Regards,

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