Reviewer’s report

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

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**Reviewer:** Sarah Saleem

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Comments to the authors on Socio-Economic factors Associated with delivery by traditional birth attendants in Iraq, 2000

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The study is about the socioeconomic factors associated with delivery by traditional birth attendants in Iraq. Multiple Indicator Cluster Survey (MICS) data for 2000 were used. The socio-economic factors assessed were wealth, area of residence (urban versus rural), parity, maternal education and age. A total of 22,980 women participated in the survey, with 2873 providing information on delivery. Findings from this study indicate that young adult age, low education and poverty were associated with having delivery conducted by a traditional birth attendant.

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.

The detail comments are as follows;

**Abstract:**

- Under the heading ‘background’in the third line authors have mentioned’ 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”. It would be more interesting if authors could present results on the outcome of deliveries conducted by TBAs and those conducted in hospitals. I am assuming that MICS survey has this information as well.

- Under the Heading ‘Conclusion” the authors have used the same assumption as mentioned above to conclude the study results which is very confusing and makes me think what was the hypothesis of this study or analysis to begin with? It is suggested that these lines be removed or rephrased.

**Introduction:**
• 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. This would make a nice case for the current analysis and will support the assumptions mentioned in the abstract (BTW which is absent from rest of the paper). Also considering the objectives of MICS survey mentioned in the last para of the introduction section; the adverse outcomes (if any) occurring in deliveries conducted by TBAs will make a strong case in prevention of prenatal and early neonatal deaths by recommending facility based deliveries.

Methods:
• Under the sub-heading ‘Questionnaire’ in the the last two lines there is some confusion in the denominator in the response rate of the households—this is probably a typo mistake which needs to be corrected.
• 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.
• The definitions of wealth needs further elaboration such as how household assets were categorized in quintiles; according to number of items irrespective of monetary value or on scoring according to their monetary value. It is true that other surveys have used this method but for some readers this could be very interesting to know. Also definition of married is not clear. It has been mentioned in Table 1 that 99.3 percent of women were currently married and 22 were not. These women could fall into ever married category being either recently widowed or divorced-this needs clarification as discussion generated around this variable is not convincing (see my comments).

Results:
• The total number of women surveyed were 22,980 but only 2873 i.e 12.5% provided information on delivery conducted during last 12 months. Either this small percentage of women delivered in last one year (which is highly unlikely) or only these many women were willing to provided information or the information is missing on many women. In either case some explanation is required for the small numbers.
• The outcome binary variable is delivery conducted by TBAs and not by TBAs. I am assuming the other group had facility deliveries by skilled birth attendants. This needs clarity in the write up and also appropriate definitions of outcome variable should be given.
• Table 1. Title should show total number of respondents included in the analysis. I am assuming it to be n= 2873. Information on age is missing on more than a quarter of respondents (on 780 subjects) which could bias the results as we don’t know to which age category these women belonged to. Also it is not clear whether age reported here is what it was at the time of interview or when the woman delivered. If it the first case then it should be adjusted and then should be a year less. This needs clarification. The numbers are missing in the maternal education status as well - information is available on 64 % of the subjects only.
and can give biased results.

Discussion:

Second para starts with “We also found that married women were less likely to deliver under TBA compared to unmarried. Although we are unable to suggest why this may have been the case, it is possible that the social stigma associated with pregnancy without a spouse may have facilitated TBA or community deliver. This hypothesis however needs to be assessed in future studies”. As mentioned earlier, the definition of married is not clear. All of these women could fall into ever married category. The women who are not considered in ‘currently married category’ could be either divorced or widowed when this interview was carried out but were living with their husbands when they gave birth to the baby. Or they conceived while still living with their husbands and their status changed when they delivered the baby. So it is difficult for me to accept the concept of ‘social stigma attached to not having a husband’. In my opinion selection of TBA for delivery by these women could be more due to financial reasons. I am not sure about the social and cultural standing of Iraq but I strongly believe being a Muslim traditional country, having pregnancies out of wedlock is a very remote possibility. If this is the case then a para on changing structure will add to the knowledge of the readers. My suggestion is to rephrase the para and also the highlighted and underlined line as no pregnancy can occur without a spouse (definition?).

Third para is confusing and give contradictory statements. It is not clear what the argument is about and to which context of the results. Coming to the last para, last two lines authors mentioned about the study being a cross sectional survey and inability to assess causal relationship with factors studied and being delivered by a TBA. Good argument. However such studies can be used for hypotheses generation and future research. My second comment is that even being a cross sectional survey the time period studied was for last one year prior to survey. I can safely suggest that most of variables studied could not have changed drastically during this one year, be it age, parity, wealth affecting the outcome. Change in marital status is a possibility.

**Level of interest:** An article of limited interest

**Quality of written English:** Needs some language corrections before being published

**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'