Reviewer’s report

Title: Male involvement in maternal health: Perspectives of opinion leaders

Version: 0 Date: 28 Aug 2017

Reviewer: John Kuumuori Ganle

Reviewer’s report:

General Comment

This paper on 'Male involvement in maternal health: Perspectives of opinion leaders' aims to 'explored the reasons for men's resistance to the adoption of a more proactive role in pregnancy care and their enduring influence in the decision making process during emergencies'. The article addresses an important issue in current SRH and MCH discussions, namely the lack of involvement of men in SRH and MCH and the need for strategies to involve men. While the article's findings could be important, the paper in its current form suffers a number of deficiencies that need to be addressed before the manuscript can become suitable for publication in BMC Pregnancy and Childbirth. Specific comments are as follows:

ABSTRACT:

1. Background: The objective of the paper is not clearly articulated. The authors say that 'We explored the reasons for men's resistance to the adoption of a more proactive role in pregnancy care and their enduring influence in the decision making process during emergencies'. I wonder whether the authors were also interested in understanding men's resistance to their … 'enduring influence in the decision making process during emergencies'. Authors should make clear what the purpose is. Maybe the objective should be 'to examine men's attitudes to male involvement in maternal healthcare and barriers to their involvement'.

2. Methods: this ok.

3. Results: some of the results are questionable and inconsistent. More details are provided below in the results section

4. Conclusion: Their conclusion that 'Initiatives to promote male involvement should focus on young men and use chiefs and opinion leaders as advocates to reorient men towards more proactive involvement in ensuring the health of their partners' appears not well supported by the data. Generally, their data suggest that young men really do not have problems becoming more involved. Rather it is the chiefs and opinion leaders who claim some of them are too old to be involved in issues of maternal health. Therefore, I think the real deal is actually engaging more with these chiefs.
INTRODUCTION

The introduction of the paper lacks background information on the maternal health situation in Ghana and the study context more specifically. Why do we need to involve men at all? I think the answer is found in the fact that utilisation of maternal healthcare services is still very low in many African contexts, and men have been found to play important roles in this process. I guess there probably will be no debate about male involvement if no women were giving birth at home and no women were dying because of lack of access to skilled delivery services. Therefore, authors should provide recent data on the maternal health situation in Ghana e.g. ANC coverage, skilled attendance rate, postnatal care rates and maybe contraceptive prevalence.

Also, the paper needs to be strengthened by foregrounding the introduction in previous research on male involvement in maternal health in Ghana. There are a number of recent papers from Ghana that have addressed this issue, and authors need to consider this literature. It is not a good way to sell a paper if the impression is created that work has not been done in this area. Relevant papers in this regard include:


Ganle JK, Dery I & Manu (2016). 'If I go with him, I can't talk with other women': understanding women's resistance to men's involvement in maternal and child healthcare in Northern Ghana. Social Science & Medicine, 166:195-204.

Indeed, the authors could also reference the recent WHO's recommendation for male involvement in maternal health to highlight the importance of the issue. Also, previous studies on the key themes of the paper (i.e. men's attitudes, barriers, and enablers) have not been reviewed and synthesised. Authors need to do this so as to give the reader a sense of what is already known internationally.

METHODS

1. Sampling:

It would be good if the authors provide basic information about the characteristics of their study participants. This is important because it has implications for the kind of results they got as I have remarked below.

-page 7, lines 51: Authors talk about health compounds. As far as I know there are no such things as health compounds. The closest is the community health planning services (CHPS). Authors should clarify this.

2. Data collection
-page 8, line 17, authors talk about 1 hour of in-depth interviews. This would certainly have produced tones of data. Therefore, it is surprising that this is not exactly reflected in the data that their report. Authors should describe in detail what issues were covered in these in-depth interviews, and if part of this data has already been published or are to be published later.

Page 8, line 30, authors talk of the 'larger study' but no reference is made to this larger study anywhere in the paper. If this paper is part of a larger study, then authors should state that as part of the design and indicate whether some of the data from the larger study have already been published.

Page 8, lines 35, authors talk about 'men's enduring role'. This phrase is part of their objective statement but it is not clear to me what they mean? Is it to suggest men's roles are not changing?....

3. Ethics

Page 9, lines 18 /19: authors talk of voluntary verbal consent. Why was no written consent obtained? How was the verbal consent process managed? Was it recorded? Was it witnessed, if so by who? Authors should clearly describe the verbal consent process and mention who administered the verbal consent.

RESULTS

-page 9, lines 32-45, this information does not seem relevant. Also, the claim that opinion leaders have not been traditionally involved in research seems very misleading and authors should provide reference(s) to back this claim if at all they want the information here to be part of their findings. If the claims are valid at all, this section should be added to the justification for selecting these types of participants.

Page 9, lines 51-53: authors write that 'Health seeking decisions, particularly relating to pregnancy and childbirth, are traditionally made primarily on consultation with a soothsayer for a preliminary diagnosis and advice about the plan of action'. I question this claim. On page 5, line 55, authors say that 'Polytheism is common'. They acknowledge that Christians and Muslim are present in their study area. Question is: do Christians and Muslims also consult a soothsayer first? I think it is important that the authors report fully on the type of participants they interviewed as this view does not seem to reconcile with their own observation that the communities practice different forms of worship. Indeed, I recommend the authors use the 'Consolidated Criteria for Reporting Qualitative Research (COREQ)' by Tong et al. (see paper below) to guide the reporting of their methods and findings.

Page 9/10, lines 58-4: which traditional belief systems???

Page 10, lines 9-12: does seeking soothsayer happens in all families? It is very concerning that the results do not appear to reflect the religious heterogeneity that the authors described in their methods section.

Page 12, lines 45-65, it is strange that the authors start the discussion here with the views of healthcare providers about what men can do to support their partners. What really was the essence of having men in the study? The same is true in several instances where healthcare workers are speaking about what they think is the situation with men. If the authors really talked to the men and women as they claimed, then they should allow their voices to be heard.

Page 13, lines 35-40, "The men reported that it was necessary for them to continuously monitor the health of their partners by pouring libation and consistently consult sooth-sayers throughout the period of the pregnancy'. Does this happen in all families? The authors create the impression that everyone in the study was a traditionalist? It is important that they clarify this in their sample selection, otherwise, these findings do not appear to reflect a society that is religiously heterogeneous.

Page 14-17, the results here focus on ANC. Is there any reasons why? Besides, is that the authors definition of maternal healthcare? Would men's attitude be different if it were delivery or postnatal? Did the authors explore these other aspects? If not, then it might be better to rework the title and objective of the paper to focus just on ANC.

Page 17, line 39; 'The need to increase male involvement in maternal health was recognised by all participants'. Previously, authors reported that men did not see the need to be involved! So where is this need to be involved coming from??

Page 18, lines 18-21, I do not see what the surprise is here. The people you interviewed, if they were opinion leaders and chiefs were most likely elderly people. And the intervention was targeting young fathers. Therefore, it is possible that most of the men you interviewed have finished childbearing and were really not going to be involved and were therefore not targeted. So I do not see the issue of effectiveness being an important issue here. Authors need to speak with young men to be able to evaluate this intervention.

Page 19, lines 27-37, which participants suggested the use of the chiefs. From your results, the chiefs were part of your study participants, and most of them were largely unsupportive of male involvement. So I wonder how they are supposed to be used?

Page 19, lines 39-44, was this reported by the chiefs and opinion leaders in your study? If truly this was happening why was this not reported by the opinion leaders???

DISCUSSION
-page 21, line 22, authors talk about results from near miss, which near miss audit?? It appears there are other results that the authors are not reporting and they need to clarify these issues. On the same line, the authors stated that results from the health workers and near miss showed men gave little support to their wives. What really did men say in relation to this? And what was the role of the women in this study? Did they have anything to say to either confirm or repudiate men's involvement or lack of involvement?

There is also no discussion on how men's concerns about inappropriate health facility design and practices make them uncomfortable accompanying their partners to the facilities. How should this be addressed?

Also, authors talked about giving expedited services to women who visit the facility with their husbands? Have the authors considered the possible effects of this, for example women who not have partners or whose partners do not accompany them to health facilities?

CONCLUSION

PAGE 24, lines 37-44, 'Without working with chiefs and other pinion leaders to design innovative strategies for engaging men in maternal health interventions and to give more autonomy to women to take decisions related to their health, reducing maternal mortality and disabilities due to pregnancy would continue to progress at a slow pace'.

In general, while chiefs are important gate-keepers, your results showed that many are actually the ones opposed to or not very supportive of greater male involvement. Rather, many young and educated men are already involved. Is a focus on young men not a better way to generate greater support for women? It is surprising that you are ignoring the young and educated men who are already leading the change.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

Not relevant to this manuscript

**Quality of written English**

Please indicate the quality of language in the manuscript:

Acceptable

**Declaration of competing interests**

Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare I have no competing interests

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal