Reviewer's report

Title: Bayesian random effects modelling with application to childhood anaemia in Malawi

Version: 5 Date: 29 November 2014

Reviewer: Ezra Gayawan

Reviewer's report:

The authors have improved on the previous version of the article but there are still some concerns that need to be addressed before I can recommend for publication.

Major:

Abstract: The background needs to be rewritten to introduce us to the problem instead of what we are currently having.

Pg 4, last sentence of the first paragraph, I don't know what flexibility the authors are referring to that have been neglected by all the studies they cited. They will need to be specific and convincing or remove the statement "but all these studies ...". The statement is also repeated in the last sentence of the second paragraph.

Pg 14, Discussion, the authors argued that "Modelling of metrical covariates non linearly avoided the rejection of some important covariates ...". They used age as an example for such argument. I do not agree that the inclusion of 0 in the CIs that resulted in the saying that age was not significant in the fixed effects means that it is same as the significance of the variance component. In the former, the significance is in respect to the reference category. If the reference category is changed, you can have a different result for some levels and some can be significant. I therefore suggest that the authors relax the issue of significance for the nonlinear effects and in particular, not compare that with when the variable is categorized. You can, for instance, present an argument that using smooth functions allows us to view the detailed relationship between the variable and the response variable etc.

Pg 16, line 11, child and mother's age were presented as nonlinear effects not fixed as mentioned here and should therefore be so discussed.

Pg 16 line 23, "Child age has been found to have both linear and nonlinear effect" is contradictory. Stick to the results of model 4 you presented.

Pg 17, line 9, the statement "Breast feeding had no linear effect ..." contradict what is reported on page 11 under Results. I still suggest the authors relax on the issue of significance for the nonlinear effects and concentrate on the patterns exhibited. The same applies to the statement "Mother age had a non linear effect ..." in a few lines under. It makes the whole thing look confusing.

Minor:
Pg 3, last sentence, "Sickle cell disease has been also ... " should read "... has also been ..."

Pg 5, the paragraph before Methods states that "in Section 2, ...", since the sections of the article are not labelled, if they will remain so in the final version then the paragraph is unnecessary.

Pg 5, insert "and" between Demographic and Health

pg 6, line 12 from top, "cut of point" should read "cut off ...

pg 6, line 18 from top, "eat meat" should read "whether or not the child eats meat". Since the study includes children below the age of one year, it should not be expected that those children would have been able to eat meat to warrant its inclusion as a variable.

Pg 7, line 4, the statement "The independent variables were ..." might not be necessary as this has been said in a few lines above here.

Pg 11, line 7, should read, "Discussion of the results will therefore be based ..."

Pg 11, last line but one, 6 not 5 months

Pg 13, Table 3, it is not necessary presenting BIC and its values in this table. As can be seen, the results of BIC does not suggest that model 4 is the best and there is no reference made to that in the text. So, why not removing it?

Pg 14, line 5, I would say 22 years instead of 25.

Pg 15, 8 lines from below, I think there should be a reference for the statement "Areas of higher altitudes are associated ..."

Pg 17, line 3, do you mean to say premature birth instead of prematurity?

Pg 17, last paragraph I think you might just say that some important variables such as deworming, which might affect anaemia are not included in the DHS survey and hence in this study. I do not see any reason the variable would create bias to the spatial effect.

List of abbreviation: I do not think this list is necessary.

Figure 4, the label includes "Green (negative effect), gray (insignificant effect)" etc but in actual sense, the map is not meant to show the significance but just the estimate. Hence, I suggest you remove this so that the other figures portray the significance.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests