Author's response to reviews

Title: Preferences for working in rural clinics among trainee health professionals in Uganda: a discrete choice experiment

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Author's response to reviews: see over
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Dear Dr. Ridde,

Thank you for your letter of April 9, 2012 requesting a revision and resubmission of our paper, “Health worker preferences for working in rural clinics in Uganda: a discrete choice experiment.” On behalf of my co-authors, I am resubmitting the revised paper, with highlighted edits. In this cover letter, I address the comments of the reviewers. We would like to thank the three reviewers for their careful and thorough reading of this paper and their very helpful input. We have addressed all the reviewers' comments and think that the revised manuscript is stronger as a result. Please let me know if you have further questions. We look forward to hearing from you.

Best regards,

Peter Rockers
Reviewer: ALEKSANDRA TORBICA

Reviewer's report:

The article represents interesting investigation on future health worker preferences for job opportunities in rural area in a low-income country. The study employs a method rooted in random utility theory and extensively used in health economics to explore different issues, discrete choice experiment (DCE). The idea is very interesting although not original since there are several studies in the literature that previously investigated the similar issue using the same methodological approach. The article is very well written, with a good structure and easy to follow. In certain parts, however, I had the feeling that it was too schematic and short so could benefit from further explanations to enhance clarity.

The following are few areas in which improvements should be made before final publication:

1) Background

There are two issues that should be included in the introductory part of the paper. The first concerns the bulk of DCE literature on health workers’ preferences that focuses on “developed” countries. Although this is not a main focus of the paper, it could benefit from briefly mentioning the existing evidence also in other parts of the world.

Response: we now include reference to the DCE literature in developed countries, and highlight the importance of the topic throughout the world (pgs. 3-4).

The second issue refers to the use and design of DCE method in developing countries. The issue has been largely debated (see for example Mangham et al. How to do (or not to do) . . . Designing a discrete choice experiment for application in a low-income country, Health Policy Planning 2009). The introduction (and discussion) section should include a few author’s reflections on the topic.

Response: thank you for this comment. We now include a short paragraph in the Conclusions section where we discuss the challenges of conducting DCEs in low-income countries and the resources available to practitioners to guide DCE work (pg. 13).

2) Methods

The method section is very clear and rather complete. There are, however, few important information missing that should be included in the paper:

- Sampling: it appears that no attempt was performed to calculate the sample size needed for estimating the main effects in the final model. There is great heterogeneity in the DCE literature regarding the sample size calculations, but recent methodological guidelines (eg. Lancsar and Louviere, Pharmacoeconomics 2008) require authors to be more explicit on
this point.

Response: we have added to the Methods section to address sample size considerations (pg. 5).

- Instrument: I think that some parts provided only in technical appendix should be reported in the main text. More specifically, more details should be provided on the focus groups and pilot testing (how many students from each group participated?). In addition, the use of fixed choice task should be explained in the main text. Finally, more information on validity test included in the instrument would be appropriate in the main text for completeness and clarity.

Response: thank you for this suggestion. We’ve added a brief description of the focus groups and pilot testing to the Methods section of manuscript (pg. 6). Further, we now discuss the validity tests that were conducted in the main manuscript (pgs. 7-8).

- DCE attributes: while all 6 attributes appear rather immediate, a bit more explanation should be given for “support from manager”. They way it is presented leaves a lot of room for interpretation also by the respondents, so more details on how this was defined would be welcome.

Response: we’ve now clarified the manager support attribute in the Methods section. This explanation was given to participants to ensure consistent interpretation of this attribute (pg. 6).

3) Results

Demographic sample characteristics reveal rather heterogeneous group of respondents in regards to, for example, life experience in rural areas (the portion of sample that lived in rural areas ranges from 62 for pharmacy to 91% laboratory students). This particular attribute could be important in explaining heterogeneity of preferences within and across sample groups. Would it be possible to further investigate this aspect, i.e. by including interaction effects in the final model?

Response: this is a good suggestion. We’ve run several interaction models to investigate the modifying effects of gender and rural experience (as well as university) on respondent preferences. We find that gender and rural experience have limited power in explaining heterogeneity of preferences. We do find a significant effect of university on the preferences of medical students. This analysis is now described in the manuscript (pgs. 9-10).

Authors show that large majority of the sample in all 4 groups would consider working in rural area, but from the previous question it appears that % of students likely/very likely to work in the rural area after graduation is very different across groups. Please explain better.
Response: we have added a discussion of this discrepancy to the first paragraph of the Discussion section (pgs. 10-11).

The way the results are presented (by group) doesn’t really allow the reader to compare preferences across groups.

Response: thank you for this comment. The reason we have not displayed the mixed logit estimates for each cadre next to each other is because direct comparison of model coefficients is impossible. The unit for these estimates is utiles, with different scaling factors for each model. We have included the willingness to pay estimates in Table 4 to allow for cross-cadre comparison. We have clarified this in the Results sections (pg. 8).

The result section should include some details provided in the technical appendix (validity checks).

Response: we’ve updated the Results section to include information on the results of validity tests (pg. 10).

4) Discussion

This section should be expanded by including some of the issues mentioned in the introduction (i.e. DCE literature on health workers preferences and risk/challenges of using this method in developing countries).

Response: as mentioned above, we now discuss in the Conclusions section how DCE may be used more generally for HRH policy in developing countries (pg. 13).

Among study limitations, the sample size issues should be mentioned and further explained.

Response: we’ve added mention of the sample size issue to the limitations discussion (pg. 12).

While hypothetical nature is known limitation of stated preference techniques, a DCE study could inform policy making in real world, as suggested by authors. A bit more reflections on generalizability of these findings in other settings should be provided.

Response: we’ve added to the Conclusions section to make the point that our findings are consistent with findings in other low-income countries. However, as we now mention, we do not want to overstate the generalizability of our findings (pg. 13).
Reviewer: Marjolein Dieleman

Reviewer's report:

Review article “Health worker preferences for working in rural clinics in Uganda: a discrete choice experiment”

Overall

The authors address a topic that remains important and describe in a clear and accessible way the DCE method. It is also interesting that the research provides DCE results for different cadres, because, as the authors rightfully state, health service delivery is team work. The study is systematically described and gives good insight in how the results could be used. The research question is therefore relevant for the area of HRH and is well defined. At the same time, some issues would need to be addressed to allow the reader to better place the study findings and the use of DCE in addressing HRH attraction and retention. Addressing these issues would improve contextualization of the research question and the understanding of the actual use of the research results.

Major compulsory revisions:

Title: the research addresses student preferences- and the title would need to be better aligned with the contents by replacing “health worker” by “last year students’ preferences”

Response: thank you for this suggestion. We’ve changed the title to more accurately describe respondents.

Background:

The authors explain the importance of using DCE for various cadres by writing that it is appropriate to consider the “inter-cadre effects”- although the explanation of the need to have a good skills mix in the team and therefore retention strategies for different cadres is clearly described, it is unclear how this relates to “inter-cadre effects”.

Response: we have removed reference to “inter-cadre effects” to avoid confusion.

It is important to write a short paragraph on current wages (in USD), incentive packages for attraction/retention, or generic incentive packages to better understand the results of the study. For instance: do health workers currently receive an allowance for housing, for rural areas? Is there a bonding program in place for different cadres- and if so: for how many years people have to work in a rural area? What is the current salary of the 4 cadres? A description of these issues will help to better understand the choices made by the students in the context of Uganda’s HRH policies.

Response: we have added a paragraph to the Background section to clarify the state of
health worker remuneration packages in Uganda at the time of the study (pg. 4).

Methodology:

Well explained- and the additional technical appendix is useful for more in-depth understanding of the methodology. Two questions: how is “manager support” explained in the interviews and how was the amount of 500,000 USh decided upon?

Response: we now provide clarification regarding the manager support variable in the Methods section. With regard to the 500,000 USh amount, this is simply the scale that was arbitrarily chosen for representing the estimates in the tables. The salary amounts that were included in the DCE instruments were different depending on cadre (see Table 1). For each cadre, the lowest salary level included in the instrument represented the actual salary for that cadre’s workers at the time of the study. Then, the incremental salary increases for each cadre were determined to be reasonable and policy relevant according to the MOH and FGD participants. We have provided clarification of this point in the Methods section (pg. 6).

It is also important to explain the location of the schools: Mulago in Kampala is very different from Mbarara and Jinja; Mbarara is in a rural area- does this lead to different choices among the students and were the students in these schools from rural areas? It is in this light also important to write that not having been able to include Gulu- and other rural schools might have created a bias.

Response: the point of urban versus rural training programs is important. Note, however, medical students from Gulu (the only relevant training program at the school at the time of the study) were included in the study. We’ve highlighted this variation in the Methods section (pg. 6).

Given school location- could the authors explain why all students from the same cadre, but from different school location were analysed together? It is important to know if they had similar choices or not. This also counts for gender: is there a reason why the authors did not compare if there were differences in preferences among male and female students?

Response: we have now run interaction models for each cadre to determine whether university or gender (as well as having lived in a rural area for at least one year, as per reviewer 1’s comment) significantly affected preferences. We describe this analysis and the results in the manuscript (pgs. 7 & 10).

Results

Currently the results present an interesting overview per cadre. In relation to the comment re sampling, it would be good to include results on analysis according to school location and for gender- or explain why this is not done (or that no differences were found).
Response: see response to comment above.

Discussion:

The authors discuss the results systematically and provide a good overview of the alternatives in strategies, in particular in relation to salary increase. Although this is interesting, it is also important to put these results in the current context of Uganda and prospects of use of the study findings by the Ministry: therefore, it is advised to add a paragraph on the feasibility of the choices- is it financially feasible if all cadres would receive 500,000 USh in addition to their salaries? How does that fit with the current wage bill and with the risks of other cadres expecting similar raises? And in relation to management support program: what is currently being done to improve health facility management? And have results of this study been discussed- what will happen next?

Response: as mentioned above, the 500,000 USh level does not represent a meaningful salary level, but rather a standard scale used to represent the results of the mixed logit model. We now clarify this point in the Results section (pgs. 8-9). We appreciate the suggestion of mentioning how these results are being used by the MOH in Uganda. We’ve now described how the Ministry is using these results (along with several other sources of information) to draft an HRH reform proposal (pg. 13).

Lastly: the authors indicate the limitations of DCE and the last sentences addressed the difference between stated choices and actual choice. Elsewhere DCE have already been conducted: in order to better understand the usefulness of DCE’s, the authors would need to add some of the results of other DCE studies: is there any proof of the actual implementation of preferred retention strategies in other countries? And with what results? And are there studies showing differences in opinions on preferred strategies comparing student results with results of studies among health workers in post?

Response: we have added to the limitations discussion to mention that the predictive power of DCE results like those we present have yet to be demonstrated. Further, we discuss some of the implications of this for policymaking. Finally, we mention that a few studies have been published that find significant differences in preferences between students and health workers (pgs. 12-13).

Minor essential revisions

Background- please add references to the last sentence of the first paragraph on p. 4. Add over what period of time and when the study was conducted. In discussion the authors write “management support program”, in the conclusions “management training” these two are not necessarily the same and need to be aligned.

Response: we’ve made the requested revisions.
Reviewer: Sitaporn Youngkong

Reviewer's report:

General comments:

The paper applied discrete choice experiments (DCE) to analyse stated preferences for job characteristics among medical, nursing, pharmacy, and laboratory students in training programs at the Universities in Uganda. The results of this study are intended to inform policy on designing particular effective strategies to address health worker shortages at health facilities in rural areas. However, I have some questions and comments as below;

Major compulsory revisions:

1. Regarding the DCE design, five of six attributes were the same for each of the 4 cadres. The final attribute was different for each cadre. Why did the authors come up with this design? Why did not you choose the same set of attributes for the design?

Response: we now clarify in the Methods section that the attributes for each cadre were determined independently, and the five overlapping attributes were not chosen to be included in all instruments, ex ante. The final attributes in each instrument were included because the preliminary work (e.g., focus groups, etc.) indicated that they were the relevant attributes for workers in each cadre (pg. 6).

2. The results of willingness to pay on page 8 do not seem to match with the data in Table 4. For example, in the text the authors stated that ‘Respondents in all groups had a consistently high willingness to trade salary for good quality health facility infrastructure and equipment and a supportive manager’. While Table 4 shows the different perspective among cadres and it is not right for every cadre that they were willing to trade salary with such the 2 attributes. It would be more understandable if the authors clarified how you come up with the above conclusion. Please check or clarify.

Response: thank you for making this point. We’ve modified our language so that our interpretations of the WTP estimates are less categorical. Further, we have added a clarification regarding the interpretation of willingness to pay estimates. In particular, we now point out the debate regarding the usefulness of exact willingness to pay estimates from DCEs (pg. 10).

3. Table 3 (of every panel) shows the results of mixed logit model of DCE data. It is not clear what the column of ‘Mean’ and ‘SD’ mean? Are those ‘Coefficient’ and ‘Standard error’? Please clarify both in the table and text.

Response: we’ve added a paragraph to the beginning of the Results section (pgs. 8-9) to clarify the meaning of the results presented in Tables 3 and 4.

4. The reporting of the results requires some improvement.
4.1 For Table 1, the description of attributes and levels should be presented along with their definitions. This may be more understandable for the readers.

*Response: we have revised Table 1 to provide the exact text that was presented to respondents.*

4.2 For Table 3, the results should include not only the estimates of model coefficients, but corresponding standard errors or confidence intervals along with associated p-values.

*Response: Table 3 provides standard errors for coefficients and standard deviation estimates. Further, significant p-values are indicated with stars, as explained in a note below each table. We have added an explanation of how to read Table 3 to the Results section (pgs. 8-9).*

5 DCE is becoming a valuable technique used for eliciting information on preferences. However, DCE has a number of limitations that need to be coped with additional approach, e.g. deliberative process, to make better understanding of the respondents’ preferences. For this manuscript, although respondents were told (at the beginning of the DCE administration) to consider all scenarios to be located in rural areas, as stated in the discussion section, we do not know whether or not they really realized this assumption when they made decisions from the DCE survey.

*Response: this is correct. We have added a sentence to the discussion of limitations to clarify that we have no way to confirm that respondents committed to considering all scenarios to be located in rural areas (pg. 13).*

Moreover, there would be some more reasons that attract and retain health workers working in rural areas. This can be captures in the process of deliberation or other additional approaches. Extensively discussion on this issue is appreciated.

*Response: this is an important point. We’ve added some discussion about the applicability of DCE for policymaking. We now clarify that DCE should be considered one source of information among many to guide HRH policymaking (pg. 13).*

Minor essential revisions:

1. The authors use ‘medical officers’ and ‘medical students’ interchangeable (this also happens with ‘nursing officers’ and ‘nursing students’). I understand that participants in this study were students who representing each health worker cadre. However, it seems to me that using ‘officer’ in this case does not refer to ‘professionals’.

*Response: thank you for pointing this out. We’ve carefully read the manuscript and made certain that we are careful with our use of these terms. As the reviewer suggests, we do need to be clear that the students were sampled because they were training to join the professional cadres of interest.*
Discretionary revisions:

1. The authors state in the first sentence of the 4th paragraph on page 3 that ‘One method for assessing the potential effectiveness of strategies for attracting and retaining health workers in underserved areas is the DCE’. I don’t think only the results analysed from DCE leading to the potential effective strategies for this issue. Please reconsider this point.

Response: as discussed above, we now clarify that DCE is one of several sources of information useful for HRH policymaking.

2. Table 3 for all 4 panels should be combined in a Table.

Response: as we discuss in response to a comment from reviewer 1, the reason we have not displayed the mixed logit estimates for each cadre next to each other is because comparing these estimates is impossible. The unit for these estimates is utiles, with different scaling factors for each model. We have included the willingness to pay estimates in Table 4 to allow for cross-cadre comparison. We have clarified this in the Results section (pgs. 8-9).

3. The authors use many names/terms inconsistency, e.g. attributes and cadres. This can make the readers confused.

Response: we have read the manuscript carefully and made every effort to make sure all terms are used consistently.