Reviewer's report

Title: Determinants of use of maternal health services in Nigeria - looking beyond individual and household factors

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Reviewer: Björn Ekman

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Determinants of use of maternal health services in Nigeria – looking beyond individual and household factors.

1. Is the question posed by the authors well defined? The research question and the exact focus of the study need to be strengthened; it is stated on page 22 that ‘a major focus of … the effect of community level factors’ this should come out as clearly earlier in the paper.

2. Are the methods appropriate and well described? Generally, the study has applied appropriate methods given the nature of the data (household survey data). However, it is far from convincing that the use of multilevel (ML) modeling provides a preferred approach as there are other ways of dealing with the problem that the authors discuss (nested data and potential (unobserved heterogeneity or endogeneity) bias). The nested data is fine to handle with ML, but it is not clear that also endogeneity is actually addressed by ML. The authors do not explicitly say that a problem with the data may be endogeniety, but from the discussion I gather that this is so. There are three sources of endogeneity: omitted variables, measurement error, and simultaneity. ML cannot in fact do much about either of these. To address endogeneity one would need either panel data or a valid instrumental variable (IV). The cross-sectional nature of the data makes the first impossible and the second difficult. I would suggest that the authors clarify what the ML approach actually contributes to and tone down the claim that ML can also address bias from unobserved heterogeneity.

3. Are the data sound? Also see previous point. The data are largely sound and the authors use the data in an acceptable manner. One would like to know something about the possible complex survey design of the data and the extent to which the authors have considered this in the estimations.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition? This not possible to say, but it would seem so given that they clearly identify the source of the data.

5. Are the discussion and conclusions well balanced and adequately supported by the data? Generally, yes, but there is an issue with the exact scope of the study; see above.

6. Are limitations of the work clearly stated? The fact that no information on the geographic distance has been used should come out earlier as this is known to
be an important factor.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Referencing is good.

8. Do the title and abstract accurately convey what has been found? Generally, yes.

9. Is the writing acceptable? The paper is very well written and contains only a small number of typos and grammatical errors (e.g. p. 21 missing p in word ‘revious’ in first para).

Please make your review as constructive and detailed as possible in your comments so that authors have the opportunity to overcome any serious deficiencies that you find and please also divide your comments into the following categories:

• Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore) None.

• Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. P. 6, second para. ‘health seeking behavior is poor’ use of the word poor here?

2. The use of the word ‘optimum’(noun) in ‘the optimum use of …’ Should that not be ‘optimal’ (adj.)? Granted, optimum can be an adjective.

3. Define LGA.

4. ‘Curvilinear’, is that a ‘non-linear’ relationship?

5. Age: did you also try using age and age2 to explore the ‘curvilinear’ nature of this indicator?

6. P. 11 – (Cronbach’s alpha: …) – explain meaning of this.

7. Suggest making a table of the information contained in sub-section Measurement (p. 9ff). The use of hypotheses for some of the variables is fine, but is not used consistently.

8. Also, under Results, the first para does not seem to stem from the ‘Analysis’ but could be put in the table of variables and descriptive statistics.


10. Please clarify the use of the ‘empty’ model (p. 12f). Does it contain an intercept? The discussion on why you use ML is not clear. Is it because of the nested nature of the data or because there is a random state effect – which is only part of the nested issue?

11. P. 23: Explain how this study has implications for evidence-based programming and policy.

12. What are really the advantages of using a composite outcome indicator? Are there risks involved in using such an outcome variable?
• Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

I have two major concerns. The first lies with the explanation for why ML modeling is used. I agree that the nested nature of the data is one reason for the use of ML, but as I have noted above, I don’t think it addresses the more serious problem of unobserved heterogeneity (which I think is what the authors are also concerned with). The differences between ML and Model 2 are not really large as the authors acknowledge, and it is not clear that the conclusion about the ‘underestimation’/’overestimation’ using Model 2 is really verified by comparing the results with the ML model. Generally, I would think both models generate largely the same more or less biased coefficients – but this is due to, among other things, omitted variables that the cross-sectional data cannot pick up. My suggestion is that the authors address the endogeneity problem and explain more clearly how this may affect the results. One option is of course to run the regressions and suffer the consequences, which is what most researchers actually do and also what I think this study does also with respect to the ML approach. Also, why is a random effects variable not used in Model 2?

My second concern is what we are to make of the ‘community level’ factors that the study actually sets out to address and thereby fill a perceived gap in the evidence base. As far as I can see, the authors’ key community variable is the ‘media saturation’ indicator, which turns out to be non-significant. Is this not at odds with your (implicit) hypothesis? This needs to be addressed also in the Discussion where it does not feature at all as far as I can see. Also: Which are your community-level indicators – please specify these clearly? Is urban/rural not also a community-level indicator?

I would recommend that the authors attempt to clarify the exact scope/focus of the paper and how the community-level issues are being addressed and what the results of the analysis with respect to this specific issue are.

Publication: Yes, if revisions are made.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
'I declare that I have no competing interests'