Author's response to reviews

Title: Benchmarking health system performance across districts in Zambia: a systematic analysis of levels and trends in key maternal and child health interventions from 1990 to 2010

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Author's response to reviews: see over
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Dear Editors of *BMC Medicine*,

On behalf of all the authors of our study, I’d like to thank you for considering the manuscript entitled “Benchmarking health system performance across districts in Zambia: a systematic analysis of levels and trends in key maternal and child health interventions from 1990 to 2010” for publication in *BMC Medicine*.

We appreciate the comments provided from both reviewers of this paper. The research team has taken the time to read the comments by each reviewer carefully, and we have responded to each point brought up by both reviewers and the editor (included below). We have also edited the original manuscript to address particular concerns that were raised, highlighting changes in the manuscript. The manuscript has benefited greatly from these reviews and edits, and we appreciate the reviewers bringing these issues to our attention.

We hope that we have adequately addressed both reviewers concerns and that *BMC Medicine* editorial staff will find this manuscript acceptable for publication. Please let us know if there are any further questions.

Sincerely,

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We appreciate the comments and feedback from our two reviewers. Please see our responses below:

Reviewer 1

Minor essential revisions

A new important result is introduced in the discussion: that performance was not highly correlated with average district socioeconomic status. This is an important finding, and is in contrast to other country experiences. I therefore suggest that this analysis should formally be introduced in the methods section, and the results shown in a figure or table in the results section.

Response: We agree that this is an important point, and we incorporated an explicit investigation of this relationship in the methods, results, and discussion sections.

Discretionary revisions

Page 1: First name of first author should be spelled out.

Response: We made this change.

Page 4. Choice of 17 indicators. Since the authors basically use and report on 12 priority indicators (discussed also on page 7), perhaps that should be the chosen number? Why did the composite coverage indicator use only 10 of them?

Response: Based on the reviewer’s comment, we clarified in the text that we identified 17 key indicators that are closely tied to child survival that could be estimated at the district level from the identified data sources, but that we report on 12 priority indicators in the main text.

Page 5: The choice of “underweight” as an indicator should be justified since many other studies use stunting as an indicator.

Response: We agree, and we added to the text that stunting and wasting are also common measures of childhood nutrition. We note that stunting reflects chronic under-nutrition, and wasting reflects acute under-nutrition. We selected underweight, because is representative of both chronic and acute under-nutrition and is the preferred World Health Organization measure of malnutrition.

Page 6: The phrase “Except the Netmark surveys…” comes before the Netmark Surveys are explained.

Response: To clarify, we added a reference to Table 1 in the text for further information about each data source.
Page 6: The assumption of 0.01% coverage for malaria interventions prior to 1997 cannot be correct, seems implausible, and should at least be justified.

Response: Based on the reviewer’s comment, we added a comment to the text that while there were isolated malaria control programs prior to 1997, for example, in the Copperbelt, there was no coordinated national effort for malaria control and the majority of the population was not covered by ITNs, IRS, or IPTp during this time period. We also supplied references to support these statements.

Page 6: “The fixed effects of this model included the bases for a natural SPLINE, …” This concept needs an explanation (or is it a typo?).

Response: We added to the text as short explanation of natural splines, a method of interpolation or smoothing using piecewise polynomials.

Page 12: Discussion of lower ITN ownership and use in Lusaka. I don’t have the contextual knowledge here, but could it be the case that the denominator, need, is lower in Lusaka than elsewhere?

Response: Based on the reviewers comment, we noted in the text that malaria transmission intensity is lower in Lusaka than many other parts of the country. It is likely that this may be related to lower ITN coverage in Lusaka, though our data do not allow us to infer causality.

Page 14: Typo – STGs should be SDGs.

Response: We corrected this error.

Page 14: Typo – Replace “the coverage several key” with “the coverage OF several key”

Response: We corrected this error.

Page 18: References. There are perhaps too many references; some of them may be excluded without losses for the reader. Reference 10 could be supplemented with the recent WHO UHC report “Making fair choices on the path to universal health coverage” (2014) that makes exactly the same recommendation.

Response: We thank the reviewer and replaced reference 10 as recommended. Regarding other references, there are indeed many. To our knowledge there has been no previous review of studies estimating health indicators in low-income countries at subnational geographic units. Therefore, we believe that the literature review in the background section, the main source of references in the paper, is a valuable contribution to the literature.

Supplementary figures: Perhaps one of the maps could be included in main manuscript. They provide easy to understand information.
Response: We agree and incorporated two illustrative maps in the main manuscript.

Reviewer 2

The methods are clear and fairly well described but more detail is needed. The authors have clearly described their analytic methods and that is good. What needs more clarity is on the actual data collection methods. For example, while the authors state that they identified “all available data sources with information on maternal and child health and socioeconomic factors” they do not tell us how they identified these sources. Is there a chance that some important data sources have been left out?

Response: We thank the reviewer for identifying this point, and we clarified in the text that we conducted several in-country meetings with major stakeholders in maternal and child health to identify all available data sources. We also met individually with many stakeholders to assure we identified all available data sources.

Considering that there are different data sources, it may be important to say something about each of these data sources and the methodological variability among them. Definitions of some indicators may need to be described. One such an indicator is Skilled Birth Attendance (SBA). Should we assume that all the data sources used the same definition of SBA?

Response: We clarified in the text that we conducted all indicator calculations according to the definitions in Table 1 and we assured that we used the same indicator definitions across sources. We calculated all indicator estimates using the original survey microdata (with the exception of the Netmark surveys), rather than relying on survey reports. This procedure allowed us to ensure consistent indicator definitions across sources.

The other example of a detail is the denominators used in coverage indicators. One denominator that has been problematic in Zambia is immunization coverage for various vaccines. When the population figures from Central Statistics are used some districts come up with coverages of way above 100%; so some districts use ‘head count’ population figures as their denominators. Was this taken into account when calculating coverages? Was the definition of coverage homogeneous across all the data sources?

Response: We have modified the text to indicate that all indicators were constructed according to the definitions in Table 1 and consistent definitions were used across all sources. We agree that “head count” measures of immunization coverage should be addressed explicitly. Based on the reviewer’s comments, we clarified in the text that we did not include immunization coverage estimates constructed by pairing administrative data on the number of doses administered with population figures, because, in contrast with survey-based estimates, such measures are often subject to significant numerator and denominator bias which are likely exacerbated at the district level.
The authors use a Composite coverage, and describe the method used to construct this composite, but in my view not in sufficient detail for replication. The authors also do not state what limitations this composite metric has. As Laurens Cherchye and others observe, while composite indicators are usually used for benchmarking countries performance, their use seems controversial with regard to the subjectivity that is attached to their construction (Laurens Cherchye & Wim Moesen & Nicky Rogge & Tom Van Puyenbroeck & Michaela Saisana & A. Saltelli & R. Liska & S. Tarantola, 2006. Creating Composite Indicators with DEA and Robustness Analysis: the case of the Technology Achievement. Public Economics Working Paper Series. http://ideas.repec.org/p/wpe/papers/ces0613.html). It is therefore necessary to provide some more detail on how the composite indicator was constructed.

Response: We agree, and we refined the methods section and clarified that for simplicity and ease of interpretation, we constructed composite coverage as the simple average of ten interventions (named on pages 8-9), with each intervention given equal weight.

Editor

Please also ensure that your revised manuscript conforms to the journal style (http://www.biomedcentral.com/info/ifora/medicine_journals). It is important that your files are correctly formatted.

Response: We thank the editor and have reviewed our manuscript carefully and made the necessary changes.