Author’s response to reviews

Title: Estimating the Contribution of Disparities within Developing Countries to Global Inequalities in Under-Five Mortality

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Version: 2 Date: 9 February 2014

Author’s response to reviews: see over
Dear Dr. Popham,

We greatly appreciate your careful review of our manuscript. We have responded to the reviewer comments below, and provide copies of the manuscript with changes tracked and with all changed incorporated.

Thank you again for your consideration of this work.

Sincerely,
Agbessi Amouzou
AUTHORS’ RESPONSE:

Reviewer's report
Title: Estimating the Contribution of Disparities within Developing Countries to Global Inequalities in Under-Five Mortality

Version: 1 Date: 28 December 2013

Reviewer: Mohammad Hajizadeh

Reviewer's report:

Title: Estimation of the Contribution of Disparities within Developing Countries to Global Inequalities in Under-Five Mortality

Summary:

Using data from Demographic and Health Surveys (DHSs), conducted between 1995 and 2010 in 67 developing countries, the authors aimed to examine how much of the under-five mortality gap between developed and developing countries could be attributed to within-country disparities in developing nations. The finding suggested that disparities in under-five mortality within developing countries account for roughly half of the global gap between developed and developing countries. Although the paper is straightforward and easy to follow, some revisions and clarification are required to improve the manuscript.

Major Compulsory Revisions

1- Although the authors cited some relevant studies in the “Introduction” section of the paper, they did not provide a comprehensive review of the available studies on inequalities in under-five mortality within and between developing and developed countries.

RESPONSE: The reviewer is right. We are have carefully searched for published literature on health gap between developed and developing countries and were not able to find any. We think the reason for this lack may have to do with the fact that this inequality is obvious and tends to be only asserted than carefully analyzed. We have therefore focused our review on articles that highlight the importance of focusing on equity in health as a strategy for accelerating improvement in health.

2- The contribution of the paper to the current literature needs to be highlighted clearly. I mean is this a first attempt to measure the contribution of inequalities within developing countries to global disparities in under-five mortality? If so, it should be said in the introduction section of the manuscript.
RESPONSE: We thank the reviewer for the suggestion and have added the following to the introduction “Although several studies, such as the “Global Health 2035” Lancet Commission report, have recently drawn attention to health inequities, no known study has attempted to quantify the size of the inequity between developed and developing countries and the relative contribution of within developing countries’ inequality gap to the global inequality. Such studies are necessary to provide global evidence for health inequality reduction programming.”

3- For clarity, the “Method” section can be divided into two (data and statistical analysis) or three (data, measures and statistical analysis) subsections.

RESPONSE: We thank the reviewer for the suggestion. We have now split the method section into subsections of “Data” and “Analysis”.

4- In the “Method” section, it is not clear how the authors dealt with sampling weight provided in each DHS when they pooled 67 surveys. In other words, since the authors pooled DHSs, I was wondering if they applied the de-normalization of standard weights approach (as per the DHS Sampling and Household Listing Manual) in order to calculate an appropriate weight for each observation in the analysis.

RESPONSE: The sampling weight was combined together with an adjustment weight (the inverse proportion of country specific births of all births in the past ten years) in the pooled analysis. This ensures that the pooled mortality rate derived is equivalent to the weighted average of country specific mortality rates with weights represented by the proportion of births in each country among the total number of births in the ten years preceding the survey. The sampling weights provided by DHS were not de-normalized.

5- Although the authors mentioned that they used the World Bank’s World Development Indicators to obtain under five mortality rates for developed countries, it is not clear how many countries are classified as developed countries in the World Bank’s WDI.

RESPONSE: We agree that the concept of developing country can be confusing and actually the World Bank classifies countries by income level. In general developing countries will belong to the low and middle income group. We now provide in the supplemental material the list of countries classified as low and middle-income countries according to the World Bank.

6- The “Discussion” section is very short. The authors need to highlight the results of their study with other relevant work.

RESPONSE: The reviewer is right that the discussion may be expanded more. However, as we mentioned in the discussion, the analysis included in the paper, although very revealing and important, are only illustrative. In addition, there isn’t any study that has tackled the subject in similar way. For this first set of analysis, we
purposively left the discussion short, acknowledging the limitations of the analysis and avoiding much generalization.

7- The main limitation of the study is that countries were surveyed in different years. Nevertheless, there is no discussion on the possible effect of this limitation on the study findings.

RESPONSE: The reviewer is right. Our results represent average over the period covered by the surveys rather than just one specific point in time.

Additional Comments:
1. Is the question posed by the authors new and well defined?
Yes, the authors examined an interesting question in the paper.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?

Although, the methods used in the paper are well described, it should be helpful if the authors provide more information as to whether the authors used the DHS suggested procedure when they pooled surveys for the analysis.

RESPONSE: We have added additional clarification, especially on the weighting procedure, to the method section.

3. Is the data sound and well controlled? NA

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
For clarity, the “Method” section can be divided into or three subsections.

RESPONSE: We have divided the method section into two subsections: Data and analysis.

5. Are the discussion and conclusions well balanced and adequately supported by the data? The “Discussion” section needs to be expanded.

RESPONSE: Please see our response above to comment 6.

6. Are limitations of the work clearly stated? The main limitation of the study is that the DHSs were surveyed in different points of time. However, there is no discussion on this limitation in the “Discussion” section.

RESPONSE: We have now included a limitation paragraph in the discussion section to bring up this issue.
7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? The authors need to provide a comprehensive review of the current literature.

RESPONSE: Please see our response to comment 1 above.

8. Do the title and abstract accurately convey what has been found? The abstract is well written and informative.

9. Is the writing acceptable? Yes.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
Author's response:

Reviewer's report
Title: Estimating the Contribution of Disparities within Developing Countries to Global Inequalities in Under-Five Mortality

Version: 1 Date: 6 January 2014
Reviewer: Laura Dwyer-Lindgren

Reviewer's report:

Discretionary Revisions:
1) Though most readers are likely familiar with the Millennium Development Goals, it may be worth including a brief description where they are mentioned in the introduction.

RESPONSE: Thanks for the suggestion. We have now defined the Millennium Development Goal four in the introduction.

2) Along the same lines, I think it would be helpful to briefly describe what a wealth index is.

RESPONSE: We thank the reviewer for the suggestion. We indicated what the wealth index is and provided a reference for its full description. We included the following in the data section: “The wealth index, a composite measure of a household economic status, is derived from household assets and characteristics using principal component analysis. The index is widely used to split the household population into economic groups of different levels in order to analyze economic inequalities.”

3) In the methods section second paragraph when describing how the country datasets were combined I think it would be sufficient to keep just the sentence that says ‘The pooled U5MR obtained using these weights is a weighted average of country-specific under-five mortality rates, with weights representing the proportion of country births in the overall number of births’ and remove the three previous sentences. I found the first part of the description hard to follow, but the weighted average description is, I think, sufficient on its own.

RESPONSE: Thanks for the suggestion. For more clarity, we have revised the section as suggested. However based on other reviewer's suggestion, we provided further short details on the weighting process.

4) In the third paragraph of the results section table 2 is carefully explained. I think it might be unnecessary however to copy the numbers from the table into this paragraph (i.e. the number of births, deaths, the U5MR in developed countries)—in my opinion the table itself is a helpful and sufficient way to present
this information.

RESPONSE: Thanks for the suggestion. We have revised the text as suggested.

5) In the discussion section the authors are careful to acknowledge that the 10% threshold for ‘best-off’ is arbitrary, which is appropriate. I think the provided justification for that threshold (that it fits easily with our decimal system) is not very convincing, however. I would recommend removing this justification—I think it’s simply unnecessary given your sensitivity analysis and care in cautioning that any threshold is arbitrary.

RESPONSE: Thanks for the suggestion. This has been revised.

6) In the discussion section, I think the second limitation is largely unrelated to the purpose of the paper, which is to describe what proportion of the mortality gap is explained by within-country differentials, not to explain why these within-country differentials exist or how they may be modified. I don’t think it’s necessary to caution that that a sudden decline in under-5 mortality is unlikely because your paper doesn’t suggest that it is. What may be more appropriate is to caution that while you’re examining between- and within-country differentials in under-5 mortality you’re not making claims about what is causing those differentials.

RESPONSE: Thanks for the suggestion. However we think it is important to acknowledge the mortality changes may be preceded by social and economic changes and the exercise presented, holding constant mortality in the top best off group is illustrative and hypothetical. We acknowledge the reviewer’s comment and added additional sentences to express the fact that we are not making any claims about the causes or determinants of mortality changes.

Minor Essential Revisions:
1) U5MR should be clearly defined in the background section.

RESPONSE: We have defined the U5MR in the introduction.

2) What constitutes ‘developed’ and ‘developing’ should be clearly defined in the background section. Further, in the method section where the 67 DHS surveys selected are described it might be helpful to say what proportion of the ‘developing’ world’s under-5 population these 67 countries represent.

RESPONSE: Given we relied on the World Development Indicators, we refer to the World Bank classification of countries by income level. In general, developing countries would belong to the low and middle income countries. We provide in the supplemental materials the list of countries included in the low and middle income countries, based on the World Development Indicators 2012. We also include in the data section the percent
of total population covered by countries included in the analysis among all low and middle income countries.

3) In the results section ‘Table 1b’ is referenced but the provided table 1 does not appear to be split into a part A and part B. Similarly, in table 2 ‘box 1’ is referenced, but this seems to mean the last line of table 1, not a separate box.

RESPONSE: We apologize for this oversight. The table numbering the text has been corrected.

4) This is a minor point, but three different sources of country-level U5MR are referenced: IGME 2012 (citation 11 in the background section), IGME 2010 (citation 16 in the discussion), and the World Bank World Development Indicators (citation 15 in the methods). For consistency the same source should be used throughout, if possible. In particular, there shouldn’t be any reason to use more than one version of the IGME estimates.

RESPONSE: We have now harmonized the references. Only the 2012 IGME report is now referenced. We cited IGME’s report when we mentioned the total number of under-five deaths. However this source does not produce crude birth rates or population totals. We pulled these data from the WDI and no data were pulled from IGME’s estimates for the analysis.

Major Compulsory Revisions:

1) My main concern with this analysis is that it seems to confuse within-country inequalities and within-region inequalities. If the authors are interested particularly with within-country inequalities than all steps of the analysis should be carried out on the country-level. In particular, the assessment of U5MR for the ‘best-off’ should take place at the country-level: i.e., a country-specific ‘best-off’ U5MR should be used to calculate counterfactual deaths for the purposes of estimating the reduced mortality gap. The fact that it currently doesn’t has some counterintuitive implications, in particular, that the ‘best-off’ U5MR is higher than the U5MR reported by the entirety of some countries: there are 18 countries in supplemental table 1 where the U5MR in 2007 is less than 44.1 (the level calculated from the pooled data for the top decile). This means that the calculations for estimating the reduced mortality gap currently assumes that mortality actually increases in a substantial number of countries. Alternatively, if the authors are interested in considering within-region inequalities rather than within-country inequalities than the pooled ‘best-off’ U5MR makes sense, but it should be made from the ‘best-off’ overall in the developing region, not the ‘best-off’ from each country individually: what it means to be in the top decile will depend on what country you’re currently considering, and also the reference date of the survey since not all surveys considered occurred at the same time. To do this would require constructing wealth indices from the DHS that are comparable across survey, unlike the ones that are currently provided in the DHS datasets.
RESPONSE: The reviewer is making a very good point. However, as we mentioned in the analysis section, the pooled mortality rate for the top decile that we calculated was weighted such that it is equivalent to the weighted average of country specific mortality estimates. In other word, another alternative way of computing the pooled mortality rate for the top decline (as the reviewer rightly explains above) was to compute the rate for each country and then compute the weighted average across countries. We did not proceed this way because sample size at country level was not sufficient to isolate the top decline for mortality computation. However by weighting the pooled analysis with country specific births and the sampling weight (already provided by each dataset), the pooled rate obtained is similar to the country specific average rate.

With regard to comparison of the wealth index across countries, the reviewer is right that the index, as individual score, is not comparable across countries and there have been attempts to compute indexes that comparable across countries. However, by deriving the top decile (or quintile), we derived the top 10% best off (or 20% best) in each country, which expresses the relative wealth in each country and therefore comparable based on the objective of the paper. Our interest was to define to top best off in each country to assess the relative inequality within the country. This justifies the procedure used.

2) In the background the authors compare the ‘burden’ of under-5 deaths by comparing the absolute numbers of deaths in developed and developing countries. Similarly, in the conclusion the authors state that “Approximately six million more under-give deaths occur in developing countries than in developed countries every year.” This comparison is misleading, particularly as phrased in the conclusions section, given the size of the under-5 population in the developing versus the developed world. This comparison should be made with rates, not absolute numbers.

RESPONSE: The comparison that the reviewer is referring to focuses on quantifying the number of under-five deaths in each region without making any inference about the risk of death. While it is true that the size of the population of under-five is different in the two regions, the number of deaths is also an important public health indicator for decision making and is very illustrative of the burden of the phenomenon. In the introduction, we have also made the comparison using mortality rates but this comparison does not convey to quantity of deaths suffered by each region. We believe comparing the number of death is very important and should be kept in the paper.
3) The methods for analyzing the birth history data should be more explicit. In particular, when you say that the approach is ‘similar to the procedure implemented by DHS’ you should state explicitly in what ways it differs.

RESPONSE: We removed the terms “similar to the procedure implemented by DHS”. However it would be tedious to explain the life table procedure based on the synthetic cohort approach used. We provide a reference for readers interested in learning or implementing the approach.

4) The authors should acknowledge limitations imposed by the DHS data. Both the birth history data used to collect U5MR and the asset data used to construct the wealth indices are subject to error. Further, since the survey date varies and the world development indicators may take into account other data besides the DHS, there is sometimes a large difference between the U5MR from the DHS and the U5MR in 2007 from the world development indicators (for example, Brazil, Kazakhstan, Kyrgyzstan, and Niger), which draws into question the calculation of the ‘reduced mortality gap’ which relies on the comparison between these two sources.

RESPONSE: Thank you for the suggestion. We have acknowledged the limitation due to different dates of the surveys used. However with regards to the data from the WDI, total population, crude birth rates and U5MR taken from WDI were used to compute the adjustment weights and the total number of deaths for the reference period. Mortality rates obtained from the survey data were applied to the total number of deaths to derive the inequality gaps. The results obtained were not dependent of the total number of under-five deaths estimated from the WDI database.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

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