Author's response to reviews

Title: Delivery, immediate newborn and cord care practices in Pemba Tanzania: A qualitative study of community, hospital staff and community level care providers for knowledge, attitudes, belief systems and practices

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Author’s response to reviews: see over
To, Dated: 14\textsuperscript{th} Feb, 2014

The Editor
BMC Pregnancy & Childbirth
BioMed Central
United Kingdom

Subject: Submission of Revised Manuscript

Dear Sir,

With reference to our manuscript entitled “Delivery and cord care practices in Pemba Tanzania: A qualitative study of community, hospital staff and community level care providers for knowledge, attitudes, belief systems and practices”, manuscript ID 9092012331062734, we are hereby submitting the revised manuscript with all the corrections incorporated. Please find below point-by-point response to reviewer’s comments.

Kindly acknowledge the same.

Yours truly,

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Reviewer's report 1
Reviewer: Columba K Mbekenga

Major compulsory revisions:

- The aim:
  Different aims are stated at different points in the manuscript. For example, in the abstract (page 3); first paragraph (last sentence); second paragraph (in method subsection, last sentence); page 6 (second paragraph, statement no. 4) just to mention a few. The authors need to be consistent on how they state the aim throughout the manuscript.

  For that matter, the aim doesn’t match what is stated in the methods i.e. ‘explore attitudes, beliefs and practices...related to delivery and newborn care’ Vs. ‘explore practices, barriers and impediments to implementation of chlorhexine cord care regimen’. The two are different and would differently different findings. This confusion is also reflected in the title provided where knowledge, attitudes, belief systems and practices are mentioned. However, the findings point to a focus on practice and very little on other aspects. The authors need to be specific and rephrase their aim to fit what was exactly done, and change this across the manuscript where the aim is stated.

  Response: We appreciate the concerns of the reviewer which primarily resulted from word limit considerations. We agree that the primary focus of the current study was to understand belief, knowledge and practices of the community as well as health functionaries around delivery, immediate newborn care and cord care practices as well as perceptions and impediments towards CHX cord care. However we did want to have little broader information regarding delivery and newborn care that could have impacted implementation of the main CHX efficacy trial. Also there are as we discovered differences between West Africa, East Africa and then Zanzibar which is an Island with long history of Omani influence. We have tried to change the title as well as make consistencies between the abstract, methods and discussion section as regards the aims of the study as suggested by the reviewer.

- The method section:
  Two qualitative data collection methods are mentioned i.e. IDIs and FGDs. However, the authors do not provide any justification for combining the two methods. It is not clear what kind of data is collected using each specific method (what was explored for each specific method), and how the data collected by the two methods complement each other. Definitely, individual interviews would not yield the same kind of data as FGDs, and this methodological difference is not reflected in the manuscript.

  Response: The issue with qualitative analysis approaches in developing country settings has consistently been that women either confer to what is an accepted norm or behavior as they perceive it or what is a behavior recommended by programs. In order to really dissect the opinion and consistency of beliefs and practices we tried to use a
dual approach of using both IDIs and FGDs thereby preventing a bias. This has been also now clarified in the methods section of the manuscript.

In a subsection sampling and sample size; the second sentence is written ‘final sample size was based on the experience of social scientists and ongoing review of the process of data collection”; this statement is not clear, may be the author could be more explicit on this, given the later statement in the next sentence, reads ‘data collection continued until data saturation was reached”.

Response: The sentence has been modified as per reviewer’s suggestion.

In the sampling strategy; the authors use the term untrained and trained TBAs. It would be beneficial for the readers if the authors could elaborate on what they mean by those two terms. For example for the trained TBAs, who train them, for how long, which content and what are their limitations when it comes to practice (what are they allowed to do/ not to do) by the health care system, and how are they connected to the system. This could be part of the background information or described as part of the setting. This kind of description will help when interpreting data, especially those from the TBAs.

Response: The description of trained and untrained TBAs has been added to the manuscript.

• Data analysis:
The authors mentioned that they use a thematic approach in analyzing data, but do not provide any scholarly reference for their approach. This is very important especially for the readers who might be interested to follow their steps in coding and forming themes.

Response: Reference has been added to the manuscript.

The analysis process is not clear. A step by step description is needed to guide the reader on how they approached and organized their data. To give an example, it seems code family and themes mean the same thing (from how they write in the manuscript). But later, the authors write, ‘two investigators applied codes to the interviews using ATLAS... Coded text was retrieved and emerging themes analyzed in relation to other themes and variables’. Then, several questions arise; how can one apply code into interview data? The term variables here refer to what? From the way the analysis process is written, it is not clear what was the first step and last step hence confusion. The authors need to clearly describe the process of analysis, and if possible give an example of what they did in the manuscript.

Response: Data Analysis section has been modified to give more details and references have been added to reflect the process of using Atlas ti software. The steps involved in trying to allocate codes to segments of data and then having themes which combine multiple codes is a standard practice using qualitative data collected in the
format that we did. We believe that modifications made in the text and the clarifications should be sufficient to address this concern. Any further explanations would increase the content of the text beyond needed limits. However if the editor so feels we would be willing to add a paragraph on how qualitative analysis is done.

• The results/discussion:
The results section is very rich. A lot of important findings are included in this section. However, I have problem with how the authors treat their findings from the two data sets i.e. FGDs and IDIs. The way they are reported, it is like they come from the same set. What people can say or express in a group is normally what is accepted or adhering to the social norms of a particular context, and it is likely, this would be different from individual opinion or expression in individual in-depth interviews. I would wish the authors to consider these methodological differences of the two methods and include a methodological discussion at the end of their manuscript for the two methods they use. I would also suggest they find ways to explain how the two sets of data complemented each other.

Response: We agree with the reviewer about certain differences between two methods of data collection but we disagree with the reviewer treating two methods of data collection as two isolated truths independent of each other. The intent in the data collection process is to get to the truth which is only one. We have used the same intent in our analysis process of trying to understand responses from IDIs as well as FGDs eventually trying to triangulate those responses to try our best to formulate the truth of the community perceptions. In our opinion there cannot be multiple truths some based on IDIs and some based on FGDs and one needs to keep these truths separate.

Another problem I see for TBAs especially, the trained ones, who probably are connected to the health care system, they are likely to give desirable answers (from the health care perspective), so that they are not implicated in practicing against what they are taught and allowed to do. There is a need to have this as part of the discussion for limitations.

Response: The TBAs in the community are not in any way paid or governed by a formal health system and it is unlikely they would have any consideration for the regulations of the Health Ministry. However a note to this effect has been added to the limitation section as desired by the reviewer.

The results section has a lot on practices than any of the other mentioned aims i.e. exploring knowledge, attitudes, beliefs system, barriers and impediments…. As I said in my previous comment at the beginning, I think the authors need to rephrase their aim/objective to match better with what their findings are. I am also wondering of the choice of method to study practices. Since the authors plan to do a bigger quantitative study, this would have been better captured in that particular methodology.
Response: The comment of the reviewer has been addressed with additions of sections pertaining to chlorhexidine and also addition of supplementary tables to deal with the same. The aims and objectives sections throughout the text have been modified and made consistent.
However, I had several concerns about the paper as written. In the introduction of the study, the authors say that this phase of research (and this manuscript) was designed to: 1) collect information on delivery practices and understand neonatal and umbilical cord care practices in the community; 2) Get feedback, perceptions, and suggestions from TBAs, community members, and health professionals regarding liquid cleansing solution as an umbilical cord care practice; 3) To evaluate the acceptance and barriers for the use of the proposed chlorhexidine cleansing solution; and 4) To develop communication messages, study procedures, and the framework for implementing a cord care intervention. In reading the study, I had competing reactions. First, the manuscript seemed too broad – I would have preferred to see a more focused inquiry on cord care and attitudes toward chlorhexidine alone.

Response: The manuscript has been modified to include more data on chlorhexidine cord care both in the text of the manuscript as well as addition of supplementary tables. The text has been modified to include reference to other studies while at the same time including the rationale that practices around delivery and newborn care in Zanzibar may be different given 100 years of Omani influence. There is no data from Zanzibar and therefore we feel that documentation of this data is important.

Second, I wasn’t sure that the manuscript (or the abstract) met the goals described in the introduction. Third, I wasn’t sure that this manuscript taught me anything particularly new. Recent work in Ghana (Moyer et al. BMC Pregnancy and Childbirth 2012, 12:50 http://www.biomedcentral.com/1471-2393/12/50; Hill Z, Manu A, Tawiah-Agyemang C, Gyan T, Turner K, Weobong B, Ten Asbroek AHA, Kirkwood BR: How did formative research inform the development of a home-based neonatal care intervention in rural Ghana? J Perinatol 2008, 28:S38–S45.) and Tanzania (Mosha F, Winani S, Wood S, Changulacha J, Ngasalla B: Evaluation of the effectiveness of a clean delivery kit intervention in preventing cord infection and puerperal sepsis among neonates and their mothers in rural Mwanza Region, Tanzania. Tanz Health Res Bull 2005, 7(3):185–188.) have widely reported similar results to what was presented. I think the manuscript would be strengthened if the sole focus was attitudes toward uptake of Chlorhexadine, and the barriers / perceptions in the context of planning for a wider study.

Response: As pointed out above the manuscript added information regarding delivery and newborn care in Zanzibar which is not existent in the literature as of now. We have added the references to publications cited by the reviewer in the paper now most of which are from West Africa and the only one from east Africa are from Tanzania mainland which may be in principle fairly different from Zanzibar. We have also modified aims and objectives as well as added sections with more details on chlorhexidine cord care as suggested by the reviewer and hope that should match the desires of the reviewer. We do however disagree that the results regarding delivery and newborn care in Zanzibar are not of interest because that provides the context and framework for chlorhexidine cord care intervention.