Reviewer's report

Title: A framework for the improved use of routine health system data to evaluate national malaria control programs: evidence from Zambia

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Reviewer: Patricia Graves

Reviewer's report:

I enjoyed reading this very interesting paper. The methods represent a significant advance on previous similar work. The use of both confirmed and clinical malaria cases, the use of anomalies to adjust for seasonal variation and the inclusion of climate variables was commendable. The problem is that there are relatively few years analysed (2009-2011) and these were years of very significant changes in diagnostic availability, reporting and other factors, as the authors acknowledge. The lack of programmatic data on net distribution led to need for extrapolation from surveys and the inability to adjust for net distribution or durability over time (by month). The multiple layers of assumptions on top of spatial predictions make the methodology hard to assess clearly, especially for a reader not fully up to date with Bayesian techniques.

I think the short Results section in the abstract about one small and (in my opinion rather misleading) association with nets (since most of it seems to be coming from the low endemic areas) did not capture at all the main points in the paper. My reaction on reading it was:

1. this paper introduces some significant methodological improvements to the assessment of malaria control effectiveness, most notably the inclusion of diagnostic and reporting rates and methods to extrapolate spatially from cross-sectional surveys to net coverage indicators by district (although one would not need to do that if one had the programmatic data)
2. malaria is increasing in Zambia in most regions, and nets do not seem to be having impact on that in the highest incidence areas
3. that control programmes would have a hard time repeating this analysis on a regular basis. We need to work towards simpler and clearer methods that can be applied in country by the control programmes.

I think this paper would be better split into two (although I realize that has cost implications in this day and age). One could be the methodological approaches and perhaps description of the malaria situation and interventions/changes applied, and the other the application of the methods to the Zambia data to draw conclusions about effectiveness of different control methods. Currently, the interpretation of the data and its summary in the results section gets the least attention given all the effort needed to describe the methodological issues.

The most important concept that control programmes do not seem to usually
grasp is the necessity to quantify inputs (nets, spraying etc) in the same geographical and time units as the outcomes (incidence by district-month, for example). This paper could contribute to that process but that seems to be a bit lost in here. If (e.g.) net distribution could be tracked by district and month, it would not be necessary to do the complicated extrapolation from relatively crude ITN cross-sectional survey data.

A lot of effort has been gone to here to gather complete data for every district – however while it is valuable to have data everywhere for monitoring of the program, more sophisticated analyses to quantify effect size for particular interventions can be done using a subset of districts that have complete data. Another reason to split it into two.

Major Compulsory Revisions:

1. The abstract refers to ‘district-time’ units in the Methods. I initially assumed that the time unit was month for all variables but became a bit confused with the net data when I realised it was annual (‘Methods: Measures of primary exposure variables’) and apparently included as a constant in each year of the study?

Could it not have been adjusted or interpolated somehow over time? As it stands, and given all the assumptions made, I don’t find the results on impact of nets too convincing.

Does Zambia not have the records of when all the nets were distributed, and where?

2. IRS: in the Methods: Measures of primary exposure variables para 2, the IRS data is dismissed rather quickly as being endogenous and not considered further (although included in the model as ‘control variable’). Endogeneity is definitely likely, but we really need some more basic information on how much IRS was done, where and when, given the focus on IRS in Zambia and the issue of whether to do combined IRS and ITN in the global community these days. It seems glib to just dismiss it as ‘oh we can’t tell what is happening due to IRS’ since that is the whole point of studies such as this, to determine the effectiveness of all interventions. More exploration of the endogeneity issue and how to deal with it would be welcome, since IRS impact has to be assessed somehow or why are we doing it? There’s a bit too much emphasis on ITN. Did you look at calendar month as instrument as in Over et al 2006 AJTMH?

3. Intro para 2 and 3 – I think the description of previous longitudinal time-series studies as ‘simple’ is not really valid (many used climate variables and/or multiple sites for example e.g. Thomson et al in Botswana, Coetzee in S Africa, Loha and Lindtjorn in Ethiopia 2010, Abeku studies in Ethiopia) and the novelty of the district-time approach is slightly overstated. Apart from being a common econometric approach, not just invented by Victora for health, it was applied for malaria in both Over et al and the Eritrea studies Graves et al (cited in discussion). It’s true previous studies did not adjust for everything mentioned: diagnostics, reporting etc but did have better data on monthly intervention inputs and used imputation for the reporting issues. Please revise these paras.
4. Please be consistent in use of ITN ‘coverage’ terms (coverage includes ownership, use and access) and distinguish the definitions. On page 4 we have “ITN household possession’ (= % of HH with at least one?), on page 5 we have ‘ITN program intensity’, page 11 we have ‘ITN:household ratio’ para 2, and ‘overall ITN coverage’ in para 3, Figure AF3 shows ITN per person’ etc – please clarify and be very consistent about the variables used in models and elsewhere.

5. On page 11 we suddenly get a reference to AF 6 – what do the other parts of the additional files add? Please include some reference to them, put in main doc or leave them out.

Minor Essential Revisions:

1. Please cite additional files in order and you have two AF 6 figs, please resolve
2. Discussion para 2, last sentence, it is not quite true that studies did not try to account for correlated data on malaria over time – we did include malaria cases lagged one month in the Eritrea studies to try and get at the time correlation, so please note that. For spatial correlation, that is true.

Discretionary Revisions:

1. It is unfortunate that inpatient cases were not also considered, as they may be less subject to the diagnostic and reporting biases of outpatient cases. It is stated in the ‘Methods- Study site’ that these were available. Please either include them or comment on why inpatient cases and deaths were not used (I assume for deaths there were too few but please comment).

2. As mentioned above, please consider splitting into two papers with one focused on methodology and one on the results of the analysis of effectiveness of control methods. (using inpatients as well if possible)

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests