Author's response to reviews

Title: Female migrants, Family Members and Community Socio-Demographic Characteristics Influence Facility Delivery in Rufiji, Tanzania

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Version: 4 Date: 24 July 2014

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

Thank you for considering our manuscript titled, “A Retrospective Cohort Study Investigating Individual and Community Determinants of Facility Delivery in Rufiji, Tanzania”.

We have taken your comments into consideration and attach our responses in this cover letter. For clarity we have highlighted areas where changes were made and add the reviewer’s comments to allow tracking of responses. We have also changed the title of the article to reflect the main findings from this study to “Female migrants, Family Members and Community Socio-Demographic Characteristics Influence Facility Delivery in Rufiji, Tanzania”

Kind regards,

Francis Levira

Reviewer: Victor Inem

Title: A Retrospective Cohort Study Investigating Individual and Community Determinants of Facility Delivery in Rufiji, Tanzania
Version: 2
Date: 5 May 2014

Reviewer’s report:
Abstract
Authors are encouraged to add some quantitative data to it
Abstract updated.

In the Background
Delete the phrase country of study after Tanzania
Deleted.

Reviewer: Cheryl Moyer

Title: A Retrospective Cohort Study Investigating Individual and Community Determinants of Facility Delivery in Rufiji, Tanzania
Version: 2
Date: 27 May 2014
Reviewer's report:

The authors have presented a well-written article exploring 10 years of HDSS data regarding the social and community factors associated with place of delivery in Tanzania. This manuscript is interesting to read, uses innovative methods to explore HDSS data, and adds a new perspective to the discussion of 'social influences' on facility delivery.

That said, I had a few comments that, if addressed, might strengthen the manuscript.

1) First, the background section includes 8 good references regarding factors associated with facility-based delivery, but does not give mention to any of the review articles that have included 70+ articles. These include Say and Raine (2007); Gabrysch and Cambell (2009); and Moyer and Mustafa (2013). It seems like it might be worthwhile to reference these works.
   Reference added

2) Second, I found Figure 1 particularly telling - there does not appear to be much change within villages, despite huge variability across villages, in FBD rates and the change in those rates over 10 years. It would be helpful to add a paragraph in the discussion addressing this observation. Why do the authors think rates are so resistant to change?
   Figure 1 presented a sample of few villages out of 33 analyzed to show that some of villages had consistently high coverage, low coverage and sharp increase. In the discussion paragraphs reasons for variation have been explained including why village’s low coverage can be linked to the remoteness of the villages from the main such as BUM and MLA as detailed also by the coverage map in figure 2.

3) Third, the authors don't speculate on why the community's average years of schooling among females might be negatively associated with facility delivery - even though this is a counter-intuitive finding.
   Paragraph added.

4) Fourth, the final paragraph in the conclusion could be strengthened. The authors conclude that "interventions designed to increase facility delivery must acknowledge these (social) factors to effectively promote health services utilization." How? What might that look like? What do these results really mean for programs and policies? I humbly push the authors to think a bit more about the implications of their findings.
   Conclusions modified and strengthened

Reviewer: Heather Sipsma

Title: A Retrospective Cohort Study Investigating Individual and Community Determinants of Facility Delivery in Rufiji, Tanzania
1. Abstract: Please revise the first sentence of the Background section; it currently sounds as if just policymakers are advocating for facility delivery, but this is not the case. Much of the medical community and public health experts are advocating for this as well. Abstract and background revised page 2

2. Abstract: Please include a sentence in the background that points to the rationale for the study. In other words, include a statement about what is currently unknown or inconsistent within the relevant literature. Abstract and background revised page 2 & 4.

3. Abstract: Please be more specific about what “acknowledging” the factors affecting facility delivery might look like. Abstract and discussion revised. Page 2 & 10

4. Background: I suggest the authors reorder the presentation of the background literature so that it flows from individual to the community level factors, instead of beginning with facilities. Re-ordered page 3

5. Background: Please note that the individual-level factors affecting facility delivery are many and are well-established (which will then contrast with the limited and inconsistent nature of what is known about community level factors). Amended page 3

6. Background: The literature on facility level factors is scant and needs additional development. The authors cite only 4 articles that suggest the presence of a skilled birth attendant decreases maternal mortality. As the authors suggest, this factor may only be loosely related to facility birth. There are a few articles I can think of off the top of my head which discuss facility level characteristics that may affect place of delivery: New article added in addition to the one below page 3


7. Background: It is not clear what contribution reference #8 makes in the introduction; the authors only suggest that from this reference, we know that community-level factors vary among countries, which does not seem to be the research question of interest in this paper. Please ensure this paragraph on community level factors focuses on the literature that examines the potential influence of community-level factors on birth setting. Details of the study findings from Tanzania and Kenya added in page 3.

8. Background: Among the studies that examine the influence of community-level factors, do they all account for individual-level characteristics? If not, this would be strength of your approach and a gap in the literature for the authors to highlight. Yes they do and clarified on page 3.

9. Data: The study design is not clear. Are these longitudinal data or serial cross-sectional? It is important to clarify the points in time at which measurements occurred. This was longitudinal data analysis, clarified in the methods on page 5.

10. Data: What if the mother did not have a sister-in-law or sisters? Were these variables then simply considered missing? Similarly, is the count variable for number of facility based deliveries accumulated over different years or reported/measured at one time? If a mother has no sister-in-law she was not included in the model, that is why the model with sister-in-law effect did have fewer observation compared to model without this variable (910 vs 19,992) (number of observation is shown for each analysis on top of each table). The numbers of facility based deliveries were accumulated over different years based on our delivery records of each woman. This was a longitudinal data for therefore each demographic event (including birth) is recorded to each woman. Outlined on page 5 & 16.

11. Data: Please include a reference for the principal component analysis used to construct your wealth measure. Added on page 6.

12. Model specification: The authors specify a 2-level model, but discuss three levels, including those of children, mothers, and village (community). No child-level variables are mentioned in the Data section, however, so this is confusing. There are no child level variables mentioned in the Discussion. There are only two levels. Description added in the method section and discussion.

13. Results: How was the increase in facility delivery analyzed? This is not clear in the analysis section. Only multilevel logistic regression models are discussed. The analysis was conducted by computing the proportion of facility delivery (facility delivery/all deliveries) in 2001 and 2010. Methods added in the methodology on page 6.

14. Results: The analytic approach for the results presented in the second paragraph is also not clear and not supported by the measures discussed in the Data section.
Data section added on communities mapping on page 6

15. Results: The authors should also include an analytic approach for the results presented in Figure 3.
Methodology added on page 6.

16. Results: The tables should separate the individual-level and community-level factors from one another so it is clear to the readers which variables belong in which category.
Tables updated page 16 - 18

17. Results: Explain how the non-linearity of the age variable on facility delivery was tested or drop this parenthetical comment in the Results section.
Removed

18. Results: Table 2 lists “cousin facility delivery” and “paternal cousin facility delivery” but the text in the Results discusses “sister-in-law count of births.” Please be consistent with this terminology throughout.
Changed to sister and sister-in-law as in the table 2 page 18.

19. Discussion: The authors should discuss the discrepancy between the effect of individual age (as corroborated by other literature cited in the introduction) and the effect of community mean age.
Amended page 9

20. Discussion: The authors should include additional in-depth discussion of the implications of these community-level results. What does this mean for community health workers, for instance, in the area and their efforts towards increasing facility births?
Added page 9

21. Overall: Many terms were used inconsistently (e.g., “community factors” and “community-level factors”). It improves the readability of the paper to use these more consistently throughout.
Changed throughout to community-level factors

Reviewer: Philip Adongo

Title: A Retrospective Cohort Study Investigating Individual and Community Determinants of Facility Delivery in Rufiji, Tanzania
Version: 2
Date: 2 June 2014

Reviewer’s report:

Reviewer comments:
Major Revisions
This paper, which looked at “A Retrospective Cohort Study Investigating Individual and Community Determinants of Facility Delivery in Rufiji, Tanzania”, is very important addition to the literature on delivery in health facilities. However I have two issues that the authors will need to seriously consider addressing.

1. The assumption that in-migrants and sister or sister-in-law place of delivery could be described as social network could be very misleading. The term social network presupposes that there is interpersonal relations- the author cannot attest to the fact that was any interactions between the mothers such persons. It important to say what they found instead of talking about social network which, has not been proven with data. The term changed throughout the document

2. The descriptions of the variables, both in and out migration were not well defined. In reference to these, did the authors distinguish between rural-rural migration and rural-urban migration? The anticipation is that migration may improve knowledge as rightly discussed by the authors but where people migrate into places of similar socio-economic and environment characteristics, what will be the additional knowledge gained? Did the analysis take this into consideration? We don’t have a variable for rural-rural and urban-rural migrants, however I have changed the analysis to include a variable for female migrant vs non-migrant and residents.

Minor Revisions
Abstract:
METHODS: The method describes only what was done and does not provide any information regarding the source and magnitude of the data used for this paper. . The source was the Health and Demographic Surveillance in Rufiji as described on page 5. We have now added the magnitude on page 5.

RESULTS: The authors should support the findings with some figures, for example when the authors say household with in-migrants, what is the extent? Providing the odds and confidence intervals will give a better understanding of paper findings. I suggest this done for all key findings mentioned in the abstract. This has been mentioned on page 8/9 for in-migrants, and amended in the results based on the abstracts.

INTRODUCTION:
Paragraph 1- the first 2 sentences are statements of fact and should be supported with evidence (referenced). Please delete “the country of study” from the fifth sentence. Paragraph 2- here again the first 2 sentences should be supported with appropriate literature. . Amended on page 3.

DATA AND METHODS:
Paragraph 1- HDSS should be introduced before been used subsequently. Paragraph 2- you introduce RHDSS and then you write Rufiji HDSS. The authors are advised to show consistency in their writing. The manuscript has several of these inconsistencies and needs to be sorted out. Amended throughout the document

RESULTS:
As mentioned in the method section abbreviations mean nothing when they are not introduced. It is not proper to write village names in abbreviations anyway. Are these abbreviations which the authors used names known to the communities? I do not believe villages/communities names in Tanzania are known as abbreviations (for example village IKC, community IKN ???) If these are not villages but zones or geographic identifications then the author should report them as such. Abbreviations amended and added to abbreviation list on page 10-11

The authors are advised to provide a better title for figure 1. The word “regional variation” has been used in the title and this may be misleading since you describing the situation in one district. Figure text changed

DISCUSSION:
Paragraph 1: does not discuss any result; it only repeats what has been mentioned in the result section. The authors may consider improving it or completely deleting it. In most discussions, the first paragraph outlines the results of the study as per other articles you have published (http://www.ncbi.nlm.nih.gov/pmc/articles/PMC3585789/). We feel it is important to recap the results before the discussion

Paragraph 2: the 1st sentence is so loaded with several important issues that after reading through, one does not get the message it tries to depict. The sentence needs to rewritten to clarity the message that it seeks to convey to the reader. The same can be said about the 2nd sentence, it looks very blundering. The paper should have a section that thoroughly discusses all the limitations of the study instead of burying one sentence in the discussions as limitation in paragraph 5. I will suggest that the authors create a section on the limitations of the paper. Amended on page 9. There is already a section which outlines the limitations on page 10.

CONCLUSIONS
The authors conclude the paper based on social networks, which results of the study cannot definitely proof. The authors are advised to rewrite the conclusions. Amended on page 10.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published
**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

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