Author’s response to reviews

Title: For more than money: willingness of health professionals to stay in remote Senegal

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Dear Editors of BMC Human Resources for Health,

Thank you very much for giving us the opportunity to revise our paper. We have uploaded the revised manuscript, tables and figures through your online submission site and the revisions are highlighted in blue. Also, please find following our response to the reviewers’ comments. We hope that the revised manuscript fully addresses the reviewers’ comments. We are grateful to the external reviewers, who provided us with very helpful comments and suggestions to improve our paper.

Please let us know if there is anything in the revised paper and/or our response to the reviewers’ comments that requires further clarification.
Yours sincerely,

Ayako Honda

Reviewer 1

1. Authors did not number their manuscript which is a very basic and fundamental mistake to make at this stage of ones academic life. It makes reviews unnecessarily cumbersome.

We have added line numbers to the manuscript, as suggested.

2. Lines 19-24, page 4: Authors should properly situate this "sweeping" statement since it is also increasingly becoming common knowledge that the world is getting urbanized and more cosmopolitan in nature. Many countries in Africa are getting rural-urban drifts where rural folks are migrating unabated to urban areas. Authors should emphasize more on the Senegalese situation and how this reflects distribution of health workforce.

Thank you for pointing out this important issue.

While we are aware that many people in Sub-Saharan Africa are moving from rural to urban locations, the rural population is still larger than the urban population. In Senegal, the balance has not dramatically changed over the last decade. Consequently, while it is important to consider urban migration and its implication for the distribution of healthcare professionals, inequitable distribution of healthcare professionals is still an important issue to investigate.

We have updated the statistical information in the paper and added further details on the rural-urban mix in Senegal to the relevant section.
3. Lines 7 (page 3), 37 (page 4): Authors should consider changing the use of the term "maldistribution", not sure whether it is proper formal English Language. Perhaps "in-equitable" might be more appropriate

Maldistribution” is defined as: (https://www.merriam-webster.com/dictionary/maldistribution).

We have however changed the word maldistribution to inequitable distribution where relevant.

4. Line 26, page 4-5: Subject verb agreement challenge, sentence should read "…Senegalese Health Ministry has made…”

The word “have” has been changed to “has”.

5. Line 38 (page 4-5): Sentence should read "…Human Resources Department …has a working definition…".

The word “have” has been changed to “has”.

6. Lines 9-17 (page 6): Authors should please provide relevant citation for the claim that WTS is a new methodology for DCE in LMICs Methodology

We have checked the literature that reports on the use of CEs to determine the job preferences of health workers in LMICs and confirm that this is the first study that has used WTS.

7. Line 38: The term "D-efficient experimental designing technique" should be briefly explained for novice readers in the area.

We have revised the text as follows:
‘An experimental design technique known as D-efficient design, which identifies a subset of tasks to minimise standard errors (SEs), was employed to obtain a more manageable number of choice tasks. Based on the assumption of null interaction effects and using non-informative priors, the approach generated 15 choice tasks for physicians and 16 choice tasks for non-physicians.’

8. Authors did not expatiate on the validity and reliability of the proposed research instruments. Supporting literature on the DCE needs to be expanded to justify its use in the current study.

The methods section now includes justification for the use of the method based on a literature review.

9. Line 16: Authors should take out the word "was" and only maintain "comprised"

We have removed “was,” as suggested.

10. Line 22: Contradiction with earlier statements in the Methodology section where we are made to understand the non-physician respondents were mainly nurses and midwives, but it appears there are other professional cadres (n=11) who also need to be described.

The description of non-physician respondents has been provided both in the Method and Result sections.

11. The entire discussion section needs more work in terms of comparing the findings with relevant local and international empirical studies. It appears the authors merely presented their findings again in the Discussion section of the paper without sufficiently demonstrating their findings deviation or otherwise from the existing literature.

Thank you for the comments. The entire Discussion has been rewritten, and is hopefully more interesting.
As part of this complete rewrite, we no longer simply present our results, but we discuss them, making reference to other relevant literature.

12. The discussion is scanty and should be properly explained in the light of the findings. Relevance of the findings health policy and practice is nearly absent and it is not exactly clear what the Health Ministry in Senegal in other developing countries in SSA should do with these observations.

See above – the Discussion has been revised to further elaborate on the Senegal context and policy developments made in light of our study.

13. Authors should consider the following suggested literature also looking at staff motivation through innovative approaches at the community level since the central governments (with their limited resources) cannot already find immediate solutions to these human resource challenges within the health sector.

Thank you for your suggestion. We have cited one of the references in the revised Discussion, supporting the assertion that a community-level approach is necessary when government cannot immediately address human resource issues due to a lack of resource capacity.

Reviewer 2

1. In the background section where they say DCE has been used widely, there is Ref#11. I think the authors should list a few more recent applications of DCE to HRH problems.

Thank you for the suggestion. In the Discussion, we have discussed our findings in comparison with existing studies.
2. In the methods, the authors say only the most important factors were included in the DCE. Can you say how you decided on the #? And was it a Delphi or a less formal process to narrow down?

Thank you for this very important comment. In light of it we have rewritten the Methods section, giving more weight to how our attributes were derived from the qualitative work.

More specifically, we applied BWS (Case 1) to determine the relative importance of factor identified in the qualitative investigation. Our study is the only one, to our knowledge, that uses BWS to reduce the total number of possible attributes to a ‘manageable level’. We have added further explanation on the use of BWS in both the Methods and Discussion. We highlight that this is a novel contribution of our paper.

Reviewer 3

1. Despite the authors' claim, I am not convinced that this paper adds much to the literature on DCEs applied to rural retention issues. The findings are very similar to other studies. And the methodological approach (which is sound BTW) is not particularly innovative. For instance, an unforced response approach is rather common.

We undertook a review of published CE studies in LMICs and identified that the following areas of our study are novel from a methodological perspective:

(1) use of a two-stage approach to develop attributes (i.e. qualitative work followed by BWS (Case 1)), see above, response to Reviewer 2;

(2) use of the number of years of assignment to estimate willingness to stay (WTS) for marginal improvements in attributes; and

(3) at an applied level, this is only the third study to use the CE methodology in Western Africa (Bocoum, Koné et al. 2014, Robyn, Shroff et al. 2015), and the first study to use a CE in Senegal.
Further, it is the first CE study in LMICs which used contract type for public sector health professionals as an attribute, although two studies have looked at the number of years a person has worked before being promoted to a permanent job (Miranda, Diez-Canseco et al. 2012, Rockers, Jaskiewicz et al. 2013). Given our finding that contract is the most important attribute, its inclusion is clearly important. We discuss this at length in the Discussion.

We also discuss our results in light of other study results.

2. I am a bit annoyed with the attribute regarding "permanent / temporary contract", and for two reasons. A first reason is related to the fact that providing a permanent contract is an extremely powerful offer, as anybody with some experience on HRH in Africa knows. This is confirmed by the authors' findings. Given that this attribute is present in this DCE, I will assume that all the sampled health workers are on a temporary contract basis (MoH, facility or local authority). Is it the case? Could the authors provide any info on that?

I am asking because it is rather unusual that health workers (especially for physicians) in Senegal to be in this legal situation. If all sampled health workers are with temporary contract, I wonder about the nationwide representativeness of this sample. If - conversely - some surveyed workers are already on a permanent contract, how this is factored in in the analysis? Again, we need more info on the existing situation of the sampled health workers (beyond the usual demographic variables).

A second problem I have with this attribute is that it may be highly preferred because being given a permanent contract allows the recipient to freely move to an urban area without risking losing her/his salary. In Senegal, payroll management for civil servants (i.e. those having a permanent contract) is still centralized, thus generating situations where salaries follow recipients (wherever they are actually posted). Consequently, a preference for a permanent contract may not be interpreted as a preference for "stability in employment" but rather as a preference for getting an opportunity to come back in an urban area. Maybe this could be checked by the authors through a quick qualitative survey.

Thank you for these comments, which we have reflected on and explored.
We have checked the current proportions of permanent and annually contracted health professionals in Senegal compared to our samples. This is now referred to in both our Methods and Results sections.

We note that while 48.9% of public sector health professionals, including doctors, nurses and midwives, were permanent government employees in 2016, the proportion for our sample was around 60%. This suggests that the shift from annual contracts to permanent contracts is unlikely to be associated with a return to urban posts.

In discussions with the Senegalese Ministry of Health (MoH), we can also confirm that current assignment policy does not link type of contract (permanent government employment or annual contract with MoH) to geographical assignment (although the locally hired must remain in the specific geographical areas / posts to which they were appointed).

Further, in Senegal, whilst payroll management of government workers is centralised (through the Ministry of Public Service), to move from one position to another, including from a rural to urban post, one needs to apply for the post, even if already permanently employed. Consequently, a permanent contract does not guarantee the free movement of healthcare professionals in the public sector job market. Under Plan Cobra, which commenced in 2006, annual contracts were used to deploy health workers to rural posts. However, annual contacts are no longer exclusively associated with rural postings and there are annually contracted health professionals in both urban and rural areas. The key difference in the job conditions for the two types of contracts are the length of job security and the provision of social security entitlements. Annually contracted healthcare professionals have a slightly higher base salary than those with permanent contracts.

We have added this detail and explanation to the Discussion.

3. Maybe, I missed a detail in the WTS, but I could not figure out how the WTS estimated among the physicians was 10.7 years while the maximum range for this attribute is 8 years.
We have tested for non-linearity in the years of assignment attribute, however, there was no improvement in the goodness of fit and, therefore, we have retained the standard linearity assumptions and treated the attribute as a continuous variable to estimate WTS.

It is not uncommon for marginal rate of substitution estimates (in our case, WTS) to be outside the range presented to respondents.

4. It would be worth analyzing further the overall impact of a rural origin of participants.

Thanks for this comment. Analysis of our data shows that the rural background of participants, either through time spent during childhood or during professional education, did not have clear impact on willingness to stay longer in rural posts. We have added some text on this to the Discussion.

Reviewer 4

1. My main concern is to suggest 1) a fuller literature review of what is already known about the factors affecting retention so that the findings can be better situated as themselves (as opposed to the methods used to reach them) novel.

Thanks for this suggestion, we have addressed it in two ways:

In the Methods we have included a reference that provides a systematic review of the use of CE to investigate job preferences in LMIC.

In our rewritten Discussion we now discuss our study findings in comparison with existing literature. Key here is that our study is the first to include contract as an attribute – and this was found to be the most important attribute. We also estimate willingness to stay in remote areas; this has not been previously estimated.
2. A fuller explanation of the situation of health workers in 'difficult' areas. In particular, I wanted to understand better the significance of permanent contracts that turn out to be so important. Are permanent contracts unavailable to any of the staff working in rural areas? That would seem to be the case for the responses to be so significant on this job attribute, and that begs a host of further questions about the nature of their current contracts and why they are temporary. In many settings, it is only temporary contracts that can be attached to specific duty stations and permanent contracts offer the opportunity to start negotiating a different job in an easier area – so permanent contracts may not be just about job security but be distorting the findings in an opaque way that might question the interpretation currently offered.

Thank you very much for the thoughtful comments.

We have added information on the current proportions of permanently employed health professionals in the country and the proportion in our samples to the Results and Discussion.

The information about differences in salary and other benefits for permanent and annually contracted employees has also been added to the Discussion to help clarify our interpretation of the results.

Given that approximately 60% of our samples, who were assigned in remote areas, are permanent government employees, geographical assignment and type of contract are unlikely to be linked.

We also discuss now at more length contracts in the rewritten Discussion.

3. I was also less than fully convinced about the cultural difficulties of using a WTP approach. When the issue of 'salaries' is sensitive, it usually does only mean those parts of the pay structure that are labelled 'salary' and not those that are labelled as bonus or allowance, that are generally much more transparent to everyone. I don't know that this is the case in Senegal but have found issues of allowances and the levels of allowances not sensitive in many other African settings (while salaries can be).
It seems to me a weakness of the study that the level of the rural allowance was not specified. Clearly a token rural allowance will have little impact but a large one, specified as such, might have made a much bigger difference. Hence much depends on what the respondents interpreted an unspecified allowance to amount to – my guess is that unspecified amounts might be assumed to be pretty token. I think some discussion of this among the study limitations would be appropriate.

We understand this point completely. However, when we were developing the attributes and levels for the study, we had planned to include specific salary scales to estimate the WTP for marginal improvements in attributes. However, after pilot testing the initial questionnaire, which used a salary attribute, the local interviewers reported that the respondents felt very uncomfortable answering questions about salaries (both the attribute and questions about salaries). The interviewers thought the questions affected the engagement of respondents with the choice task. The interviewers reported that asking specific monetary questions was considered culturally insensitive.

Following this feedback we discussed the matter with the Health Ministry. They indicated that they did not plan to increase salaries for those assigned to rural/remote areas and that they would prefer to explore policy options other than salary increases.

Given the cultural sensitivity and lack of policy relevance, we decided not to use monetary units as an attribute but to use years of assignment to estimate the marginal benefit of an improvement in attributes.

This also turns out to be a novel contribution of the paper.

A section on the unspecified level of allowance has been added as a limitation of the study in the revised Discussion section.

4. You used two different approaches to undertake sub-group analyses – you created separate groups for physicians and non-physicians but did sub-group analyses among those two groups for gender, age etc. Why was that hierarchy of subgroups considered appropriate? (i.e. did you think the differences between physicians and non-physicians would be larger than for gender for
example?) Again, a bit of further use of existing literature about what drives these differences would add depth to this discussion. For example, I was not surprised that among physicians, there was an extreme gender divide. Physicians will almost always be the 'in-charges' in any facility and there are serious real or perceived security concerns for women being 'in-charges'.

We used (slightly) different questionnaires for physicians and non-physicians (given their different work conditions). This, in turn, produced two data sets. We have made this point clearer in the paper.

5. The conclusion is that a very low proportion of staff will see out their contracts under current arrangements, but this seems somewhat confronted by the quite long periods (averaging 5.3 and 7.4 years for physicians and non-physicians respectively) that they have already served in these posts. Can you reconcile these observations? It seems to me that there has been some degree of instrumental responding – if the staff had alternatives such as those suggested, they would take them, but they don't. Can you really turn this into a likelihood of abandoning posts? And it does suggest a risk of bias that you discount when you discuss limitations. The extent to which these are people who have served long in difficult areas makes them people who cope or have learned to cope better and they must be more likely to stay even if their list of grievances is similar to the perceptions of those working in easier areas.

Thank you for this interesting observation and reflection. We have added discussion around this point to our new and revised Discussion.

6. On a more minor point, you claim that the method of asking about the choice between two jobs, and then the choice between the preferred and the current job enables the 3 jobs to be ranked but if I prefer Job B to both A and C (current), then you can't rank jobs A and C, can you?

Thank you for this point – we have hopefully made this clearer in the revised text in the Methods. More specifically, we have added:

‘Data from the CE allowed ranking of the three jobs (jobs A, B, and current), as well as estimation of the probability of a particular job being best (ranked first) or worst (ranked last).
We applied partial rank ordering when a respondent answered A (or B) in both questions, with only the best choice data used. This approach maximised the information obtained from the CE.’