Author's response to reviews

Title: Socio-economic factors associated with delivery by traditional birth attendants in Iraq, 2000

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Author's response to reviews: see over
17 January 2009
Dear Editor,

We are pleased to submit a revised manuscript. We have addressed the reviewers’ comments:

Manuscript title: Socio-economic factors associated with delivery by traditional birth attendants in Iraq, 2000

Seter Siziya, Adamson S. Muula, Emmanuel Rudatsikira

Responses to reviewers’ comments

REVIEWER: ZAFAR FATMI

Comments in the second paragraph
In this paragraph the reviewer sought more explanation on sampling so that the readers could understand it easily. The reviewer was concerned about what is new in our paper.

Response to comments in the first paragraph
When we embarked on the analysis we of course had information as to what has already been published and is available in the literature. We however did not know what results we would get. There were limited data from the Middle Eastern Region though. It was only when we obtained the results that we found what most previous authors had found. However, the paper does contain new information for Iraq as observed by the reviewer.

Comments in the third paragraph
Further citation is required to support the findings of the study in the discussion.

Response to comments in the third paragraph.
We have added a few more references supporting further justification for the study in the Introduction and the discussion section.

REVIEWER: SARAH SALEEM

Comments in the second paragraph
The study overall does not add anything new to already existing knowledge about the factors associated with deliveries conducted by TBAs. The results presented could be of more interest to local readers than for international readership.

Response to comments in the second paragraph
There are few studies that have been reported that have used nationally representative samples. The results from our study confirm some results that were obtained in studies with smaller sample sizes. Since most of the studies on the subject have been cross sectional, a Meta analysis of these studies would add value to what we know so far, and our results would be of interest to the international readers.

Abstract:
Comments in Bullet 1
The reviewer indicated that it would be more interesting if authors could present results on the outcome of deliveries conducted by TBAs and those conducted in hospitals. The reviewer assumed that MICS has this information as well.

Response to comments Bullet 1
Data on outcome of deliveries was not collected in the Iraq MICS.

Comments in Bullet 2
The reviewer suggested that the assumption stated below be removed or rephrased: “The assumption being that delivery under the supervision of TBAs was likely to be under less than optimal circumstance with heightened risk of adverse maternal and fetal and neonatal outcomes compared to health facility delivery under the supervision of health professionals”

Response to comments in Bullet 2
We agree with the reviewers comments and removed the assumption in question.

Introduction:
Comments in Bullet 1
Introduction section needs more elaboration especially on birth outcomes in deliveries conducted by unskilled birth attendants, and some research on why women prefer TBAs over facility based deliveries.

Response to comments in Bullet 1
We have addressed the reviewer’s comment in the second paragraph in the Introduction section. We have given reasons as to why women may prefer to be attended by TBAs during delivery than by skilled staff in health facilities. We have also added facts on health facility indicators to strengthen our paper.

Methods:
Comments in Bullet 1
Correct response rate for households.

Response to comment in Bullet 1
We have corrected the denominator that was a typographical error.

Comments in Bullet 2
Data analysis: Though authors have mentioned using SPSS 11.5 using weights for analysis better statistical packages such as STATA for complex survey analysis and SAS would have been better options for this kind of surveys.

Response to comments in Bullet 2
Two ways that could be used in the analysis to consider the complex design in the analysis are: (1) use of weights that take into account the differing sampling probabilities and response rates at different stages of sampling, and (2) use of complex programmes in packages. Better still a combination of the two methods may produce better estimates. Use of weights in the analysis is common in such surveys as can be observed in the literature. We have conducted weighted analysis.

Comments in Bullet 3
The reviewer sought clarity on definitions for wealth and marital status.

Response to comments in Bullet 3
We have clarified the definitions for wealth and marital status in the Methods section. We have also clarified how the factor “age” was collected.

Results:
Comments in Bullet 1
The reviewer sought some explanation for the small number of participants that provided information on deliveries conducted during last 12 months.

Response to comments in Bullet 1
Because we used secondary data we are unable to comment on the small percentage of participants that provided information on delivery conducted during last 12 months. None of the authors participated in the Iraq MICS. We can only speculate what led to small numbers. We choose not to go ahead with the speculation and instead pointed out this concern as a limitation in the last paragraph of the Discussion section.

Comments in Bullet 2
The reviewer sought appropriate definition for the outcome variable.

Response to comments in Bullet 2
The outcome variable was categorized into women who had deliveries assisted by TBAs on one hand, and women who did not have deliveries assisted by TBAs (i.e. deliveries that were supervised by health personnel [doctors, nurse/midwives, and auxiliary staff) or relatives/friends. We have clarified this definition in the methods section.

Comments in Bullet 3
State the total number of participants in the title of the table. Age was missing on about a quarter of the respondents. The reviewer further sought clarification on whether the age that was reported was the age at the time of interview or when the woman delivered. If it
is the first case, the reviewer suggested that age be adjusted by reducing it by one year. The reviewer also noted a lot of missing information on maternal education.

**Response to comments in Bullet 3**

We obliged and stated the total number of participants in the title of Table 1. We cannot comment on the number of missing information on age for the reason given above. However we do have information on how the factor “age” was collected. Interviewers asked respondents what their ages were last birthday. This may coincide with the ages at deliveries since we only women who delivered in the last one year were eligible in our study. In this case we need not adjust by reducing 1 from the obtained ages. We can not do much about the missing data even for the factor “maternal education”, except to highlight this observation in the paragraph on Limitations.

**Discussion**

**Comments in Paragraph 1**

The reviewer found it difficult to accept the concept of social stigma attached to not having a husband we referred to in the second paragraph of the Discussion section.

**Response to comments Paragraph 1**

We overlooked the fact that marital status was NOT significantly associated with TBA delivery assisted. We, therefore, removed the paragraph in question from the Discussion section.

**Comments in Paragraph 2**

Third paragraph is confusing and give contradictory statements. The reviewer indicates that cross sectional studies can be used for hypotheses generation and future research. The reviewer also stated that the time period was so short that most variables studied could not have changed drastically during this was one year. Change in marital status is a possibility.

**Response to comments in Paragraph 2**

Area of residence was NOT significant in multivariate analysis, and we, thus, removed part of the paragraph that dealt with area of residence. Results bivariate analyses may be confounded by other factors that were considered in the study.

Studies to study causation are expensive and most of them take years to complete. This is the more reason as to why a series of cross sectional studies are preferred to cohort studies. We agree with the reviewer that generally, cross sectional studies are meant for hypotheses generation and recommendation for further studies. However, consistent findings from cross sectional studies in different locations with large enough sample sizes may provide evidence for causation.
We hope we have responded to the satisfaction of the reviewers.

Adamson S. Muula