Reviewer’s report

Title: Early Warning Signal for Dengue Outbreaks and Identification of High Risk Areas for Dengue Fever in Colombia using climate and non-climate datasets

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Reviewer: Marco Ajelli

Reviewer's report:

Lee and colleagues developed an early warning model of dengue transmission risk based on an environmental/climatic index. Actually, the index they have defined is more general and, as also argued by the authors, could be applied also to other mosquito borne infections.

Developing new indicators to be used in early warning systems is a highly important topic. The index suggested by the authors sounds reasonable and the use of one single index has the advantage of limit the (statistical) issue of multicollinearity.

My main doubts on this works are: 1) the performed quantitative validation is lacking (see below), and 2) the temporal window used for defining the index sounds very arbitrary.

1) The authors claim that their EWS is (in general) able to predict future epidemic outbreaks. However, EWS only provides an alert that there is an increased risk of experiencing an epidemic in the coming months. The model has not the capability to predict the timing of the outbreak (which, as also stated by the authors, might occur just in one month or even 5 months later) or the size of the outbreak, etc. A quantitative analysis showing what the system is really capable to predict the epidemic (e.g., in terms of the lag between the detection of the signal and the start of the epidemic and/or of the epidemic peak and/or magnitude) is needed.

2) The choice of averaging over 12 months to define the CRF index sounds very arbitrary and pretty long with respect to the actual duration of dengue outbreaks. A sensitivity analysis showing the effect of this choice is required.
Other comments.

- I am not familiar with the surveillance system used in this study. However, in most surveillance systems, when a municipality is not responsive, it often means that the municipality is not observing cases or having just little activity, well below the average. Therefore, it is hard for me to agree with the authors' assumption that non-responsive municipalities are experiencing the same activity as the other municipalities of the same department (lines 145-146). The authors should support their assumption with strong evidences or, alternatively, perform a sensitivity analysis on this choice.

- In general the paper should be toned a bit down about the predicting capability of the method (unless the authors are able to quantitatively validate the capability of their method - see comment 1 of this report).

- Line 177-178. Actually for vector borne diseases high population densities have two opposite competing effects: i) dilution of infections individual in a large pool of possible hosts to bite, leading to a decrease of the effective reproduction number, and ii) a large number of susceptible hosts to infect, which means the potential for a large number of cases when the reproduction number remains constantly above the reproduction threshold. As such, the paragraph around Line 177-178 should be revisited.

- Line 116. It is actually not necessary a large number of vectors to experience an epidemic; it is sufficient that the product of the number of vectors, biting rate, and transmission probability is large enough to sustain the epidemic. Please rephrase the sentence.

- Line 118. The one mentioned by the authors is one of the possible reasons. Another reason could be the importation of a case in a community with low immunity to that specific serotype. Please rephrase the sentence.
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.

Yes

Are the conclusions drawn adequately supported by the data shown?
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Yes

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