

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Assessing community health workers' performance motivation: a mixed-methods approach on India's Accredited Social Health Activists (ASHA) program
<b>AUTHORS</b>	Gopalan, Saji ; Mohanty, Satya; Das, Ashis

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Asha George, Assistant Professor, Johns Hopkins School of Public Health, Baltimore, USA.  No competing interests.
<b>REVIEW RETURNED</b>	01-Jul-2012

<b>THE STUDY</b>	<p>MISSING LITERATURE that is relevant: 1) Scott K, Shanker S. Tying their hands? Institutional obstacles to the success of the ASHA community health worker programme in rural north India. <i>AIDS Care</i>. 2010;22 Suppl 2:1606-12. 2) Kane SS, Gerretsen B, Scherpbier R, Dal Poz M, Dieleman M. A realist synthesis of randomised control trials involving use of community health workers for delivering child health interventions in low and middle income countries. <i>BMC Health Serv Res</i>. 2010 Oct 13;10:286. Review. 3) Daniels K, Van Zyl H H, Clarke M, Dick J, Johansson E. Ear to the ground: listening to farm dwellers talk about the experience of becoming lay health workers. <i>Health Policy</i>. 2005 Jul;73(1):92-103. 4) George A. Nurses, community health workers, and home carers: gendered human resources compensating for skewed health systems. <i>Glob Public Health</i>. 2008;3 Suppl 1:75-89.</p> <p>METHODS section is unclear. How long did data collection take? Who did the data collection? How was it supervised, quality maintained? If 10% was sample how did they manage to miss a significant part of this sample. Most surveys keep going until they reach their sample and keep track of who and why refusals/ non availability occurs. How does interviewing only those CHWs that were available affect results? No description of districts selected on the basis of administrative decision...are they representative of the state? How were CHWs gathered for FGDs? Methods section should discuss survey and FGDs separately rather than lump them together. e.g. not sure if women's groups were used for selecting ASHAs for FGDs or for survey, or both. What are the implications of relying on women's groups for your sample, rather than a list maintained by government? What type of mixed methods study selected and why is not explained.</p> <p>Authors should refer to Creswell and specify whether their study was</p>
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	sequential, concurrent, explanatory, exploratory etc. Authors should also refer to Gilson et al. 2011 PlosMed on rigour for social science studies in health systems.
<b>RESULTS &amp; CONCLUSIONS</b>	<p>RESULTS are not presented clearly. Level of motivation and determinants of motivation are mixed up. Findings are repeated more than once in the same section.</p> <p>CONCLUSIONS are more wide ranging than the data presented, especially with regards to gender implications and level of empowerment due to program. Research from Brazil covered in the George review of gender and human resources for health found that women workers targetting women beneficiaries made it difficult for them to address men or gender issues that were powerful underlying determinants of the health outcomes they were trying to change. Daniels and Scott are also more balanced in their assessment of what these programs mean for female volunteers.</p>
<b>GENERAL COMMENTS</b>	This is an important study with potentially good data. Findings help to substantiate more systematically what is known anecdotally. Unfortunately, the presentation of methods, findings, discussion and style of writing warrants major improvement if the study results are to be communicated effectively.

<b>REVIEWER</b>	<p>Prudence Ditlopo  Researcher  Centre for Health Policy  School of Public Health  University of Witwatersrand  South Africa</p> <p>There are no conflicts of interest</p>
<b>REVIEW RETURNED</b>	09-Jul-2012

<b>THE STUDY</b>	<p>In page 2, lines 27-29, your research question 3 (bullet 3) is not clear, it is also not clear how different is it to research question 2 (bullet 2).</p> <p>The authors reported using "mixed methods" approach. Can the authors clarify whether the methods were conducted concurrently or whether one methodology informed the other?</p> <p>Under the "Assessment tools" section, last sentence (line 15 on page 6) where the authors say "the FGDs explored CHWs recent experiences and perceptions..." I suggest that they replace "recent" with "current"</p> <p>The authors need to clarify if the survey questions were self administered or not. And if self administered, the limitations of this.</p> <p>The first sentence under "Sampling and recruitment" (lines 22-25) needs to be revised, it doesnt read well. It is also not clear how the 386 CHW were ultimately selected, whether there was an inclusion or exclusion criteria.</p> <p>How were the participants for the focus groups selected; were the 11 groups homogenous or not? Were the participants for the FGDs the same as those who responded to the survey? Was an FGD guide</p>
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	<p>used or not? How was rigour maintained with regard to the translation of the FGDs?</p> <p>The standard of English could also be improved, although it is not bad. There are occasions where the authors have written very long sentences and it makes it difficult to follow at times.</p>
<b>RESULTS &amp; CONCLUSIONS</b>	<p>The first paragraph of the results section is too loaded, perhaps this can better be explained in a Table.</p> <p>The results are interesting but the authors write very long sentences, making it difficult to follow the argument. Succinct reporting of the results may be useful.</p> <p>Line 6 on page 8 does not read well, it needs to be revised "the highest number of CHWs were motivated on self motivation". It may be better to say "A large proportion of community health workers scored high on self motivation"</p> <p>Lines 39-46 in page 9 needs to be clarified or revised.</p> <p>The author kept on saying "trainings" instead of "training"</p> <p>The labeling of the quotations implies that these were from individual interviews (e.g CHW#28) ; they are not labeled as FGD participants. It is therefore not clear whether these are from a single FGD or a variety.</p> <p>It is not clear how the quotation in lines 32-34 by CHW#74 is a demotivating factor, this needs to be clarified.</p> <p>A large chunk of the qualitative results are not supported by use of quotation, I suspect that this could be due to word limit issues. As such, it appears as if the results are the authors interpretation only when there are no quotes e.g. line 39-56 in page 10 and 4-15 in page 11</p> <p>The discussion needs to be re written or revised hugely, it doesnt seem to be based on the results. For instance, your results did not say much about incentives but in your discussion and conclusion there is reference of that. There are also other large chunks that are not based on the results. There is also no coherence, evidence is just thrown in there but the argument that the authors are making is not explicitly clear.</p>
<b>GENERAL COMMENTS</b>	<p>In the title and throughout the paper, the authors refer to community health workers "performance motivation". They have joined these two concepts as one meanwhile in the literature these have different definitions. For instance, Alkire &amp; Deneulin (2002) defined motivation as "that which tends to move people to act". Meanwhile, Bennett and Franco (1999) defines motivation in the workplace as "an individual's degree of willingness to exert and maintain effort towards organizational goals". Performance on the other hand is defined by Rowe et al (2005) as "adherence to an accepted standard or guidelines". There may be other definitions by other authors, I am not implying that these are the only definitions. I have noted in page 5 that the authors provided their own definition of "performance motivation". It might be useful to indicate how they came about with this definition since it is not referenced. I have also noted that in their definition, they have said the degree of interest and willingness to undertake, "maintain" and improve...But it is not clear from the 16 parameters as to which of these measures the "maintain" part of the</p>

	<p>definition. Again as one reads the paper, it appears that there is more reference to motivation than there is to performance. Even the reference list has more articles on motivation than performance. So perhaps this needs to be looked at also; to have a balance. This comment applies to the entire paper.</p> <p>The results section needs to be written in a simplified and clear manner, the authors should avoid using very long sentences.</p> <p>The section on "ASHA Program: An Overview" is written as if it refers to one woman; this needs to be revised slightly to accommodate the fact that this is about the entire program.</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer: George Asha

MISSING LITERATURE that is relevant: 1) Scott K, Shanker S. Tying their hands? Institutional obstacles to the success of the ASHA community health worker programme in rural north India. *AIDS Care*. 2010;22 Suppl 2:1606-12. 2) Kane SS, Gerretsen B, Scherpbier R, Dal Poz M, Dieleman M. A realist synthesis of randomised control trials involving use of community health workers for delivering child health interventions in low and middle income countries. *BMC Health Serv Res*. 2010 Oct 13;10:286. Review. 3) Daniels K, Van Zyl H H, Clarke M, Dick J, Johansson E. Ear to the ground: listening to farm dwellers talk about the experience of becoming lay health workers. *Health Policy*. 2005 Jul;73(1):92-103. 4) George A. Nurses, community health workers, and home carers: gendered human resources compensating for skewed health systems. *Glob Public Health*. 2008;3 Suppl 1:75-89.

Authors' response: We have added these four references in the relevant places in the discussion section (page 12, para 1)

METHODS section is unclear. How long did data collection take? Who did the data collection? How was it supervised, quality maintained? If 10% was sample how did they manage to miss a significant part of this sample. Most surveys keep going until they reach their sample and keep track of who and why refusals/ non availability occurs. How does interviewing only those CHWs that were available affect results? No description of districts selected on the basis of administrative decision...are they representative of the state? How were CHWs gathered for FGDs? Methods section should discuss survey and FGDs separately rather than lump them together. e.g. not sure if women's groups were used for selecting ASHAs for FGDs or for survey, or both.

Authors' response: During the survey a few ASHAs from the selected blocks were undergoing trainings on child health and we had limitations in time and resources. That is why we could not continue to achieve our targeted sample size. We don't think interviewing only the available CHWs would have affected the results as the CHWs undergoing trainings during the survey were new recruits. The rest of the comments have been addressed in the methods section under "Sampling and recruitment" (page 6- para 2, page 7-para 1- 2)

What are the implications of relying on women's groups for your sample, rather than a list maintained by government?

Authors' response: We had interviewed only the CHWs who were ASHAs as per the record of the Government. We had used the women's groups only to mobilize them and we did not depend on the list of the women's groups.

What type of mixed methods study selected and why is not explained.

Authors' response: Usually the term 'mixed methods' is a well-known usage when we combine both qualitative and quantitative research techniques. Now, considering the comment of the reviewer, we

have added this to the first sentence of the section “Assessment tools” (Page 6, para 1). What is the relevance of the mixed method approach was already explained in the section “strengths and limitations of the study” (Page 16, para 1)

Authors should refer to Creswell and specify whether their study was sequential, concurrent, explanatory, exploratory etc.

Authors’ response: It may not be wise to use only a reference and decide the nature of a study. This study has multiple research questions and each vary in their nature of research exploration entirely from one another. For instance, there is an element of descriptive probe (e.g. how the CHWs feel about the factors affecting their motivation). It has also an exploratory probe (e.g. finding out new ideas or evidences on the factors affecting CHW’s performance motivation). Further, it has an explanatory probe (e.g. we looked at the cause-effect relationship among the CHW’s level of motivation and the status of the determinants). Therefore, we had to describe the nature of the study based on the time sequence. Thus, we had mentioned that ‘it was a cross-sectional study’ (Page 6, para 1)

Authors should also refer to Gilson et al. 2011 PlosMed on rigour for social science studies in health systems.

Authors’ response: The existing literature is inconclusive on the scientific rigour of the social science research methods on the health systems. This is because there are diverse opinions on this. Therefore, it may not be wise to follow one reference and design the study exclusively based on that. However, we have considered some of the parameters within the scope of this study to highlight the strengths and weaknesses of the study design (Page 16, para 1).

RESULTS are not presented clearly. Level of motivation and determinants of motivation are mixed up. Findings are repeated more than once in the same section.

Authors’ response: We have simplified the results section. The level of motivation and determinants are now under different heads (page 9).

CONCLUSIONS are more wide ranging than the data presented, especially with regards to gender implications and level of empowerment due to program. Research from Brazil covered in the George review of gender and human resources for health found that women workers targeting women beneficiaries made it difficult for them to address men or gender issues that were powerful underlying determinants of the health outcomes they were trying to change. Daniels and Scott are also more balanced in their assessment of what these programs mean for female volunteers.

Authors’ response: Gender mainstreaming on the community health approach (i.e. building up women’s groups capabilities, enhanced health awareness of women and social cohesion) is one of our study findings. The point that the reviewer has raised i.e. involving women as grass-roots level care-givers than men and the gender sensitivity in this approach is still a debatable topic. We respect and accept the findings in the African and Latin American contexts on the skewed distribution of this role.

However, in our settings, women were excited to get the opportunity to become a CHW. They never posed any concern on this skewed distribution of care giving responsibility among men and women. The system is in a transition and considering the ‘base-line’ of the settings, the current improvements are definitely welcome. An absolute non-attention to the ‘gender sensitivity’ could be definitely not due to the fact that the system is gender-blind, but due to its structural limitations and the inability to meet even the bare basic needs.

No mention of IRB.

Authors’ response: This study did not go through a specific IRB. However, we ensured the utmost ethical standards by obtaining the informed consent from the subjects after explaining them the objectives of the study. The subjects had the option of not participating in the study if they wanted to and there was no coercion or enticement of any sort for participation. Further, since this was on a

government program, we obtained the approval of the local health administration.

Reviewer: Prudence Ditlopo

In page 2, lines 27-29, your research question 3 (bullet 3) is not clear, it is also not clear how different is it to research question 2 (bullet 2).

Authors' response: Bullets 2& 3 are certainly different, 2- finds out the factors affecting the level of motivation through quantitative means (it only talks about the causality from the survey data). 3- explores the current status of the determinants of motivation from the live perspectives of the CHWs (it explores how and why).

The authors reported using "mixed methods" approach. Can the authors clarify whether the methods were conducted concurrently or whether one methodology informed the other?

Authors' response: We have addressed this comment (page 7, para 2, last sentence)

Under the "Assessment tools" section, last sentence (line 15 on page 6) where the authors say "the FGDs explored CHWs recent experiences and perceptions..." I suggest that they replace "recent" with "current"

Authors' response: We have replaced this.

The authors need to clarify if the survey questions were self administered or not. And if self administered the limitations of this.

Authors' response: The section "sampling and recruitment" says about this comment. The survey was not self-administered.

The first sentence under "Sampling and recruitment" (lines 22-25) needs to be revised, it doesn't read well. It is also not clear how the 386 CHW were ultimately selected, whether there was an inclusion or exclusion criteria.

Authors' response: We have rewritten the 'sampling' section (page 6 and 7)

How were the participants for the focus groups selected; were the 11 groups homogenous or not? Were the participants for the FGDs the same as those who responded to the survey? Was an FGD guide used or not? How was rigour maintained with regard to the translation of the FGDs?

Authors' response: We have addressed this comment in the section "data analysis" (page 7)

There are occasions where the authors have written very long sentences and it makes it difficult to follow at times.

Authors' response: Some of the long sentences are now shortened

The first paragraph of the results section is too loaded, perhaps this can better be explained in a Table.

Authors' response: This paragraph is restructured now.

Line 6 on page 8 does not read well, it needs to be revised "the highest number of CHWs were motivated on self motivation". It may be better to say "A large proportion of community health workers scored high on self motivation" Lines 39-46 in page 9 needs to be clarified or revised.

Authors' response: This sentence has been restructured

The author kept on saying "trainings" instead of "training"

Authors' response: The CHWs undergo a cascade of trainings. Therefore, we used "trainings" instead of "training" in some places.

The labeling of the quotations implies that these were from individual interviews (e.g CHW#28) ; they are not labeled as FGD participants. It is therefore not clear whether these are from a single FGD or a

variety.

Authors' response: There were 11 FGDs and each had around 7-10 members. Since, the CHW numbers we have quoted in the text display a wide range, they are certainly from different FGDs. It is not clear how the quotation in lines 32-34 by CHW#74 is a demotivating factor, this needs to be clarified.

Authors' response: We have restructured this quote now.

A large chunk of the qualitative results are not supported by use of quotation, I suspect that this could be due to word limit issues. As such, it appears as if the results are the authors' interpretation only when there are no quotes e.g. line 39-56 in page 10 and 4-15 in page 11

Authors' response: We agree with the Reviewer's observation. The word limit of the journal restricts presenting quotes for each observation. However, all the results are authors' interpretation borne out of the FGDs. We have added a couple of more quotes in a few places.

The discussion needs to be re written or revised hugely, it doesn't seem to be based on the results. For instance, your results did not say much about incentives but in your discussion and conclusion there is reference of that. There are also other large chunks that are not based on the results. There is also no coherence, evidence is just thrown in there but the argument that the authors are making is not explicitly clear.

Authors' response: We have now more descriptions on the incentives in the results section. We feel that one of the strengths and highlights of this paper is that it brings in ample policy triggers. This paper needs to have a policy approach as it focus on one of the largest public sector CHW programs in the world. We accept that sometimes from an academic perspective, it might look as broad-based. However, each thread of the discussion is based on a study finding, but it has brought in wider health system issues to make the CHW program more effective. On understanding the policy dynamics of the CHW programs in the study settings and other low-and middle-income countries, the discussion dragged in many issues. We were chalking out at the policy level what really favors and constraints the performance motivation of the CHWs.

In the title and throughout the paper, the authors refer to community health workers "performance motivation". They have joined these two concepts as one meanwhile in the literature these have different definitions. For instance, Alkire & Deneulin (2002) defined motivation as "that which tends to move people to act". Meanwhile, Bennett and Franco (1999) defines motivation in the workplace as "an individual's degree of willingness to exert and maintain effort towards organizational goals". Performance on the other hand is defined by Rowe et al (2005) as "adherence to an accepted standard or guidelines". There may be other definitions by other authors, I am not implying that these are the only definitions. I have noted in page 5 that the authors provided their own definition of "performance motivation". It might be useful to indicate how they came about with this definition since it is not referenced. I have also noted that in their definition, they have said the degree of interest and willingness to undertake, "maintain" and improve...But it is not clear from the 16 parameters as to which of these measures the "maintain" part of the definition. Again as one reads the paper, it appears that there is more reference to motivation than there is to performance. Even the reference list has more articles on motivation than performance. So perhaps this needs to be looked at also; to have a balance. This comment applies to the entire paper.

Authors' response: we have addressed this comment (page 5, last para). The focus of this paper is 'motivation and its determinants' and we have not explored the level of actual performance of the CHWs. This has been already mentioned in the section "strengths and limitation of the study" (page 16).

The section on "ASHA Program: An Overview" is written as if it refers to one woman; this needs to be revised slightly to accommodate the fact that this is about the entire program.

Authors' response: The term 'program' has been already mentioned in the section. However, we have now used ASHAs instead of ASHA to give a broader feeling (page 5, 2 para).