Author’s response to reviews

Title: Spatial variation in the use of reproductive health services over time: A decomposition analysis

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RESPONDS TO REVIEWERS COMMENTS

Reviewer reports:

Hilary Davies (Reviewer 1): Thank you for the opportunity to review this paper. The authors used the Demographic and Health Survey data from Ghana to examine the regional variation of utilization of reproductive health services between the years 1998 and 2014. It was an interesting and relevant paper, however my main concern is the way the article has been written as it distracts the reader from the focus of the paper. It would benefit from a more succinct writing style and verb tense consistency. Furthermore, there are also a few sentences that lack a correct citation.

Introduction

Overall this section could be shortened and structure improved. The sentences are somewhat long and verbose and could be made more succinct. A brief description about other studies and where they took place could also help strengthen the rationale for the study.

The introduction section has been reduced as advised by the reviewer. Section of the introduction has also been rewritten and an additional study added to the two already mentioned in the paper to strengthen the motivation for the paper (see pages 2 - 4)

Methods

Page 5, lines 4-29.

This section could be improved by expanding on the GDHS dataset (e.g how individuals were selected to complete the survey and why were men and women selected from different age ranges)
The data section has been expanded as requested and the selection of females aged 15 – 49 clarified (i.e. women who are in their reproductive age). In the case of men that is not clear in the main survey report but also likely to be related to reproduction. (see page 4 – 5)

Page 6, lines 19-59.

Although the authors have mentioned that they chose covariates based on previous literature, it would benefit to show how the model was built and if including all these covariates improved the model of best fit.

This has been clarified and the processes used in the model building also spelt out (please page 6 paragraph 2).

The authors also mentioned that they would use the following two variables NSCPHGW and NSCPHGT as proxies to capture availability and accessibility to health facilities. It would be better to give the cut-off values to suggest lower levels of access rather than saying "values closer to 0"

I agree with the reviewer that having specific cut-off values will be good for purposes of interpretation. The challenge however is that I hardly have any theoretical or empirical reference on which I will base the cut-off values. Thus, I have acknowledged in the paper that having the cut-off values will be good but am unable to do that because of a lack of either a theoretical or empirical reference point for doing that. Hence, I followed prior authors to rescale the variable to lie between a range and in this case 0 and 1.

Page 7, line 3.

The regional names in the text need to correspond with names in tables i.e the authors used "Greater Accra" in the text, but used "Capital" in the tables which made it confusing to interpret.

Capital has been changed to Greater Accra in the Tables to match with what is in the text

Page 7, line 34 to page 11 line 26.

The description of the econometric model is unnecessarily detailed. This section could be improved by focusing on the important aspects of the model.

I have worked on this section and as much as possible removed material that is not key to understanding the model used.

Results

I had some difficulty understanding the focus of this section. This authors could improve this section by emphasizing the significant values rather than repeating most of the values in the tables. This would allow the reader to concentrate on the important results and can look at the
tables for more clarity. Including number of participants as well as percentages would also clarify the results section.

I have removed those sentences related to variables that were not significant, especially that related to age

Page 13, line 12.

It is unclear what the authors mean by the term "dummy" and specifically "muslim dummy"

The term dummy is a standard term use in the health economics literature to refer to a binary variable. I have however changed it in the text to read being a Muslim

Figures

Figures 1 & 2 would benefit from a key to indicate which administrative/political regions are incorporated in the four ecological zones. The figure should be able to stand on its own and therefore a key for acronyms is necessary (eg. CHPS). The heading for Figure 2 is incorrect.

I have given a legend for the administrative regions making up the four ecological zones as well as the full meaning of CHPS. Please see Figure 1 and 2

All tables:

The authors need to indicate the table in the text when they are referring to a particular table. This occurred on page 11 line 35. The units of measurement are not clear in most of the tables and values should be reported using ASM criteria for reporting.

1. I have referred to this section and realized that Table to 2 which is the subject matter for that page was referred to (see line 3 paragraph 1 and line 1 paragraph 2 of page 11.

2. The issue of the unit of measurement has been clarified in the data section for the dependent variables. With respect to the independent variables, I will respectfully appreciate some clarification from the reviewer since am not at the moment clear about the unit of measurement being referred to. For example, age of the woman is measured in years and is already indicated in the data section. Birth order is indicated in Table 1 as the order of birth 1, 2, 3, 4+, women's education is indicated in the Table as No education, primary education, secondary education and tertiary education etc. So am not too clear the measurement being referred to

In order to allow the reader better understanding of the results, the authors could include the results from the regression analysis rather than only reporting the marginal effects. 95% CI should also be reported for marginal effects, rather than only reporting Standard Errors.
1. The main reason for transforming the results from log odds to marginal effect is due to the fact that the marginal effect helps the reader to understand how a unit change in an independent variable holding all the other controls constant will lead to a change in the dependent variable. This I believe is very relevant especially if policy makers want to know the likely effect of an investment in an intervention on an outcome.

2. Secondly adding the log odds to the marginal effect will make the paper heavier in terms of tables and pages. This notwithstanding, I have included the log odds results in an appendix to the paper.

Table 1: The table would benefit with the actual number and percentage in each age group, rather than just reporting the mean and sd.

I am not too clear with this comment. The age variable in the estimated model is continuous and not categorical and that is exactly what I have provided the summary statistics for in Table 1. I am not too sure if the reviewer wants me to add additional information on age by catering it and showing the numbers and percentages in each category. Should this be the case, I have added age categories with the numbers in each category and percentages as part of the legend (notes) in Table 1.

It is unclear why you have reported age-squared in all tables except for table 5.

In Table 5 the values are reported mainly at the summary level just as education, birth order and the other variables do not have the categories reported. In other words, the net effect of a variable is reported to make reading and interpretation easy. So, the variable for age is reporting the net contribution of age (i.e. age and age square) to the period changes in the dependent variables.

Discussion

This section could be improved by restructuring and using the following sub-sections:

Summary of results

Strengths and limitations of the study

Comparison to previous literature

Implications to policy and/or research

Conclusion

This section has been restructured as suggest by the reviewer
Lauren Brinkely-Rubinstein (Reviewer 2): This manuscript explores an important topic, but needs to be better written and more concise to be publishable.

Would reconsider using colloquial language or modifiers (grim, hardly, indeed, etc.).

I have noted the reviewer’s suggestions and corrected that especially where it has been used

Suggest focusing the intro on Ghana rather than all of Africa or even SSA.

I have worked on the introduction as suggested and reduced the length substantially

Some statements in the intro re: Ghana specific stats need citations.

Citations have been provided

Suggest an overall proofread for typos (grammar, spelling, etc.), which are abundant.

I have done proof reading as suggested

The last paragraph of the intro should be in the discussion (discussing the contribution of the current study).

The first paragraph of the discussion should include an overall summary of the main findings.

I have included this in the first paragraph of the discussion section

The discussion, in general is too long and should be presented more concisely.

I have restructured the discussion section and reduced the content