Author’s response to reviews

Title: The effect of a community health worker intervention on public satisfaction: evidence from an unregistered outcome in a cluster-randomized controlled trial in Dar es Salaam, Tanzania

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Author’s response to reviews:

Dear Reviewers and Human Resources for Health Editor,

Thank you for taking the time to conduct a thorough review of our manuscript titled “The effect of a community health worker intervention on public satisfaction: evidence from an unregistered outcome in a cluster-randomized controlled trial in Dar es Salaam, Tanzania”. We appreciate your comments and feel that they have helped to significantly improve our manuscript. We have made edits (in track changes) directly to the manuscript and responded to each of your individual points below following the word "response". If you have any further questions, please do not hesitate to reach out to us.

Best wishes,

Co-Authors

Point-by-point response to reviewer comments:

Reviewer #1: Overview: The aim of the paper as reflected in the background conflicts with the method of approach and the result therefore is not be sufficient to draw the conclusion that CHW programmes in themselves can lead to public satisfaction with the health system. Secondly and assuming that all bias is overlooked, a more appropriate conclusion will shift the emphasis to maternal health services provided by CHWs rather than CHWs in general themselves.

Response: We agree that it is important that we are clear that the focus of this intervention and evaluation was on maternal health. We have made edits throughout, including editing the aim of the paper in the background section to be more precise. The abstract now reads “This paper determines the effects of a CHW program focused on maternal health services on public satisfaction with the health system among women who are pregnant or recently delivered.” And the background section of the main text now reads ‘We sought to fill this gap in knowledge by analyzing data from a population-based cluster-randomized controlled trial to measure the effect of a CHW intervention for maternal and child health on public satisfaction with the health system among women who were pregnant or recently delivered.” We have also edited the discussion section and the conclusion to be more focused. The discussion section now begins with “This cluster-randomized community health worker intervention provides evidence that CHWs providing maternal health support are able to improve women’s satisfaction with both the CHW program and the overall public-sector health system in Dar es Salaam.” The conclusion section now begins with “This study provides evidence that community health worker
interventions can lead to improved public satisfaction with the health system among women who are pregnant or recently delivered.”

Abstract and general comments: Randomised controlled trials are designed to answer specific primary and secondary outcome measures. There is no reference to public satisfaction among the documented outcomes measures in the original registry file (NCT01932138). This paper is a post-trial survey that attempts to answer a different research question from the original trial itself and therefore is misleading if published as a trial in itself. The original trial (see reference above) had 190,530 participants compared to the 2,329 women who participated in the survey.

Response: We thank the reviewer for this thoughtful comment. Public satisfaction with the health system was not a pre-registered primary or secondary endpoint for this trial. We agree that this is an important limitation of this analysis. We have now clarified this in the manuscript by stating in the methods: "The satisfaction outcome variables were not pre-registered primary or secondary endpoints of this trial." In addition, we state in the limitations: "The outcome examined in this analysis was not a pre-registered endpoint of this trial. The results should therefore be interpreted as being hypothesis-generating rather than conclusive evidence." The sample size stated in our pre-registration is of the number of women who participated in the trial. We were initially planning to assess the trial's main endpoints using clinical registers but due to data quality issues reverted to conducting a population-based survey at the end of the trial period instead. The number of women who participated in this survey was 2,329. We now clarify this in the methods: "We had set out to measure all endpoints of this trial through clinic-based registers. However, because of concerns regarding data quality and problems with linkage across clinical registers, the study team decided while the trial was ongoing to instead conduct a population-based survey at the end of the study period. The results reported here are from this population-based survey." Finally, we have edited the title of the paper to make it clear that the main outcome was unregistered. It now reads “The effect of a community health worker intervention on public satisfaction: evidence from an unregistered outcome in a cluster-randomized controlled trial in Dar es Salaam, Tanzania.”

The paper needs to be clearer on whether it is examining public satisfaction based on the services provided by the selected CHWs or pre-existing CHWs contracted by the public health systems (It is mentioned somewhere in the paper that a CHW structure already existed).

Response: In the “variables and analysis” section we have listed the two variables and added additional text that reads “In addition, respondents were asked to state their satisfaction with the CHW program on the same scale. The survey question did not specify CHWs trained specifically by the intervention. It is thus likely that respondents were evaluating the CHW program they were exposed to, which would have been the standard program in control areas and the enhanced program in intervention areas.”

The insinuation that people who are "not using care" do not do so because they are "most likely to be dissatisfied" with the current health system is simplistic. For example, could it be that some may already appreciate the public services but are unable to access it due to financial, ignorance/lack of knowledge reasons? In general, the tendency is for individuals to expect better from learned or experienced professionals than from other health workers less so trained (CHWs
in this case) to provide the same service. As such, the level of satisfaction attributed to CHW interventions may be amplified due to this factor. For example, what is the current measure of public satisfaction in services provided by higher level cadres? More is likely to be expected from them for the same type of service.

Response: We agree with the reviewer that satisfaction is both a complex and nuanced construct. We also agree that expectations play a role in reported satisfaction. We also know from research conducted by co-authors in Tanzania and colleagues in other countries both within and outside East Africa, that those who are dissatisfied with services are less likely to report intent to return for care. Satisfaction is one of many factors that has been hypothesized and empirically shown to be associated with utilization. We maintain that it is thus supported to claim that those who do not use services are likely to be less satisfied with the health system than those who do. We have moderated our language here in the background section where this point is referenced to now read “It is precisely these intended beneficiaries of healthcare who are not engaging with the health system who should be the target of interventions to improve public satisfaction with the health system – one of many reasons they are not using the system could be that they are dissatisfied with the current system. Non-users are also likely to derive a benefit from increased use of healthcare.” We have also added relevant citations. We have made an additional edit to moderate the language in the discussion section as well, which now reads: “This measure includes those individuals who are not using the system for reasons that could be related to their satisfaction, thus giving us a more true assessment of the impact of the intervention on the target population.” We have not gone into a discussion of how expectations will play a role on varying level of satisfaction with different cadres as this is not something that we measure or discuss in this paper. It would be a very interesting study to look at empirically in the future, but not one that we can address with this study.

Methodology: The paper reports the percentage increase in public satisfaction but does not mention anywhere that a baseline measure of public satisfaction was obtained. The y-axis of figure 2 reports percentage changes but also goes without a label.

Response: The analysis compares the intervention group to the control group at endline. We have edited the text of the results to make this clear, rather than saying an “increase” the text now reads: “The proportion of women reporting they were satisfied or very satisfied (rather than neutral, dissatisfied, or very dissatisfied) with the CHW program was 16 percentage points higher (95% CI: 3, 30) in the intervention arm than the control arm, and the proportion of women reporting they were satisfied or very satisfied with the public-sector health system was a significant 15 percentage points higher (95% CI: 3, 27) in the intervention arm than in the control arm.” We have also edited the y-axis of figure 2 so that it has a label that reads “Percentage point difference between intervention and control groups.”

It is a generalization to assume that outreach services on maternal and child health and targeted predominantly or exclusively at women will provide a complete assessment that could equate to general satisfaction with the wider health systems.

Response: We have made a several modifications throughout the text to make it clear that the public satisfaction that we are referring to is among women who are pregnant or recently
delivered a child. This includes editing the definitional sentence in the background section to now read “From a health system perspective, however, satisfaction with the health system among the entire target population, in this case all women who are pregnant or have recently delivered, (henceforth referred to as ‘public satisfaction’)...”Because the question women responded to referred to the general public health system in Dar es Salaam, we have maintained this language throughout.

The study outcome is itself very heavily biased by the fact that CHWs were the ones extracting and recording the survey data even if they were sent to unfamiliar locations. An analogy for example can be to send police officers to provide security interventions and then ask the same recipients about their level of satisfaction with public security. The tendency will be for participants to report favourable outcomes to the collectors. The data collectors (CHWs as in the case of this study) could also bias participants towards reporting favourably in their interests especially if the outcomes would portray them in a better light.

Response: We agree that using CHWs to survey women was not an ideal choice. The reviewer has identified two valid concerns for bias. In the first, it is a concern that there would be some social desirability bias. While this would likely occur in both intervention and control areas, thus making the difference between the two (which is the main statistic of interest in this paper) still valid, it is possible that it would occur more in the intervention areas. We have made sure to clearly outline this potential source of bias in the limitation section. The second form of bias would be if there is some form of interviewer bias where the CHWs biased respondents, either intentionally or subconsciously, to report more favorably on them in intervention areas. The study team made efforts to prevent this form of bias through training of data collectors and through the use of structured surveys that did not have leading questions. We have addressed this in the limitation section with an additional sentence.

Result: In table 2: Suggest adding the term "satisfaction with community health workers services" and possibly, "satisfaction with public health system services". Depending on the study aim, providing this specificity will help clarify exactly what you are trying to compare. Satisfaction with health systems services in general or satisfaction with public providers of health services whether CHWs or higher level health workers?

Response: Thank you for raising this very important distinction. We returned to the original questionnaire to make sure that our labels match the question directly. The two questions were “Overall, how satisfied are you with the public healthcare system in Dar es Salaam?” and “How satisfied are you with the [CHW] program?” We have thus changed the labels in table 2 to read “Satisfaction with the public healthcare system in Dar es Salaam” and “Satisfaction with community health worker program.” We also went through the entire text to make sure that we were consistent throughout with this terminology.

Discussion: It is difficult to see the critical link between "improved levels of satisfaction" and public health systems other than the CHW visits/interventions which many previous studies have already established. The "multiple pathways" as described for public health system satisfaction currently indicate elements that CHWs have traditionally discharged e.g. patient education as part of their routine activities.
Response: The section that describes the pathways through which the CHW visits/interventions may have affected public satisfaction is meant to provide a more nuanced discussion, which could help future researchers investigate these pathways in more detail. We agree that the pathways include elements traditionally discharged by CHWs. We are not aiming to say that this intervention was dramatically different from other CHW interventions, but that the evaluation of public satisfaction was unique and the results should be considered when policy-makers consider CHW programs. We have not found that many other previous studies have established a link between CHW programs and improved satisfaction. We cite two systematic reviews and only one out of 26 studies looks at this link. If the reviewer could provide citations for additional studies, we would be more than happy to include them in our discussion.

Conclusion: A stronger argument that concretely ties the link of CHW interventions to improved public satisfaction needs to be put forward in the context of a study design that can answer the research question.

Response: In the third paragraph of the discussion section we hypothesize and discuss multiple pathways that could link CHW interventions to public satisfaction with the health system. On page 16, in the strengths and limitations section, we added additional text to discuss the interpretation of satisfaction within the context of this study, which has a control group. We hope that this helps to clarify.

Reviewer #2: First of all, I must affirm that this is an excellent piece of work by any standards. It is novel in its quest to fill gap in knowledge in health systems research. The idea of unpacking how human resources for health interventions such as the CHW program impacts on health system satisfaction is timely as little is currently known in this area. Of commendation is that this research has not only succeeded in generating a body of evidence, but has also succeeded in raising pertinent questions for further research.

Response: Thank you, these comments are appreciated.

I submit that all the aspects of this study have demonstrated some degree of scientific rigor but for some certain elements of the methodology. These methodological considerations may have enfeebled the strength of evidence generated, even though some of these were already highlighted as limitations of the study.

First of all, the intervention group had more respondents than the control, about 3 times more. From this submission, it is unclear whether sampling weights were considered during analysis to equalize sample size distribution across the study groups. Non-equalization of sample size could have skewed findings towards the intervention group. More clarity on sample size considerations (such as weighting) between the study groups during data analysis could improve the validity of the findings.

Response: Thank you for making this important point, which has led us to conduct additional analyses to verify our findings. It is true that the intervention group had more respondents than the control group and this has to do with how the groups were randomly assigned (block
randomization to balance the population size in the ward) combined with targeting the same number of individuals in each ward for data collection. This is detailed in the methods. Upon reviewing the reviewer’s comments we created sample weights and re-ran a sensitivity analysis using these sample weights and the “svy” option in Stata. Our regression results do not change and we have thus maintained the unweighted results as these are easily interpretable by most policy makers.

Secondly, the CHW intervention is just one of several interventions that health system offers. There are other service delivery structures beyond CHW that make up the public-sector health system. Thus assessing the level of satisfaction with the public-sector health system based on experience with the CHW alone may not be sufficient for this purpose. As several other factors might have also been responsible for public satisfaction in the health system like availability of functional health facilities amongst others, this study has not sufficiently demonstrated whether public satisfaction as demonstrated from the study could have solely been as a result of the CHW intervention. The study could have done better by demonstrating the degree to which public satisfaction in a health system could be solely engendered by the CHW intervention or conditions and factors that must be in place before the CHW intervention could have such effects of increasing public satisfaction in a health system. Caution therefore needs to be exercised in taking up this evidence for decision making for policy formulation and programmatic interventions in using CHW program alone as a means to increase in public satisfaction in a health system.

Response: We agree with the reviewer that satisfaction is a complex construct and we do not think that one service will completely make up an individuals’ rating of satisfaction. Two aspects of our study design are advantageous here. First, we have a comparison group. The only difference between the two groups, which we know of, is the CHW intervention. Because the intervention is randomized, we can attribute with some degree of confidence, the differences between the two groups to differences due to the CHW intervention. The second advantage is that we ask both about satisfaction with the CHW program and overall satisfaction with the public system in Tanzania. Because we see indications of effect in both, we can be more confident that the differences in overall satisfaction were due to differences in satisfaction with the CHW program.

Lastly, the choice of CHW as data collectors is methodologically flawed. The CHWs are key stakeholders in the intervention and they might have regarded the study as an assessment of their performance on the program. Apart from the response bias that might have been induced if respondents knew the surveyors were CHWs, the surveyors might have also influenced the respondents to elicit satisfactory responses. CHW shouldn't have served as data collectors as the bias this could have introduced into the study may have undermined the validity and veracity of study findings.

Response: Response: We agree that using CHWs to survey women was not an ideal choice. The reviewer has identified two valid concerns for bias. In the first, it is a concern that there would be some social desirability bias. While this would likely occur in both intervention and control areas, thus making the difference between the two (which is the main statistic of interest in this paper) still valid, it is possible that it would occur more in the intervention areas. We have made
sure to clearly outline this potential source of bias in the limitation section. The second form of bias would be if there is some form of interviewer bias where the CHWs biased respondents, either intentionally or subconsciously, to report more favorably on them in intervention areas. The study team made efforts to prevent this form of bias through training of data collectors and through the use of structured surveys that did not have leading questions. We have addressed this in the limitation section with an additional sentence.

While this study is recommended for acceptance, it is suggested that the authors exercise caution in the interpretation of the study findings.

Response: Thank you for recommending the study for acceptance. We have made edits to the discussion and conclusion section that we feel have demonstrated further caution in interpreting our findings. We have also further added to and edited our limitations section.