Author’s response to reviews

Title: Spatial-temporal analysis of malaria and the effect of environmental factors on its incidence in Yongcheng, China, 2006-2010

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Author’s response to reviews: see over
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The editor,
BMC Public Health
Re: Spatial-temporal analysis of malaria and the effect of environmental factors on its prevalence in Yongcheng, China, 2006-2010

Dear Editor,

We have attached an original article titled “Spatial-temporal analysis of malaria and the effect of environmental factors on its incidence in Yongcheng, China, 2006-2010” for consideration in your esteemed journal. This study not only analyzed the spatio-temporal pattern of malaria in Yongcheng, China, 2006-2010 and offered a good-fit model for *Plasmodium vivax* malaria incidence based on the data of multi-factors, but also gave several meaningful suggestions on further optimizing malaria surveillance in areas of unstable malaria transmission. All authors, such as Yan Zhang, Qi-Yong Liu, Rong-Sheng Luan, Xiao-Bo Liu, Guang-Chao Zhou, Jing-Yi Jiang, Hong-Sheng Li, and Zhi-Fang Li, have participated meaningfully in the analysis, interpretation of the data, and writing of the manuscript and concur with the submission and subsequent revisions. This manuscript has not been published nor is being considered for publication elsewhere. We certify that we have no affiliation or financial involvement organization or entity with a direct financial interest in the subject matter or materials discussed in this manuscript. Our work contains no extracts from other copyrighted material.

I have addressed the comments and revised the manuscript according to the journal style. A point-point response to reviewers’ comments were listed as follows:

Referee 1:

Major Compulsory Revisions:
1. Conclusions are obvious and old. The authors could conclude the results better. e.g. "Spatial-temporal analyses offer powerful tools for understanding incidence shifts as environment change. Historical weather and malaria data can be critical for optimizing current malaria monitoring and control, and malaria control targets should vary with the malaria transmission intensity, with more public resource allocated to controlling source of infections instead of *An. sinensis* density when malaria incidence is low."

Thanks a lot for your suggestion. I have tried my best to get the conclusions better.

2. The model has not really be extrapolated and validated for prediction, only the fit
of historical malaria incidences. So, it is far from a reliable model (mentioned in section "conclusion", "A reliable model has been developed in this study to predict the expected incidences of malaria based on historical malaria epidemics and a combination of weather factors at one month lag, which would simplify malaria surveillance by targeting control of malaria more effectively.").

Good advice. I have changed the wording. Thanks a lot.

Minor Essential Revisions

1. It is need to shorten the methods, and literatures can be used instead, e.g., "Kriging analysis for spatial interpolation of meteorological factors.", "GEE models".
The methods have been shortened. Thanks for your advice.

2. Some of sentences need be removed from section "Results", e.g., "According to QIC values in models with various lag sizes, it can be concluded that An. sinensis density, average, maximum and minimum temperature, average and maximum humidity had 1-month lags respectively; rainfall had a 2-month lag; average, maximum and extreme wind velocity had no lags (Table 1). All these significant factors with various lag sizes were entered into the multivariable model.", ...
Thanks. I have removed this sentence from the section “Results”.

3. Poor plotting or mapping for figures e.g., on legend, scale, small resolution, etc.
I have re-plotted and re-mapped some figures which have a poor quality. Thanks.

4. Significant review for English grammar and readability is needed.
Thanks a lot. I have tried my best to revise the English grammar and readability.

Referee 2:

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached) I have none of this. By and large, the paper is a source of new and critical knowledge that would be very helpful in the design of control malaria and China.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct): I have however some questions which need to be clarified as follows:

1. I am not sure if it is okay to use prevalence and incidence interchangeably. It was mentioned that the implication of the paper is on the effect of environmental factors on the prevalence of malaria, yet the paper used incidence data. Won't there be any
implication in the use of incidence data?
Sorry. I had a little confused about the meaning of prevalence and incidence. I have already changed the wording of prevalence into incidence. Thanks for your reminding.

2. I am not sure why the prevalence is expressed in many different ways such as in percentage, in number of cases per 1,000 or per 10,000 or even per 100,000. Can there be consistency in expression of prevalence?
Yes. The expression of prevalence has been changed into a same expressing way. Thanks very much.

3. The study mentioned that there have been controversies on the interrelationship between meteorological factors and malaria. Can the authors cite some of these controversies.
Yes. Controversies lie in that some experts argue the relationships between weather factors and malaria can’t be detected, and that the meteorological factors that have significant statistical correlation with malaria vary greatly over space. I have added some examples to prove that.

4. Did this paper resolve some of these controversies or raised more questions than answers.
Yes, this paper indicated that weather factors have exerted great influences on malaria incidence in the middle area of China, and the significant factors were temperature and humidity, which objected the former controversy I mentioned above, and agree with the latter one. Questions raised in this paper were: 1) that why temperature, humidity were included into model 2, 2) that why mosquito abundance was failed to be included into model, 3) and that why expected values of malaria incidence in the last two years of study period was overestimated in model 1? These questions were analyzed and answered in this paper.

5. Is there much difference in the density data collected by human baits and by bed-net traps? Is there an explanation for this?
Yes, there are. Human-bait catches and bed-net traps are two methods required by National malaria monitoring programme (Tial) for measuring Anopheles density. Therefore the density data used in this study are the most comprehensive data we can obtain. The mainly differences between the methods of human-bait catches and bed-net traps lie in following points: first of all, human-bait catches were used in outdoor areas, while bed-net traps were used in indoor areas; moreover, the former one was performed in the evening, targeting the population who were out before
sleeping, while the latter one was conducted in the early morning, targeting the population who were sleeping at home.

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)
I am attaching a copy of the paper which I copy pasted on word format so I can put my corrections. I did some editing on the paper which I thought would help in getting the message across but the editing has not been complete. It would be good for the authors to go over the paper again for the language review part. Thanks very much for your corrections. It helps me a lot.

Again, thank you very much for your consideration of this manuscript. If you have any questions, please do not hesitate to contact us.

Sincerely yours,

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