Reviewer’s report

Title: State of inequality in malaria intervention coverage in Sub-Saharan African countries

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Reviewer: Albert Kilian

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In this manuscript Galactionova et al. present the findings from a secondary analysis of 30 DHS/MIS data sets from African countries 2005-15 with respect to the equity of six indicators of malaria control interventions. The authors use an array of analytical techniques based on the standard, asset-based wealth index and quintiles used in these surveys including equity ratio, concentration index and excess change. Overall the topic of this research is relevant to the public health community and the analysis is well done. There are, however, a few issues that will need to be addressed, mainly referring to the indicator selection as well as the conclusion of the findings as they are currently presented.

More major issues:

1. Of the six indicators used to assess equity in malaria control three refer to vector control and the other three to case-management in children and prophylactic treatment in pregnancy. Obviously the degree of equity or inequity depends on the choice of indicators and for the ITN ownership and use indicators I see a problem. There are three indicators that are frequently used to assess ITN ownership: households with any ITN, households with one ITN for every two people (considered to be enough for all household members to use a net) and the population access to an ITN within the households (assuming nets are on average used by two people). The authors chose the indicator households with at least 1 ITN for every two people which is not only the lowest with respect to coverage (as households with some but not quite enough nets are excluded) but also the most inequitable as large households tend to not get enough nets and these are more likely to be in the poorer wealth quintiles. In general the indicator of any ITN in the household is much more pro-poor particularly in countries with a recent mass distribution campaign, and the access indicator is the best to assess "universal coverage" with ITN. I am not suggestion that the calculations are re-run with a different indicator as this is probably not possible, but the choice of indicators and the possible consequences for the results should be discussed in some detail.

A similar problem exists for the ITN use indicator. Although the findings presented in the manuscript suggest that ITN use tends to be more equitable than ownership (wealthier people are less likely to use ITN), the use indicator is not independent of ownership as having access to a net is a necessary condition to use. Hence the equity of ITN use in a situation with a strong pro-rich bias of ownership can only change to pro-poor within the limits of the underlying ownership inequity. This aspect needs to be addressed and discussed.
There are also some issues with the case-management indicators: why were three doses of IPT used rather than the more common "at least two" and why was malaria treatment not restricted to the "recommended" medicines containing ACT? In both cases this could have significantly altered the equity results and this should at least be discussed.

2. The conclusions in the abstract are poorly formulated and as a result appear not to adequately reflect what is presented in results and discussion. The first conclusion states that "... progress in malaria control is halted by historically high levels of inequity...". Do the authors really mean "progress in malaria control" (which is not really addressed in the results) or rather the equity in malaria interventions (which as discussed above may to a large part depend on what indicators are chosen)? And what is meant by "historically high levels of inequity"? It sounds like there never have been higher levels of inequity of malaria interventions in history... but data from previous periods is presented or discussed. Or do the authors refer to the previous data within the time series of some countries that they have presented? If so, it needs to be phrased more clearly.

The second conclusion (and the respective point in the discussion) states the lack of improvement in equity in service delivery to translate into equity of risk or burden of disease. But what is the rationale to assume that equity in access to services of prevention or treatment should result in equitable burden if the risk of transmission is much higher in areas where the poor live, independently of the service delivery. Such equity could only be expected once transmission levels have been brought down to near elimination.

3. Although in the conclusion the authors talk of the "programmatic implications", they never clearly state what in their opinion the programmatic implications of their findings are. Such a statement would strengthen the paper.

4. The authors use the term "inequality" only to describe a pro-rich gradient and refer to the inequality that favors the poor as "pro-poor". This is their choice, but at least it should be more clearly stated. I suggest that in the last sentence in the methods section the work inequality is put in quotation marks and the following is added (as alike): … while inequalities and changes that favour the poor are termed "pro-poor".

More minor issues:

5. In the beginning of the background section the shift in target groups is described as "control efforts" while actually this refers to one part of the vector control efforts, namely malaria prevention with ITN, but not IRS or other vector control interventions.

6. The y-axis scale in Figure 1 A is not legible. In Figure 2 axis titles are missing for the y-axis in A and x-axis in B.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.
Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
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No

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