Author’s response to reviews

Title: ABSOLUTE INCOME IS A BETTER PREDICTOR OF COVERAGE BY SKILLED BIRTH ATTENDANCE THAN RELATIVE WEALTH QUINTILES IN A MULTICOUNTRY ANALYSIS: COMPARISON OF 100 LOW- AND MIDDLE-INCOME COUNTRIES

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Author’s response to reviews:

Title: Household income and coverage with skilled birth attendance: an analysis of cross-sectional surveys in 100 low and middle-income countries.

Manuscript ID (PRCH-D-17-00634)

Dear Editor,

We are grateful for the attention given to our manuscript. We appreciate the time and effort dedicated by the editor and by the two knowledgeable reviewers in evaluating our manuscript. We hope the revised version addresses your concerns and those of the reviewers.

Please find below a point by point response to the editor and reviewer’ comments.
Yours sincerely

Gary Joseph on behalf of the authors

Editor reports

Your manuscript "Household income and coverage with skilled birth attendance: an analysis of cross-sectional surveys in 100 low and middle-income countries." (PRCH-D-17-00634) has been assessed by our reviewers. They have raised a number of points which we believe would improve the manuscript and may allow a revised version to be published in BMC Pregnancy and Childbirth.

In particular they both highlighted the lack of thorough analysis around the comparison of wealth (particularly relative) and income. Too much of the analysis has been given for granted in its validity. Reviewer 2 in particular highlights an issue which I have as well in the lack of accounting for purchasing power in rural vs urban level within countries. Only if these key issues are addressed the paper will be considered for publication.

A large portion of the health equity analyses using survey data is based on wealth quintiles – as of 2008, the asset indices are corrected for urban/rural residence. Our focus in this paper was to show where the use of an absolute measure of income (rather than relative wealth) presents advantages. Thus, our focus on the comparison of wealth and income. Along our replies, we provide justification for our approach, and explain how the manuscript was revided. We have also carried out all the analysis for institutional delivery, based on the suggestion of Dr Rutstein, and included these results in a web appendix.

Reviewer reports:

Shea Rutstein, Ph.D. (Reviewer 1):
This manuscript is quite interesting and well done. However, there are two areas of concern that could be improved: definition of skilled birth attendance and discussion and conclusions about relative wealth versus "absolute" income.

1. The definition of SBA is too broad by including country-recognized cadres such as auxiliary midwife, auxiliary nurse, community health worker, etc. How were these additional SBAs determined for each country? I think place of delivery (hospital, health center, clinic, doctor's office) is a better measure for safe deliveries.

SBA is a SDG indicator, and this was the reason why we focused on it. As the reviewer is well aware, each country has its local policies in terms of birth attendance and local denominations of trained cadres are usually added to the standard doctor, nurse, midwife list of skilled professionals. Having said that, we agree that level of training will vary between these professionals and between countries. We, thus, repeated all the analysis using institutional delivery (defined as birth taking place in a hospital, health center, clinic or doctor’s office) and observed that such analyses did not change the main conclusions of the paper. We prefer to retain the focus of the paper on SBA coverage (given that it is a SDG indicator), and we provide the analyses for institutional delivery in the web appendix (see appendix Figures A1 to A5 and Table A1). We added text on the results for institutional deliveries in the abstract and throughout the manuscript (lines 35-36; 43-44; 86-87; 90; 159-160; 168-169; 184-185; 197-198; 200-202; 366-368). If the editor or the reviewer feel strongly that this is not appropriate, we can switch the results and include institutional deliveries in the body of the paper and SBA results in the appendix.

2. Relative wealth index versus absolute income:

   a. As described by the paper, absolute household income is determined using three sources of information: World Bank estimates of GDP, national income inequality, and wealth index quintiles for each survey. As I understand it, the WB estimate gives the national income per
household level adjusted for consumption share, population size, and mean number of persons per household; the national income inequality assigns a percentage of that income to household wealth quintiles, and the household wealth index assigns the quintile household income (consumption?) to all households determined to be in that quintile. If this procedure is correct, please spell it out in more detail. If not, then give more details about how the household income is calculated for each household.

The reviewer’s understanding given is correct, and we had used similar wording in the paper. We now provide more details to make our approach more clear to the reader in the Methods (lines 113-121) and in the Discussion (lines 235-241), where we describe attempts by different authors to estimate absolute income from asset indices; we also added references to previous work on this topic.

b. Why are wealth quintiles used for assigning household income rather than single percentiles or individual wealth scores? Household size varies by wealth quintile (and score), can this variation be incorporated in the calculation rather than the overall mean number of household members? Should the number of adult equivalents be used instead of the number of members of all ages? Since the analysis is limited to households where a birth occurred in the three years prior to the survey, which is likely to be different by wealth quintile (i.e. fertility varies by quintile), shouldn't it also be taken into account in calculating the household income (consumption)?

Our approach can indeed generate percentile-specific predictions, and we tested these predictions as well as decile specific predictions in our previous work (cited below). The main challenge with smaller groups is that classification error using the wealth score likely becomes quite a bit larger: while we are fairly confident that the asset quintiles correlates highly with the income quintiles, we would expect lower correlations at the percentile level. There is, therefore, a tradeoff between a more graded income attribution and classification error in the use of the
wealth index to create deciles or finer divisions. Empirically, quintile-specific predictions appears to be roughly equivalent to finer predictions – given that quintiles are already made available in all DHS and widely used in equity analyses, we opted to stick to quintiles (For more details see: Fink G, Victora CG, Harttgen K, Vollmer S, Vidaletti LP, Barros AJ. Measuring socioeconomic inequalities with predicted absolute incomes rather than wealth quintiles: a comparative assessment using child stunting data from national surveys. Am J Public Health. 2017;107(4):550-5 http://ajph.aphapublications.org/doi/10.2105/AJPH.2017.303657). We now address this issue in the Discussion (lines 243-245).

c. The analysis compares a relative measure to an absolute. It is wrong to use a measure of economic status that is relative to a specific country at a specific time for cross-country and cross-temporal analyses. For example, the highest quintile in some countries may have the same economic situation as the lowest quintile in other countries (e.g. Malawi versus Armenia). The economic situation may have dramatically changed over time even within a single country (e.g. Peru and Zimbabwe).

We may have misinterpreted Dr Rutstein’s comments, because such comparisons of relative quintiles are very common in the literature, including articles co-authored by him. A relative measure of wealth is what it is: a relative measure. It is not wrong to compare how the poorest 20% in a country are doing over time, nor to compare the poorest 20% in two countries provided one understands clearly what the comparison means.

Having said this, we agree that having an absolute measure is advantageous in many situations, and this is why we are proposing its use and showing how it improves the understanding of such comparisons and trend analyses. With our proposed approach, we believe many future analyses will be able to overcome the problems pointed out by the reviewer, given that we have made quintile-specific income estimates are available for download in Dr Fink’s website.
d. Survey-specific intercepts are left out of the cross-country analyses. I believe including them would substantially affect the analyses by lowering the effect of absolute income and increasing the effects of the relative wealth quintiles. Why were they left out?

The main focus of our analysis is to understand the impact of income on health outcomes, for which we want to explore both within country and between country analyses. If we adjust for country level effects, we lose the ability to make the comparison that is most important for our point in terms of absolute income – that they carry exactly the country level income information without the need to adjust. Thus, the inclusion of survey fixed effects would not allow us to explore the between country variation.

e. There are other survey-only wealth measures suitable for cross-country and cross-temporal analyses, such as the Comparative Wealth Index (Rutstein, Shea O., and Sarah Staveteig. 2014. Making the Demographic and Health Surveys Wealth Index Comparable. DHS Methodological Reports No. 9. Rockville, Maryland, USA: ICF International) and the Harmonized Asset Index (Staveteig, Sarah and Lindsay Mallick. 2014. Intertemporal Comparisons of Wealth with DHS Data: A Harmonized Asset Index Approach. DHS Methodological Reports No. 15. Rockville, Maryland, USA: ICF International). Other comparative approaches include "Estimating the absolute wealth of households" (D. Hrushka, D. Gerkey & C. Hadley, Bull World Health Organ 2015;93:483-490), which uses the Gini coefficient for the overall distribution.

Thanks for these additional references which we have now incorporated in the Discussion (lines 235-241).

f. Model 5 of the analysis includes both relative wealth and absolute income for within country analysis. However, the absolute income also incorporates wealth quintile, which is included as a separate factor. What is the level of collinearity between the two factors? Is it too
high to include both and that is why there is basically no change in the R-squared from the single factor models?

Indeed, the correlation between the wealth quintiles and the absolute income by quintile is certainly high (over 0.9% in each country). But the point of model 5 in table 1 is to make clear that when we fit both variables in the same model, the variance explained is basically the same as in the previous models 3 and 4, where each variable is included separately. The standard errors in model 5 are larger than those in models 3 and 4, but the increase is not so large to suggest that model 5 is unstable and cannot be interpreted. We have added this explanation (lines 181-184).

g. The discussion and results sections imply that absolute income can predict better than relative wealth quintile. This conclusion is possibly true for cross-country results without survey-specific control but does not seem so for within country results. Moreover, the discussions imply that differences in income only affect the demand for safe births and ignores the effects of differences and improvements that national income has on the provision of services, which may be the reason for the very great dispersion of SBA coverage at the same income level seen in Figure 1.

We fully agree with the reviewer, and this is our interpretation of the results. We revised the discussion to make this view clearer (lines 246-252).

Andrew Channon (Reviewer 2): This paper presents an innovative method to assess the link between income and SBA coverage using a new method to measure income at the household level from aggregate level data. It indicates that income measured in this way is a better predictor of SBA coverage at the country level than the use of the standard wealth quintiles and links this nicely to policy/interventions in specific countries to explain the result.
The paper is partially convincing about the benefits of this method, although there are a number of other aspects which, if added, would make it more convincing. There are a number of other areas which need to be addressed too before publication, although many of these are minor. As with any paper there are a few typos, but generally the introduction and discussion were interesting and relevant.

Major Issues which should be addressed

1. An aspect that is not discussed is the purchasing power in different countries/locations within countries. It states on p4, line 97 that 'income… has an absolute value and can therefore be compared across as well as within surveys'. Although clearly it can be compared there needs to be some acknowledgement of this issue, possibly in the discussion. Obviously, the paper does use the 2011 purchase power parity adjusted international dollars, but that is at a country level and there may be variations within a country. The comparison may be affected by the relative proportions of people living in urban/rural areas or in different districts.

While our data account for differences in average purchasing power among countries and years, purchasing power does clearly vary within countries, which will result in some measurement error of our income variable – we have added this as a limitation to the discussion (lines 346-351).

2. P6 line 147 - the classification of the birth attendants as skilled/unskilled depends on who the country recognises as skilled - where is this information taken from and did this vary in different countries over time? Could some of the results be explained by changes in who is designated as skilled over time?

We reviewed our data and the definition of categories of SBA for each country, and observed that no large change in coverage have been observed over time within countries, even with
changes in which providers were considered as skilled. We expanded our discussion on this problem (lines 360-362). Please also see our response to Dr Rutstein and the new webappendix on institutional deliveries, showing very similar results to those observed for SBA coverage.

3. The main extra analysis needed is further comparisons with other wealth indices. The results compare the wealth quintiles against absolute income (for each quintile). This leaves out other comparisons which may be simpler to do than the absolute income. This is the reason that the response to the question 'Does the work include the necessary controls' on the review form is answered 'No'.

There are two further comparisons that should be tested:

a. Using the actual mean wealth index score for each quintile, as is often given in the DHS surveys. This will indicate whether there are larger gaps between some quintiles than others, in a similar way to absolute income. It would be very interesting to see if this improves the fit from the current model using quintiles (which assumes equal distance between each quintile) and how close this gets to the fit of the model with absolute income.

As suggested by the reviewer, we performed the analyses using the actual mean wealth index score for each quintile, both for SBA and institutional delivery coverage, and the results showed that the mean wealth index score is a worse predictor of SBA coverage than the relative wealth quintiles and absolute income. Such results were added as Model 2 and Model 5 in the section of results (table 1). Text was added to the Methods (lines 137-139), Results (lines 175-176) and Discussion (lines 251-252).
b. Similarly, to the above, much work was conducted by Rutstein and Staveteig (2013) and Smits and Steendijk (2015) on an international wealth index which takes account of the differences between countries. This should be another comparison to the two already conducted.

Reply: We thank Dr Channon for this suggestion. However, the focus of our analysis is very different from these authors, who did not attempt to ascribe a monetary value to the income of wealth quintiles. We now mention these approaches in the discussion (lines 235-241), but we think that a proper comparison between the different approaches is beyond the scope of the present paper, and prefer to address it in a future publication.

4. Table 1 - the parameters for the asset quintiles for Model 1 and Model 3 are the same, although the coefficients expressed as percent point are different. Is this correct?

We checked the coefficients and observed that they are not different (Model 1 and Model 3).

Minor Issues

P3 line 69 - 'requests the enhancement of country capacity' - please change the word 'requests' - it is a target/goal rather than a request.

The word has been changed as suggested by the reviewer.

Done (line 53)

P3 line 80-81 - rephrase the sentence as it doesn't make sense.
P3 paragraph starting in line 82 - this paragraph is interesting, but very short for what is a complex and debated issue. Suggest either expanding to cover some of this debate - about how income affects health - or to reduce the paragraph down to say simply that there is a link and many other papers have discussed this, with further references highlighted rather than just the one that is noted at the moment.

P3 line 87 and elsewhere - 'health surveys done in LMICs' - better to say 'conducted in LMICs’?

P7 line 166 - countries chosen on 'contrasting levels and patterns of inequality' - it looks like these were chosen based on income levels. Clarify.

Thanks for the suggestion, we now mention that these countries have different income levels as well as contrasting inequality patterns (lines 153-154).

P9 line 206 - the Ethiopian HHs in the top quintile do not look like they have the same income that Nigerian HHs in the intermediate or third quintile. From the graph it looks like they have an income slightly less than the second quintile in Nigeria.
We agree with the reviewer for this point and corrected the sentence (lines 209-210).

Discussion - the Figure numbers throughout are incorrect.

The Figure numbers were checked and are now correct.