

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

## ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Contribution of women's development army to maternal and child health in Ethiopia: A systematic review of evidence
<b>AUTHORS</b>	Yitbarek, Kiddus; Abraham, Gelila; Sudhakar, Morankar

## VERSION 1 - REVIEW

<b>REVIEWER</b>	Victor Becerril Montekio Centro de Investigación en Sistemas de Salud Instituto Nacional de Salud Pública - MEXICO
<b>REVIEW RETURNED</b>	29-Sep-2018

<b>GENERAL COMMENTS</b>	<p>I congratulate the authors for an interesting review that may help advance in the field of community workers' participation to improve maternal and child health in low and middle income countries. Nevertheless, first of all I would like to signal the need for a thorough English language review of the article. There are even some paragraphs where the ideas are unclear or even contradictory. For instance, see page 8, lines 23 to 27 for lack of clarity and page 15, lines 28 to 32 for contradictions in the arguments.</p> <p>Below I provide several commentaries and suggestions for particular issues.</p> <p><b>ABSTRACT</b> The methods of the review are not sufficiently described. The question that should be answered here is: How was the review conducted? Instead the number of articles found and of those included is provided but not the way in which this selection was done.</p> <p>At the end of the ABSTRACT a series of strengths and limitations is presented. The first point is the participation of two independent reviewers. The same idea is presented on page 8, line 40. But we have no information as to how any disagreement between them was solved, neither in this part of the manuscript nor in the METHODS section. I consider this is a major issue in conducting reviews.</p> <p>In the METHODS section, inclusion and exclusion criteria are supposed to be presented. Nevertheless, a clear-cut definition of</p>
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each category is lacking and this makes it difficult to understand your methodology.

On page 7, lines 47 to 51 the consideration of studies "that evaluated the effect

of women's development army, and participation in the group, on maternal mortality, maternal and child health service use, health literacy and referral linkage." is mentioned. It seems to me difficult to state a definite quantification of the effect or impact of such interventions as those considered in the review on maternal mortality and the other indicators. Reconsider this statement and describe what the quantitative analysis really made it possible to disclose.

On page 8, lines 46-47 a reference to Research-Gate platform is made that demands a correct citation of the platform.

#### DISCUSSION

In general, I would suggest to summarize several arguments that are already present throughout the preceding sections of the paper and to focus on the main consequences of your findings relating them to the state of the art in the subject.

More specifically, on page 14 lines 14-16 you state that the WDA has contributed "enormously" to the improvement of MCH. But the limited number of the papers included in your review as well as the character of their results does not necessarily support the statement of an "enormous" contribution. The use of such an adjective should be reconsidered.

In the same page, as mentioned at the beginning, lines 28 to 35 need to be reviewed to avoid contradictions.

At the end of page 14 and the beginning of page 15, when discussing the distance from health facilities and WDAs the real issue is how much the participation of WDG's as compared to physical access to health care facilities determines health service use. Stating that distance of less than two kilometers from health facilities made WDAs more effective speaks more of physical access rather than the effect of WDAs. I suggest to reconsider this part of the discussion.

On page 16, lines 32 to 39 a description of HEWs is given that doesn't belong to the discussion section or the idea that you are trying to convey is not made clear.

Regarding what is presented as limitations of the review, first of all, the inclusion of qualitative studies per se makes it impossible to conduct meta-analysis, so it can't really be considered a limitation.

In relation to only free access papers, this would only be a limitation if the authors did find relevant unavailable articles on the first stages of the review and disregarded them because of their cost. But no information is given regarding this.

Finally, the inclusion of the whole texts of JBI's Appraisal Tools seems to me unnecessary. It would suffice to correctly refer to the original source. Including this material as supplementary files is instead cumbersome. What is relevant to your paper is what you adapted and developed as specific tools for your review.

<b>REVIEWER</b>	Ibukun Abejirinde Institute of Tropical Medicine Antwerp, Belgium
<b>REVIEW RETURNED</b>	02-Oct-2018

<b>GENERAL COMMENTS</b>	<p>General Comments</p> <p>In light of the health workforce challenges facing many countries, the ability to leverage other cadres of health workers and informal voluntary workers in facilitating service delivery is important. In this paper, authors examine the contributions that female voluntary community workers in Ethiopia have made to maternal and child health. Using a structured systematic approach and a mix of both quantitative and qualitative data the authors have attempted to synthesise benefits of the 8-year WDA policy/structure on maternal and child health. The importance of this paper especially for policy is reflected in the fact that the Federal Ministry of Health in Ethiopia is interested in answers to the review question.</p> <p>Feedback</p> <p>Title: Given the aim of the paper, authors may consider revising the title to reflect this. A suggestion- Contribution of women’s development army to maternal and child health in Ethiopia: A systematic review of evidence”</p> <p>Abstract: i) Except otherwise mandated by the journal, it may aid clarity to restructure the abstract using the headings: Background, Objectives, Design, Results, Conclusion. ii) If no statistical (or meta-) analysis was conducted the use of certain binding word should be questioned. E.g. the word ‘predictive’ in this sentence: “...poor participation and non-membership in women’s development army were predictors of maternal death and default from child immunization.” Also, “...within two kilometers from health facility is a determinant factor for skilled delivery and antenatal care service use.” Understandably, these conclusions may have been reached by the primary papers reviewed, but are not a direct outcome of this review and should therefore be rephrased. iii) When authors write that “...skilled birth attendance was improved.” Is this related to the quality of SBA or use of SBA services?</p> <p>Introduction: i) This statement is unclear (pg.5 1st para.): “The WDT made its base on “one-to-five” connections, smaller groups of six members (households).” Kindly clarify. I get the impression the intent is to say that one community health volunteer from the WDTs was assigned to five households, hence there would be 6 volunteers per WDT. ii) The repeatedly used term ‘one-to-five networks leaders’ is also rather vague. Do the authors mean sub-group leaders within each WDT structure? Also, please consider clarifying what ‘model family’ is. iii) Please include examples of other development sectors involved in the WDT collaboration (pg.5 3rd para.). “Health extension workers in collaboration with kebele administration and personnel from other developmental sectors facilitate formation of the WDTs and the one-to-five networks.”</p>
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iv) Consider deleting this sentence on pg. 5: "This was the reason for the existence of the structural arrangement"

Methods:

- i) Include in-text reference to supplementary file 1 so the reader is directed to the search strategy.
- ii) Note that in the abstract it is mentioned that only 3 databases were searched. However, in the methods section, at least 4 main databases were searched.
- iii) This sentence can be deleted as the point has already been communicated (Pg. 7, para. 1): "The women's development army approach was launched in Ethiopia since 2010 to support the HEP that was introduced six years back in 2004."
- v) In page 7, paragraph 1, the outcomes of interest in the review ("...This review considered studies that included outcomes like maternal health, maternal mortality, ANC, delivery, family planning, birth preparedness and complication readiness") are more elaborate than what is reported in the abstract. It is best to clarify which outcomes were explored in the review.

Data Collection:

- i) Pg. 7 ".....those records deemed not relevant were verified." Does this mean that the records in question were excluded?
- ii) ii) It would be useful to know if one or more reviewers applied the critical appraisal tools and how differences between raters were reconciled.

Data Synthesis and Analysis:

- i) If the aim of the review was to assess the contribution of women's development army to maternal and child health, and the outcomes of interest in the review were: maternal and child health services as well as maternal and child mortality, its unclear why authors chose to present results based on the sub-headings of i) participation in WDGs, ii) distance of WDGs from health facility and iii) effective functioning of WDGs, as these do not seem to reflect the overall aim of the review. It is my opinion that the synthesis will be richer if the results are presented based on the outcomes of interest.

Results:

- i) Authors have earlier reported selecting only cross-sectional and case-control quantitative studies, so its inconsistent to state on Page 9 that "We have included a total of nine studies regardless of their study design."
- ii) Table 1: were there no studies that addressed more than one maternal and child health issue?
- iii) I am concerned that the findings from the individual papers reviewed are largely repeated in the results section without additional analytical depth or reflection on the implications of how and why WDTs contributed to maternal and child health goals. A way to enrich the synthesis is to compare and contrast experiences and findings between studies; what worked in study A that turned out differently in B and what reasons were given for this related to the organization and effectiveness of WDTs?
- iv) Table 4: did only 5 of the 9 studies have an effect on maternal health services?
- v) Because the proportion of effect of the WDA's was not determined, it is better not to infer that "...women's development army has contributed enormously for the improved maternal and child health"

	<p>Discussion:</p> <p>i) The last paragraph of the discussion section (on HEWs and how they utilize the WDA structure) seems to be better suited for the results section.</p> <p>ii) This section can be improved by comparing and contrasting findings from the WDA experience to that of other countries that have adopted a similar community-based volunteer approach.</p> <p>References:</p> <p>i) Verify publisher and page(s) for Reference #42.</p> <p>ii) Reference #43... 'volunteer' instead of 'colunteer'. Also include doi</p> <p>iii) Include link for References #36, #27 and #28</p> <p>iv) In reference #3, delete 'www.cochranelibrary.com' at end of citation.</p> <p>Supplementary file I:</p> <p>i) There is some text at the bottom of the figure that seems to have been left there in error: "90x90mm (300 x 300 DPI)"</p> <p>General comments:</p> <p>i) 'Evidence' should replace the word 'Evidences' throughout the manuscript. Same with 'researches'....should be 'research'</p> <p>ii) The readability of the paper could be improved after a thorough grammatical check. If possible, authors may want to use an editorial service to improve the clarity and structural flow of their document. The absence of this however, does not diminish the other strengths of the paper.</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Victor Becerril Montekio

Institution and Country: Centro de Investigación en Sistemas de Salud, Instituto Nacional de Salud Pública - MEXICO

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

I congratulate the authors for an interesting review that may help advance in the field of community workers' participation to improve maternal and child health in low and middle income countries.

Nevertheless, first of all I would like to signal the need for a thorough English language review of the article. There are even some paragraphs where the ideas are unclear or even contradictory. For instance, see page 8, lines 23 to 27 for lack of clarity and page 15, lines 28 to 32 for contradictions in the arguments.

Response: we thank you for this invaluable insight. We addressed the raised issues in the revised copy.

Below I provide several commentaries and suggestions for particular issues.

## ABSTRACT

The methods of the review are not sufficiently described. The question that should be answered here is: How was the review conducted? Instead the number of articles found and of those included is provided but not the way in which this selection was done.

Response: thanking you for the suggestion, we have addressed the concern in the abstract section of the revised copy.

At the end of the ABSTRACT a series of strengths and limitations is presented. The first point is the participation of two independent reviewers. The same idea is presented on page 8, line 40. But we have no information as to how any disagreement between them was solved, neither in this part of the manuscript nor in the METHODS section. I consider this is a major issue in conducting reviews.

Response: The first and second reviewers independently participated in quality assessment of articles. Disagreements between the two reviewers were resolved primarily by discussion among themselves and the third reviewer's opinion and decision.

In the METHODS section, inclusion and exclusion criteria are supposed to be presented. Nevertheless, a clear-cut definition of each category is lacking and this makes it difficult to understand your methodology.

Response: thank you for the concern. We have clarified the definition of inclusion and exclusion criteria in the METHODS section of the revised copy.

On page 7, lines 47 to 51 the consideration of studies "that evaluated the effect of women's development army, and participation in the group, on maternal mortality, maternal and child health service use, health literacy and referral linkage." is mentioned. It seems to me difficult to state a definite quantification of the effect or impact of such interventions as those considered in the review on maternal mortality and the other indicators. Reconsider this statement and describe what the quantitative analysis really made it possible to disclose.

Response: we have considered the suggestion and addressed the issue in the revised copy.

On page 8, lines 46-47 a reference to Research-Gate platform is made that demands a correct citation of the platform.

Response: we thank you for the comment. We have cited the platform correctly.

## DISCUSSION

In general, I would suggest to summarize several arguments that are already present throughout the preceding sections of the paper and to focus on the main consequences of your findings relating them to the state of the art in the subject.

More specifically, on page 14 lines 14-16 you state that the WDA has contributed "enormously" to the improvement of MCH. But the limited number of the papers included in your review as well as the character of their results does not necessarily support the statement of an "enormous" contribution. The use of such an adjective should be reconsidered.

Response: we recognized the issue and made a revision.

In the same page, as mentioned at the beginning, lines 28 to 35 need to be reviewed to avoid contradictions.

Response: we recognized the problem and avoided the contradiction in the revised copy.

At the end of page 14 and the beginning of page 15, when discussing the distance from health facilities and WDAs the real issue is how much the participation of WDG's as compared to physical access to health care facilities determines health service use. Stating that distance of less than two kilometers from health facilities made WDAs more effective speaks more of physical access rather than the effect of WDAs. I suggest to reconsider this part of the discussion.

Response: the studies reviewed have assessed variables contributing to ANC and delivery service use at two levels. There were individual level factors and group level factors. Distance of WDGs is among the group level factors. The studies have taken the meeting area of WDGs as a reference to measure distance from health facility. Despite the fact that the meeting area of WDGs was within 2 kilometers, the residence of individual members can be beyond 2 kilometers. Similarly, those members living within 2 kilometers radius meet their colleagues out of 2 kilometers. Therefore, we can conclude that the distance of WDGs, not individual members contribute to the utilization of ANC and delivery service. The explanation for this result can be when the meeting area of WDGs became nearer to health facilities, Health Extension Workers (trained and paid community health workers) and health professionals from health facility can easily follow up WDGs during their meeting.

On page 16, lines 32 to 39 a description of HEWs is given that doesn't belong to the discussion section or the idea that you are trying to convey is not made clear.

Response: we recognized the problem and we revised the statement for clarity.

Regarding what is presented as limitations of the review, first of all, the inclusion of qualitative studies per se makes it impossible to conduct meta-analysis, so it can't really be considered a limitation.

In relation to only free access papers, this would only be a limitation if the authors did find relevant unavailable articles on the first stages of the review and disregarded them because of their cost. But no information is given regarding this.

Response: we have included both qualitative and quantitative studies. We believed that it had been good if we conducted a meta-analysis using at least the quantitative results. But it was difficult. Even the quantitative studies were dissimilar in their study design and outcome they measured.

During the literature search we got articles that were not accessed freely. From them, we could access 3 from Research Gate authors' page. But we couldn't obtain the remaining. We appreciate the comment and we have included this issue in the methods section.

Finally, the inclusion of the whole texts of JBI's Appraisal Tools seems to me unnecessary. It would suffice to correctly refer to the original source. Including this material as supplementary files is instead cumbersome. What is relevant to your paper is what you adapted and developed as specific tools for your review.

Response: thanks for the suggestion. We have removed the tools from supplementary file pages instead we put the references at the appropriate location.

Reviewer: 2

Reviewer Name: Ibukun Abejirinde

Institution and Country: Institute of Tropical Medicine Antwerp, Belgium

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

## General Comments

In light of the health workforce challenges facing many countries, the ability to leverage other cadres of health workers and informal voluntary workers in facilitating service delivery is important. In this paper, authors examine the contributions that female voluntary community workers in Ethiopia have made to maternal and child health. Using a structured systematic approach and a mix of both quantitative and qualitative data the authors have attempted to synthesise benefits of the 8-year WDA policy/structure on maternal and child health. The importance of this paper especially for policy is reflected in the fact that the Federal Ministry of Health in Ethiopia is interested in answers to the review question.

## Feedback

### Title:

Given the aim of the paper, authors may consider revising the title to reflect this. A suggestion- "Contribution of women's development army to maternal and child health in Ethiopia: A systematic review of evidence"

Response: Thanks for the suggestion. Edited in the revised copy as per the suggestion of the reviewer.

### Abstract:

i) Except otherwise mandated by the journal, it may aid clarity to restructure the abstract using the headings: Background, Objectives, Design, Results, Conclusion.

Response: thanks for the concern. The abstract was re-structured in the revised copy following the editor's suggestion as: Objective, Setting, Participants, Outcome, Results and Conclusion

ii) If no statistical (or meta-) analysis was conducted the use of certain binding word should be questioned. E.g. the word 'predictive' in this sentence: "...poor participation and non-membership in women's development army were predictors of maternal death and default from child immunization."

Also, "...within two kilometers from health facility is a determinant factor for skilled delivery and antenatal care service use." Understandably, these conclusions may have been reached by the primary papers reviewed, but are not a direct outcome of this review and should therefore be rephrased.

Response: Thanks for the good insight. We have paraphrased the statements in the revised copy.

iii) When authors write that "...skilled birth attendance was improved." Is this related to the quality of SBA or use of SBA services?

Response: is it to mean the use of skilled birth attendance. Edited in the revised copy.

### Introduction:

i) This statement is unclear (pg.5 1st para.): "The WDT made its base on "one-to-five" connections, smaller groups of six members (households)." Kindly clarify. I get the impression the intent is to say that one community health volunteer from the WDTs was assigned to five households, hence there would be 6 volunteers per WDT.

Response: Thanks for the request. The statements revised for clarity in the revised copy.

The women's development army (WDA) structural arrangement involves a women's development team (WDT) and one-to five connections. WDT involves thirty households within the same



neighborhood. One-to-five connections involve six households (one household considered as the head and five members). Five one-to-five connections together formulate a single WDT. That means a WDT involves five sub groups called One-to-five connection. One-to-five connections' leader is supposed to report to WDT leader.

ii) The repeatedly used term 'one-to-five networks leaders' is also rather vague. Do the authors mean sub-group leaders within each WDT structure? Also, please consider clarifying what 'model family' is.

Response: As described above one-to-five network is a sub-group under each WDT.

There are 16 packages of health extension program (HEP). A family that achieved all 16 packages is recognized as a model family. Leaders of WDT and one-to-five networks are expectedly from a model family in addition to their trustworthiness to the network's members.

iii) Please include examples of other development sectors involved in the WDT collaboration (pg.5 3rd para.). "Health extension workers in collaboration with kebele administration and personnel from other developmental sectors facilitate formation of the WDTs and the one-to-five networks."

Response: the agricultural and educational sectors are the major participants with the health sector in formulating WDT

iv) Consider deleting this sentence on pg. 5: "This was the reason for the existence of the structural arrangement"

Response: Thanks for the concern. Deleted in the revised copy.

Methods:

i) Include in-text reference to supplementary file 1 so the reader is directed to the search strategy.

Response: Thanks for the concern. Already cited at the last line of the first paragraph under the search strategy sub-section.

ii) Note that in the abstract it is mentioned that only 3 databases were searched. However, in the methods section, at least 4 main databases were searched.

Response: Edited in the revised copy of the methods section. We used MEDLINE, CINAHL and EBSCOhost for the search of published articles.

iii) This sentence can be deleted as the point has already been communicated (Pg. 7, para. 1): "The women's development army approach was launched in Ethiopia since 2010 to support the HEP that was introduced six years back in 2004."

Response: deleted in the revised copy.

v) In page 7, paragraph 1, the outcomes of interest in the review ("...This review considered studies that included outcomes like maternal health, maternal mortality, ANC, delivery, family planning, birth preparedness and complication readiness") are more elaborate than what is reported in the abstract. It is best to clarify which outcomes were explored in the review.

Response: Your concern is correct.

In the abstract the outcomes are stated in general terms, but in the methods section more details are available. Edited in the methods section of the revised copy in a way that clarifies the concern.

#### Data Collection:

i) Pg. 7 “.....those records deemed not relevant were verified.” Does this mean that the records in question were excluded?

Response: Thanks for the concern. It is to mean that after we did systematic search the first step was assessing the title and abstracts of the articles. Articles that didn't match the review question were filtered.

ii) It would be useful to know if one or more reviewers applied the critical appraisal tools and how differences between raters were reconciled.

Response: Prior to including the papers to the review quality assessment was done by two independent reviewers (Namely Yitbarek K. and Abraham G.). Disagreements that arise between the reviewers was resolved through discussion, and with the participation of a third reviewer (Prof. Sudhakar M.).

This issue addressed in the description of studies section.

#### Data Synthesis and Analysis:

i) If the aim of the review was to assess the contribution of women's development army to maternal and child health, and the outcomes of interest in the review were: maternal and child health services as well as maternal and child mortality, its unclear why authors chose to present results based on the sub-headings of i) participation in WDGs, ii) distance of WDGs from health facility and iii) effective functioning of WDGs, as these do not seem to reflect the overall aim of the review. It is my opinion that the synthesis will be richer if the results are presented based on the outcomes of interest.

Response: you are correct, we thought this issue while we were writing the report. We tried to synthesize into maternal mortality, and maternal and child health service use. Unfortunately, in the first category (maternal mortality) we found only one article, the remaining were under maternal and child health service use category. Therefore, it became unpleasant for synthesis and reporting. Then we discussed among ourselves and with ministry people to categorize it into the present format.

#### Results:

i) Authors have earlier reported selecting only cross-sectional and case-control quantitative studies, so its inconsistent to state on Page 9 that “We have included a total of nine studies regardless of their study design.”

Response: when we start our literature search we had no any restriction to study design. However, from the quantitative categories we obtained only cross-sectional and case-control studies. That is why we reported only these study designs.

ii) Table 1: were there no studies that addressed more than one maternal and child health issue?

Response: yes, there were. Especially the qualitative studies have seen maternal health service as a whole. Considering these issues we have edited the table in the revised copy.

iii) I am concerned that the findings from the individual papers reviewed are largely repeated in the results section without additional analytical depth or reflection on the implications of how and why WDTs contributed to maternal and child health goals. A way to enrich the synthesis is to compare and contrast experiences and findings between studies; what worked in study A that turned out differently in B and what reasons were given for this related to the organization and effectiveness of WDTs?

Response: we thank you for this important concern. We rather prefer to entertain the issues raised in the discussion section.

iv) Table 4: did only 5 of the 9 studies have an effect on maternal health services?

Response: thanks for the concern. Not only 5 studies have an effect on maternal health service. If we see table 3 there are additional two studies that presenting skilled delivery and ANC use.

v) Because the proportion of effect of the WDA's was not determined, it is better not to infer that "...women's development army has contributed enormously for the improved maternal and child health"

Response: we considered the concern and made changes in the revised copy.

Discussion:

i) The last paragraph of the discussion section (on HEWs and how they utilize the WDA structure) seems to be better suited for the results section.

Response: Taking the comment we made changes in the revised copy.

ii) This section can be improved by comparing and contrasting findings from the WDA experience to that of other countries that have adopted a similar community-based volunteer approach.

Response: we have included additional studies to compare with ours from various low and middle income settings.

i) Verify publisher and page(s) for Reference #42.

ii) Reference #43...'volunteer' instead of 'colunteer'. Also include doi

iii) Include link for References #36, #27 and #28

iv) In reference #3, delete 'www.cochranlibrary.com' at end of citation.

Response: Thanks for the concerns. All the issues raised were addressed in the revised copy.

Supplementary file I:

i) There is some text at the bottom of the figure that seems to have been left there in error: "90x90mm (300 x 300 DPI)"

Response: Yes we recognized that. Originally we haven't put the statement, it was during the submission process the system itself has put it. I hope the editors will consider this issue.

General comments:

i) 'Evidence' should replace the word 'Evidences' throughout the manuscript. Same with 'researches'....should be 'research'

Response: thanks for the suggestion. The changes were made in the revised copy.

ii) The readability of the paper could be improved after a thorough grammatical check. If possible, authors may want to use an editorial service to improve the clarity and structural flow of their document. The absence of this however, does not diminish the other strengths of the paper.

Response: We thank you very much for this invaluable suggestion.

The entire document was revised by scholars with long experience of teaching and research in English.

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Victor Becerril Montekio Centro de Investigación en Sistemas de Salud Instituto Nacional de Salud Pública - MEXICO
<b>REVIEW RETURNED</b>	21-Nov-2018

<b>GENERAL COMMENTS</b>	Congratulations for the effort on a much improved version of the original manuscript. All my previous comments have been correctly addressed.
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<b>REVIEWER</b>	Ibukun Abejirinde Institute of Tropical Medicine, Antwerp, Belgium
<b>REVIEW RETURNED</b>	14-Dec-2018

<b>GENERAL COMMENTS</b>	<p>General Comments</p> <p>Authors have applied some effort to address the comments raised in the first round of feedback. While many of the minor issues have been addressed, major concerns remain with respect to the poor clarity and low depth of synthesis. The revised discussion section shows that the narrative evidence synthesis as presented in the results section is incomplete (see specific details below). Furthermore, grammatical errors still abound.</p> <p>Direct Responses to reviewers' comments:</p> <p>i) May I call the authors attention to two terms on which clarity was requested: "one-to-five-network leaders" and "model family". While these were clearly explained in the cover letter responding to reviewers' comments, the clarity is not reflected in the main manuscript and hence still reads as confusing.</p> <p>ii) Contrary to what should be expected from an evidence synthesis, authors state that they decided to compare and contrast findings from the nine studies reviewed in the discussion section of the paper and not in the results section. It is my impression that the discussion section should be reserved for reflecting on findings within the wider literature. Nevertheless, this enriching comparison is still missing (both in the results and discussion section), which risks not meeting the practical goal of the paper- i.e. to inform National policy.</p> <p>iii) If authors choose to present results with categories that do not reflect the review outcomes, they may want to include one or two sentences to explain what criteria or reasoning guided this decision to categorise results under participation in WDGs, distance of WDGs from health facility and effective functioning of WDGs.</p>
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Other comments:

i) It is concerning that two of the papers (references #24 and #25) on which the synthesis is based seem to have reported a lack of association between WDTs and improved maternal health service utilisation. In the discussion section, authors, citing these papers state “Although there are some information that revealed performance of WDGs does not have anything in improving maternal health service use....”

However, the results section of the paper only presents positive findings on all reviewed papers, implying that the evidence have not been presented completely or objectively.

Additionally, Page 10, lines 9-10: “From 62 died mothers 40 (64.5%) were members of WDTs and among 248 alive mothers 197 (79.4%) were members of WDTs.” It is strange that authors do not reflect on the finding that a higher proportion of the sampled women who died were WDT members.

ii) Discussion points and conclusions drawn sometimes go beyond the boundaries of the evidence presented.

E.g. Pg. 14, lines 37-39: “Good participation of mothers in WDGs was found protective for defaulting from child immunization.” Note that this study found an association amongst other factors related to childhood immunisation.

Pg. 16, lines 9-28: Except you can provide evidence to support this claim, falling National maternal mortality or improved postnatal service attendance over time, cannot necessarily be attributed to the WDA structure.

Pg. 16, lines 40-44: “The results from the review have indicated that even if the reason for the existence of WDA system is to support HEWs for all the health extension packages, their intervention is limited only to maternal and child health service.” If the review aimed to look for effects specific to maternal and child health, it is not expected that authors would find effects on other health packages.

Pg. 16, lines 44-45: “Nevertheless, its contribution to maternal and child health service utilizations and mothers improved health is countless.”  
Countless?

iii) Some sentences related to results have been included in the introduction section. See last sentence on Pg 5. “Finally, it came up with strong evidence to say the WDA structure has an impact in the development of the country’s maternal and child health. Moreover, it has suggested a way to continue and/or extend the intervention for the future.”

iv) Pg. 6, last line- “Eleven potentially relevant studies were found in addition to the three hundred fifty two studies identified in database searches”. Please clarify the specific source(s) of the additional 11 studies.

v) Pg. 12, lines 29-33: “These findings add to the growing body of evidence that health workers at the community level can work with women’s groups to improve maternal health, thus reducing the need for emergency obstetric care in low-income countries.” This belongs to the discussion section

vi) The use and reference to health extension workers or community health workers and WDTs/WDAs almost

	<p>interchangeably, especially in the discussion section, seems to convey the impression that that are one and the same. I imagined the aim of the review was to highlight the added benefit of the WDA structure to the pre-existing HEW strategy.</p> <p>vii) In posing implications of their paper for practice, authors write on Pg 17, lines 13-14 “The responsible bodies in the Ethiopian Federal Ministry of Health have to prepare clear, objective and standardised performance measurement indicators for WDGs.” The basis of the paper and the results presented do not justify such firm recommendations.</p> <p>viii) The paper still requires editing for grammar.</p>
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## VERSION 2 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Victor Becerril Montekio

Institution and Country: Centro de Investigación en Sistemas de Salud, Instituto Nacional de Salud Pública - MEXICO

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Congratulations for the effort on a much improved version of the original manuscript. All my previous comments have been correctly addressed.

Thank you very much for taking time to read the response we gave and accepting the revisions we made.

Reviewer: 2

Dear Dr. Ibukun Abejirinde

Thank you for providing us a second opportunity to revise and resubmit our paper. Below we have addressed all the questions and concerns point-by-point.

Reviewer Name: Ibukun Abejirinde

Institution and Country: Institute of Tropical Medicine, Antwerp, Belgium

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

General Comments

Authors have applied some effort to address the comments raised in the first round of feedback. While many of the minor issues have been addressed, major concerns remain with respect to the

poor clarity and low depth of synthesis. The revised discussion section shows that the narrative evidence synthesis as presented in the results section is incomplete (see specific details below). Furthermore, grammatical errors still abound.

Direct Responses to reviewers' comments:

i) May I call the authors attention to two terms on which clarity was requested: "one-to-five-network leaders" and "model family". While these were clearly explained in the cover letter responding to reviewers' comments, the clarity is not reflected in the main manuscript and hence still reads as confusing.

Response: We are sorry for the confusion we have created in the original manuscript. Revision was made for clarity in the revised copy. You may look at the fourth and fifth paragraphs of the introduction section

ii) Contrary to what should be expected from an evidence synthesis, authors state that they decided to compare and contrast findings from the nine studies reviewed in the discussion section of the paper and not in the results section. It is my impression that the discussion section should be reserved for reflecting on findings within the wider literature. Nevertheless, this enriching comparison is still missing (both in the results and discussion section), which risks not meeting the practical goal of the paper- i.e. to inform National policy.

Response: we are sorry for misunderstanding the comments given during the first review. This was a perfect concern. I thought the intention of the reviewer was to let us compare the findings with other studies. Now we made changes in the results section at the level of writing-up it again.

iii) If authors choose to present results with categories that do not reflect the review outcomes, they may want to include one or two sentences to explain what criteria or reasoning guided this decision to categorise results under participation in WDGs, distance of WDGs from health facility and effective functioning of WDGs.

Response: during the introduction of WDA structure the main objective was to improve physical access, to inspire the community to involve themselves in their health matters and effectively implement some selected PHC activities at the grass root level. These objectives were derived depending on PHC principles. Based on this premises we have categorized our synthesis in to participation in WDTs, distance of WDTs from health facility and effective functioning of WDTs.

Other comments:

i) It is concerning that two of the papers (references #24 and #25) on which the synthesis is based seem to have reported a lack of association between WDTs and improved maternal health service utilisation. In the discussion section, authors, citing these papers state "Although there are some information that revealed performance of WDGs does not have anything in improving maternal health service use....."

However, the results section of the paper only presents positive findings on all reviewed papers, implying that the evidence have not been presented completely or objectively.

Response: thanking you for the important insight, we now addressed the issue in "Effective functioning of WDTs" subsection.

Additionally, Page 10, lines 9-10: "From 62 died mothers 40 (64.5%) were members of WDTs and among 248 alive mothers 197 (79.4%) were members of WDTs." It is strange that authors do not reflect on the finding that a higher proportion of the sampled women who died were WDT members.

Response: we have addressed it according to the concerns raised by the reviewer.

ii) Discussion points and conclusions drawn sometimes go beyond the boundaries of the evidence presented.

E.g. Pg. 14, lines 37-39: "Good participation of mothers in WDGs was found protective for defaulting from child immunization." Note that this study found an association amongst other factors related to childhood immunisation.

Response: We have made revisions to address this concern. Please look at the revised copy.

Pg. 16, lines 9-28: Except you can provide evidence to support this claim, falling National maternal mortality or improved postnatal service attendance over time, cannot necessarily be attributed to the WDA structure.

Response: This concern is correct and this section has been transferred to the introduction section. Changes in the indicators totally cannot be attributed to the structure. But when we see the trends improvements in various maternal and child health indicators after the introduction of WDA is higher than the previous periods. For example, maternal death was 673 in 2005 and it became 676 in 2011 per 100 000 live births, it has shown an increase by 3, in the meantime in 2010 WDA structure was introduced, then MMR became 412 in 2016; it is 264 decrease. Similarly, institutional birth has shown only a 5% increase between 2005 and 2011; but it was a 16% increase between 2011 and 2016. The other main thing has to be understood is the introduction of WDA was the major policy shift in the period. Even if we cannot hide the contribution of other services, the special thing during the period was WDA. Therefore, we can conclude that WDA has a big contribution to the improvements.

Pg. 16, lines 40-44: "The results from the review have indicated that even if the reason for the existence of WDA system is to support HEWs for all the health extension packages, their intervention is limited only to maternal and child health service." If the review aimed to look for effects specific to maternal and child health, it is not expected that authors would find effects on other health packages.

Response: Thanks for your insightful comment. We have addressed the concern in the revised copy.

Pg. 16, lines 44-45: "Nevertheless, its contribution to maternal and child health service utilizations and mothers improved health is countless." Countless?

Response: we have addressed the concern in the revised copy.

iii) Some sentences related to results have been included in the introduction section. See last sentence on Pg 5. "Finally, it came up with strong evidence to say the WDA structure has an impact in the development of the country's maternal and child health. Moreover, it has suggested a way to continue and/or extend the intervention for the future."

Response: thanks for the concern. We have deleted the statements from the introduction in the revised copy.

iv) Pg. 6, last line- "Eleven potentially relevant studies were found in addition to the three hundred fifty two studies identified in database searches". Please clarify the specific source(s) of the additional 11 studies.

Response: additional studies were obtained from free search on google scholar and google search bars. We have included this in the revised copy "description of studies" sub section

v) Pg. 12, lines 29-33: "These findings add to the growing body of evidence that health workers at the community level can work with women's groups to improve maternal health, thus reducing the need for emergency obstetric care in low-income countries." This belongs to the discussion section

Response: Thanks for the suggestion. We have addressed it in the revised copy.



vi) The use and reference to health extension workers or community health workers and WDTs/WDAs almost interchangeably, especially in the discussion section, seems to convey the impression that that are one and the same. I imagined the aim of the review was to highlight the added benefit of the WDA structure to the pre-existing HEW strategy.

Response: Thank you for your insightful correction. In the document especially in the discussion WDT and WDG were used interchangeably, but now we use WDT consistently. But WDA is a structural arrangement that involves WDT, one-to-five connections, their leadership, the reporting linkage, relationship with HEWs and health facilities, the duties and responsibilities of leaders and members etc. But WDT is a single group of thirty members

vii) In posing implications of their paper for practice, authors write on Pg 17, lines 13-14 "The responsible bodies in the Ethiopian Federal Ministry of Health have to prepare clear, objective and standardised performance measurement indicators for WDGs." The basis of the paper and the results presented do not justify such firm recommendations.

Response: we have addressed the issue according to the comment given.

viii) The paper still requires editing for grammar.

Response: we have revised the whole document for grammatical issues and let a native speaker to check it.