Reviewer’s report

Title: From high to low malaria transmission in Zanzibar - challenges and opportunities to achieve elimination

Version: 0 Date: 30 Jul 2018

Reviewer: Larry Slutsker

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Comments on Zanzibar paper

General comments: Overall this is a well-written and useful paper which details the progress of malaria control along with intervention introduction over a substantial time period in Zanzibar. The authors represent a group that has been working in Zanzibar for a long time, and their collaboration and familiarity with the national malaria elimination program there is well-known. There are a number of clarifications and suggestions provided below which may help to improve this overall very good contribution.

Specific comments:

1. Abstract methods: The methods used are described only very superficially. Can the authors provide a bit more detail, such as providing data sources in general analytic approach.

2. Abstract results: Incidence of clinical malaria. - a little unclear what the comparison group is here. Higher incidence in >5 c/w with <5? Or higher incidence in >5 in 2015 c/w with earlier years? Also, >5 is a very broad category, would be more instructive (if possible?) to have some age category breakdown of highest risk stratum, though it is understood the data might not be available in that format.

3. Abstract results: "main reduction in malaria transmission" - as defined how? EIR? case incidence?

4. Abstract conclusions: If imported infections are to be mentioned in the conclusions, then some data to support this conclusion should be given in the results.

5. Page 3 Line 10: suggest - "is considered feasible - and indeed has been achieved - using ..."
6. Page 3 Line 17: Zanzibar is a valuable case study. I am not sure it should be called the first, as other moderate/high transmission areas (e.g. Zambia) are also being evaluated. Perhaps describe Zanzibar as an extremely useful or valuable case study.

7. Page 3 Line 50 - why were these specific districts selected?

8. Page 4 Line 20: would be useful to provide some description of interview data elements, including travel hx, and what lab tests were performed, and what was the basis of sample size for surveys. Also, some description of the care seeking/utilization of public health facilities would be helpful to persuade the reader that most care is sought in the public sector and that therefore the HF data are broadly representative.

9. Page 5 Line 4: could a table be provided that details the changes in surveillance (what was collected, how often, and QA measures) as these systems were implemented? It is not clear to those who do not know Zanzibar as well as the authors what these systems mean and how they evolved/changed over time.

10. Page 5 Line 16: how many of these ento sites were in the study districts?

11. Page 6: Line 36: At first, the IRS coverage figures look high, but these were once yearly sprayings in a context where there were two distinct transmission periods. Thus the indicator of "sprayed in the last year" makes sense as a program indicator (because that is what the program was doing. But as a coverage or protection indicator, it could be a bit misleading in terms of expected impact. It might be useful to point this out at some point in the discussion.

12. Page 6 Line 58: how were gametocytes detected? By slide examination, or molecular methods, or both?

13. Page 7 Line 1: there is no table that reports parasite density (PD), which is fine (it would not be needed). Any information on changes in PD can be presented/summarized in text). The prevalence by PCR and RDT can easily be combined into the same table.

14. Page 7 Line 3: see previous query on gametocyte quantification methods

15. Page 7 Line 7: see comment above. Combine PCR with RDT data in same table

16. Page 7 Line 15: this paragraph is somewhat repetitious of previous one. Not sure why it is necessary to present the combined data in a different section from where information on individual districts is presented.
17. Page 7 Line 24: these may be statistically significant differences, but one does wonder about whether from a malaria control perspective there is much difference in a prevalence of 1.4 vs 1.9%.

18. Page 7 Line 41: how are these PDs different from the PDs given at the top of the page? Is this just among PCR detectable, but not RDT or micro detectable?

19. Page 8 Line 28: the authors seem to be referencing figure 2a-c here, but these figures are TPRs not incidence figures.

20. Page 8 Line 40: it's a bit unclear whether the controls were matched, or stratified by age? sex? or by location? or this was accounted for in mv analysis?

21. Page 8 Line 47: since the table has an overall AOR that is different, might be good to report this in the text first. Are the M/F AORs reported here significantly different? Doesn't appear so?

22. Page 9 Line 7: it's a bit confusing to understand when we might have first seen the impact of ACTs in the child mortality data...table 1 says ACTs were introduced in 2003. The graph however looks like more like 2004. And the text here says in 2005 after introduction of ACTS. Also, it should be noted somewhere that the child mortality rate was decreasing before the implementation of any significant malaria control interventions; from 1998-2002, perhaps 1.4% down to 1.0%?..though the slope looks steeper with the advent of malaria control. The authors may wish to mention this in the discussion for a more balanced presentation.

23. Page 10 Lines 8-26: this paragraph is a bit hard to follow and the main message gets diluted. The main finding is a major reduction in HBR, indoor> outdoor. Would suggest maybe fewer numerical comparisons.

24. Page 11 Line 1: there were significant declines in child mortality. No data on "malaria mortality" as presented.

25. Page 11 Line 6: Zanzibar does provide very good will access to HF care. the background section should provide some data to illustrate this - such as health care seeking behavior, and average distance to facility. it helps to provide context to those who may not know this place that well.

26. Page 11 Line 18: see previous comment about IRS and 'effective coverage"
27. Page 12 Line 2: As well as unusually good access to health care.

28. Page 12 Line 38: was the question also asked about travel in other hh members? as infections cluster in hhs, it is possible that the index case represented an infection acquired by travel from other hh members (who may have been asymptomatic or cured)


30. Page 13 Line 50: yes, it is likely that the malaria interventions indeed impacted child mortality. As per previous comment though, for a balanced presentation, the ongoing decline in child mortality before scale up of malaria control should be mentioned.

31. Page 14 Line 16: although travel and importation are highlighted several times in the results and discussion, little is said about potential strategies to specifically address this challenge. Admittedly, this is not easy and ultimately will require mainland transmission reduction. However the authors do need to address this in their comments in the discussion, as well as any proposed strategies or tools.

32. Page 14 Line 33: misspelling of larviciding

33. Page 14 Lines 39-52: are there any suggested improvements in surveillance?

34. Page 22 Table 1: Very helpful. However, this does present some interventions as implemented and continuous (such as IRS) - which may indeed be the case. If however there were gaps in coverage though (i.e years when IRS was not delivered for example) it would be good to include notations on that.

**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.

Not applicable

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Yes
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