PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Trends in malaria prevalence and health related socioeconomic inequality in rural western Kenya: Results from repeated household malaria cross-sectional surveys from 2006–2013
AUTHORS	Were, Vincent; Buff, Ann; Desai, Meghna; Kariuki, Simon; Samuels, AM; Phillips-Howard, Penelope; ter Kuile, Feiko; Kachur, SP; Niessen, Louis

VERSION 1 – REVIEW

REVIEWER	Bernt Lindtjørn Centre for International Health, University of Bergen, Bergen, Norway
REVIEW RETURNED	06-Nov-2018

GENERAL COMMENTS	This paper is based on repeated cross-sectional surveys from one county in Western Kenya bordering Lake Victoria. The paper addresses an important issue. In the first paragraph of the introduction, the authors describe the usual description of the global malaria control efforts. However, they do not mention that malaria control efforts may have stalled during the last years, and their results show that the prevalence in this county remained fairly stable over a period eight years. I suggest that they write about that. The authors address an issue which has been thoroughly described before. For example, in a recent paper in Malaria Journal, the same authors address a similar objective based on a part of the data used in the current paper, to what they do in this paper. They conclude in the journal: «Inequalities in malaria infection and expenditures on potentially ineffective malaria medication between the poorest and less-poor households were evident in rural western Kenya. Findings highlight the benefits of using MCA to assess and monitor the health-equity impact of malaria prevention and control efforts at the microeconomic level» (1). The aim of the current paper is to evaluate if eight years of the repeated surveys in the same county would show socio-economic health inequalities. The authors describe that using different methods to describe socio-economic inequalities would improve such an analysis, and also using repeated surveys would add evidence to what they already have described. However, their description of what the knowledge gaps are needs to be expanded and better described.

	The authors did eight cross-sectional surveys in the same county. I suspect that some households where surveyed more than once. Unfortunately, the authors do not describe in detail how they analysed these cross-sectional results, also accounting for repeated measurements and varying sample sized for different years. I suggest that the authors write more about the knowledge gaps that they wish to address. My understanding is that the authors state that multiple correspondent analysis of household assets gives a better classification of socio-economic groups, and the authors should present some data to show that this type of classification is better than the standard principal component analysis tools used in many other papers. Furthermore, the paper should better describe the dependency of the data and how they analysed such repeated cross-sectional surveys. The current paper and the paper the authors published in Malaria Journal in 2018 (1) are similar, and the authors need to justify what new knowledge the submitted paper would add. In the discussion, the authors mentioned that although some of the results are statistically significant due to a large sample size, the differences between the groups are relatively small. The authors need to discuss in depth the difference between statistically significant differences and differences that would be relevant for policy. References 1. Were V, Buff AM, Desai M, Kariuki S, Samuels A, Ter Kuile FO, et al. Socioeconomic health inequality in malaria indicators in rural western Kenya: evidence from a household malaria survey on burden and care-seeking behaviour. Malar J. 2018;17(1):166.

REVIEWER	Rashid A Khatib Ifakara Health Institute Dar es Salaam Tanzania I worked with Dr Kachur, SP in a CDC - IHI collaborative malaria program in Tanzania from 2001 - 2007 and we have co-authored several papers
REVIEW RETURNED	29-Nov-2018

GENERAL COMMENTS	General observations: The manuscript has been clearly written and has shared the findings that could be interesting for readers wishing to understand trends in malaria in Africa. However, the findings have been generated from surveys that were conducted five years ago. Given the frequent changes in malaria dynamics, I do not think that the information contained herein will be of a big use to malaria stakeholders. Real time information is crucial for effective planning and evaluation of policies, planning, funding, implementation and evaluations. The authors have used old references to explain the burden and distribution of malaria, eg World Malaria Report of 2016 when the 2018 report has been issued. The authors would have reported the findings of their studies the way back in 2014 or 2015.

	Title: The title should read “Trend in malaria prevalence and the related socio-economic inequality in rural western Kenya” Objective: It should be “The objective of this analysis was to examine malaria parasite prevalence and related socioeconomic inequalities in malaria indicators from 2006 to 2013 during a period of intensification of malaria control interventions in Siaya County, western Kenya” Methods: Data were analyzed from eight independent annual cross-sectional surveys from a combined sample of 19, 315 individuals selected from 7,253 households. ….. Background: The authors have used outdated references to describe epidemiology of malaria in the study. They should use the current references Results: Characteristics of study participants should be moved to Methods section The authors need to review their analysis that compares parasite prevalence and socio-economic status. The prevalence ration they have presented does not align with the proportions between the poor and the less poor. The tables should be reviewed to demonstrate sample size for each sub-population category presented in the tables. They should be shown in each category column in the tables. The authors should replace the drugs’ brand names with generic names Table 2 is empty. It should be deleted if does not report any information Table 3 does not have column headings, so it will be difficult for the readers to follow them. Discussion The study has reported the obscured variation in many outcomes that it has shared, eg no variation in ITN use between the poor and less poor population groups. Why was this case? Has there been any intervention that was implemented to address in equality in interventions coverage. What was it, where was it implemented and when? What is the likely development now? Despite the lack of variations in malaria control interventions coverage between socio-economic groups from the study, the authors have reported unequal distribution of malaria parasitaemia favoring the poorer population, the manuscript needs to raise the discussion on this.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Bernt Lindtjørn
Institution and Country: Centre for International Health, University of Bergen, Bergen, Norway Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below This paper is based on repeated cross-sectional surveys from one county in Western Kenya bordering Lake Victoria. The paper addresses an important issue.

In the first paragraph of the introduction, the authors describe the usual description of the global malaria control efforts. However, they do not mention that malaria control efforts may have stalled during the last years, and their results show that the prevalence in this county remained fairly stable over a period eight years. I suggest that they write about that.
Response
We have now included statement in the introduction stating as follows: WHO reports also revealed there had been a stagnation in progress in reducing burden between 2015 and 2017. We have also stated that
In Western Kenya, routine and unpublished data had showed that the prevalence of malaria remained fairly stable since 2006 despite intensified control efforts during the study periods.

The authors address an issue which has been thoroughly described before. For example, in a recent paper in Malaria Journal, the same authors address a similar objective based on a part of the data used in the current paper, to what they do in this paper. They conclude in the journal: «Inequalities in malaria infection and expenditures on potentially ineffective malaria medication between the poorest and less-poor households were evident in rural western Kenya. Findings highlight the benefits of using MCA to assess and monitor the health-equity impact of malaria prevention and control efforts at the microeconomic level» (1).

The aim of the current paper is to evaluate if eight years of the repeated surveys in the same county would show socio-economic health inequalities. The authors describe that using different methods to describe socio-economic inequalities would improve such an analysis, and also using repeated surveys would add evidence to what they already have described. However, their description of what the knowledge gaps are needs to be expanded and better described.

We have now improved on the language of know gap that his paper provides thus;

1. We have stated that
Trends in malaria burden and socioeconomic inequalities between the poor and wealthier individuals has not been published in endemic area of western Kenya over time, yet socioeconomic inequalities are known barriers to health utilization and control efforts

2. We have also stated that
Health inequality and equity data on malaria indicators are often collected but not analysed from an economic or equity perspective. Yet, such data and analyses are important for monitoring health inequalities and assessing the impact of malaria control interventions at the microeconomic level
3. WHO reports still reveal stagnation in reducing the prevalence of malaria globally and because we collected these data over year, we are justified to look at the last 8 years and provide evidence which will inform malaria programming in future. We have looked at this data from economic perspectives i.e. the equity impact over time to supplement epidemiological insights

The authors did eight cross-sectional surveys in the same county. I suspect that some households where surveyed more than once. Unfortunately, the authors do not describe in detail how they analysed these cross-sectional results, also accounting for repeated measurements and varying sample sized for different years.

Response: We have now stated the detailed analysis of the 8 years of cross sectional surveys as follows:
The eight cross sectional surveys were first analyzed independently and then as pooled data. The key variables were identified for each year and then appended to each other to form a large dataset. Considering that more one personal were selected in households, the analysis have considered clustering. Because these were data taken from different independent samples of the populations

over time, they are only used to assess population and group changes and not individual changes over time. Hence we did not include repeated measures analysis because we did not follow individuals. Trends analysis was conducted using Cochrane trend test

I suggest that the authors write more about the knowledge gaps that they wish to address

This is stated above on the first comment – the distributional trend - and is included in out paper summary and introduction sections:
My understanding is that the authors state that multiple correspondent analysis of household assets gives a better classification of socio-economic groups, and the authors should present some data to show that this type of classification is better than the standard principal component analysis tools used in many other papers.
Response: Yes indeed MCA gives better classification of households compared to PCA through data comparisons. This has been extensively discussed in previous publication (Were et al. 2018). In that paper, it was established however that the correlation between the two are significant implying that such analyses would not be different from the current analyses. We have therefore put less emphasis on this
Furthermore, the paper should better describe the dependency of the data and how they analysed such repeated cross-sectional surveys. The current paper and the paper the authors published in Malaria Journal in 2018 (1) are similar, and the authors need to justify what new knowledge the submitted paper would add.
Response. These datasets are valid and dependable. The data presented in these analyses are out of our protocols approved by local IRBs for each of the years and tools used were derived from national malaria indicators. The treatment for malaria and outcomes were as per the national treatment guidelines. These data were collected using mobile applications with strong validation checks.
Our paper (were et al 2018), used a single survey and was unable to determine trends over time. Historically when interventions are rolled out, there is need to monitor uptake and coverage. The international development goals also aim to monitor progress in reducing inequalities and such data are lacking. This analysis now includes trend analysis and pooled analysis which is different from the published paper and contributes to the SGDs goals of reducing inequities and inequalities between population subgroups in terms of health outcomes

In the discussion, the authors mentioned that although some of the results are statistically significant due to a large sample size, the differences between the groups are relatively small. The authors need to discuss in depth the difference between statistically significant differences and differences that would be relevant for policy.
We agree. We have further improved on our discussion by specifically discussing ITN use. The fact that in the pooled data we observed statistically significant result, the gap in equity was very small and we can confidently say the small gap implies there has been progress in reduction inequities. This may be due to massive distribution of bednets which increase the probability of usage. For policy such small gap in proportion of ITN use may denote progress in reducing inequalities despite statistically significance result.

References

1.Were V, Buff AM, Desai M, Kariuki S, Samuels A, Ter Kuile FO, et al. Socioeconomic health inequality in malaria indicators in rural western Kenya: evidence from a household malaria survey on burden and care-seeking behaviour. Malar J. 2018;17(1):166.

Reviewer: 2
Reviewer Name: Rashid A Khatib
Institution and Country: Ifakara Health Institute, Dar es Salaam, Tanzania Please state any competing interests or state 'None declared': I worked with Dr Kachur, SP in a CDC - IHI collaborative malaria program in Tanzania from 2001 - 2007 and we have co-authored several papers

Please leave your comments for the authors below My comments are detailed in the attached file

General observations: The manuscript has been clearly written and has shared the findings that could be interesting for readers wishing to understand trends in malaria in Africa. However, the findings have been generated from surveys that were conducted five years ago. Given the frequent changes in malaria dynamics, I do not think that the information contained herein will be of a big use to malaria stakeholders. Real time information is crucial for effective planning and evaluation of policies, planning, funding, implementation and evaluations. The authors have used old references to explain the burden and distribution of malaria, eg World Malaria Report of 2016 when the 2018report has been issued. The authors would have reported the findings of their studies the way back in 2014 or 2015.

Response
We appreciate the comments from this reviewer. We have given a justification of the paper in relation to the equity impact of control programmes. The relevance for present-day control is still there, yet one needs to study the present dynamics of control efforts and malaria prevalence
The WHO reports still reveal stagnation in reducing the prevalence of malaria globally and because we collected these data over the years, we are justified to look at the last 8 years and provide evidence which will inform malaria programming in future. Assessing historical trends in socioeconomic inequalities provides evidence of whether intensified malaria control interventions such as distribution of ITNs done over the years has had impact since new policies we put in place in Kenya in 2006. Publishing this data will provide evidence and methods of trends analysis for future research and hence timely. In the malaria endemic area such published data on trend are limited. We have looked at this data from economic perspectives to supplement distributional impact to epidemiological observations and dynamcs

Title: The title should read "Trends in malaria prevalence and the related socioeconomic inequality in rural western Kenya"
Response: We have considered this and changed the title

Objective: It should be "The objective of this analysis was to examine malaria parasite prevalence and related socioeconomic inequalities in malaria indicators from 2006 to 2013 during a period of intensification of malaria control interventions in Siaya County, western Kenya"

Response: We have considered this and changed the objective

Methods: Data were analyzed from eight independent annual cross-sectional surveys from a combined sample of 19, 315 individuals selected from 7,253 households. …..

Response: We have considered this and changed the paragraph

Background: The authors have used outdated references to describe epidemiology of malaria in the study. They should use the current references

Response: This have been revised and new references included. However, because this a trend analysis of historical data, some past papers are included to explain some of the events and milestones in the periods of the study
Results: Characteristics of study participants should be moved to Methods section

Response: We have move the characteristics of study participants to methods sections under population
The authors need to review their analysis that compares parasite prevalence and socioeconomic status. The prevalence ration they have presented does not align with the proportions between the poor and the less poor.

Response: We noticed some alignment issues in the results where some rows titles were not alighted to the results presented. These has now been corrected. However we also emphasize that the prevalence ratios presented are adjusted estimates after conducting multivariable analysis and not merely dividing proportions in each sub-groups. The aPR stands for adjusted prevalence ratios

The tables should be reviewed to demonstrate sample size for each sub-population category presented in the tables. They should be shown in each category column in the tables.

Response: We have introduced a column in the tables to include sample size for the subgroups. However we emphasize that due to large size of the tables, we have use column heads to report overall sample size rather the subgroup per year.
The authors should replace the drugs' brand names with generic names
Response: This has been done. See table 1
Table 2 is empty. It should be deleted if does not report any information

Response: Sorry abut that. Table 2 is there and is a key table showing trends of malaria parasitaemia.
Table 3 does not have column headings, so it will be difficult for the readers to follow them.

Response: We have column headers as years for table 3 but we are using row headers as well to give descriptions

Discussion
The study has reported the obscured variation in many outcomes that it has shared, eg no variation in ITN use between the poor and less poor population groups. Why was this
case?
Response: We have not included a trend analysis which show that there was significant increase in use of ITNs over time. However, in the pooled data, the poorest individuals we less likely to use ITN. The explanation as to why the poor are less likely to use nets is beyond the scope of this quantitative analyses but may require further qualitative research. Generally, poor individuals are vulnerable and live in impoverished conditions including lack of proper dwellings, poor knowledge and even lack of enough sleeping place. These factors amongst other factors may lead to non-use of ITN by the poor

Has there been any intervention that was implemented to address in equality in interventions coverage.

Response: None of the interventions implement are provided to address equity concerns they are often provide to everyone and individuals have to decide to utilize them. From an economic perspective the poor are less likely to access these interventions for various reasons even when they are provided for free.

What was it, where was it implemented and when? What is the

likely development now?

Mass distribution of ITNs was first done in 2006, 2011 and were distributed in the communities. The individual heads of households or their representative were notified to collect them are designated canters. ACTS are provided at the public health facilities free to children underfive

Despite the lack of variations in malaria control interventions coverage between socio-economic groups from the study, the authors have reported unequal distribution of malaria parasitaemia favoring the poorer population, the manuscript needs to raise the discussion on this.

Response: This had been adequately addressed in our discussion possible explanations of variations in coverage

VERSION 2 – REVIEW

REVIEWER	Bernt Lindtjorn Centre for International Health, University of Bergen, Norway
REVIEW RETURNED	31-Jan-2019

GENERAL COMMENTS	This is my second review of this paper. Unfortunately, the authors have not addressed some of my serious concerns of this paper. The paper describes a statistically significant change over time. However, this changes were very small, and a few percentage points of differences would probably not be regarded as of public health importance. This is an observational study and contains information bias, selection bias and confounding. The authors do not provide sufficient details on how they selected the households they studied. The authors state that they used a «Systematic sampling technique» and that «different sampling strategies were selected for logistical purposes». I understand that this sampling technique could be labelled a “convenient sampling method”. It would increase the selection bias in the study. From their response to my questions, it seems as if the unit of analysis was the communities. I disagree with the statement that some form of repeated measurements or multilevel analysis was not necessary because their unit was the community. The authors did not do a sensitivity analysis to prove their point, nor did they do a multilevel analysis to find out if there was no dependency of the data at the community level. Given the very small effect size, and using an observational study design with a number of biases and confounding that they did not adequately address, I regard their conclusions to be uncertain.

REVIEWER	Dr Rashid A Khatib Ifakara Health Institite, Dar es Salaam Tanzania I worked with Dr Kachur, SP in a CDC - IHI collaborative malaria program in Tanzania from 2001 - 2007 and we have co-authored several papers
REVIEW RETURNED	04-Feb-2019

GENERAL COMMENTS	None

VERSION 3 – REVIEW

REVIEWER	Bernt Lindtjorn Centre for International Health, University of Bergen, Bergen, Norway
REVIEW RETURNED	02-Sep-2019

GENERAL COMMENTS	The authors have now addressed the earlier questions that I had raised.