Reviewer's report

Title: Assessing real-time Zika risk in the United States

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Reviewer: Thomas Walker

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The authors present an interesting framework to assess real-time risk of ZIKV transmission in the southern United States with a focus on the state of Texas. The background provides a good introduction to the ZIKV outbreak in the Americas and highlights the autochthonous ZIKV cases in the Southern US states of Florida and Texas. The authors present the case that greater surveillance is needed given the presence of the primary mosquito vector, Ae. aegypti, in the state of Texas.

My main comment is the need to address the entomological parameters of Ae. aegypti in Texas (and within individual counties) given the strong association between vector abundance and outbreaks of arboviral diseases such as ZIKV and DENV. I believe modelling the relative risk at the county level would require greater knowledge of the vector abundance and extrinsic incubation period. However, I appreciate that the manuscript provides a framework rather than measuring a definite risk of ZIKV transmission so the following specific points would help the reader with concerns regarding the entomological side of ZIKV transmission.

Specific points:

1. A discussion on other potential vectors that are present in Texas (Cx. quinquefasciatus, Ae. albopictus) in lines 55-57 could be expanded to highlight the risk of autochthonous transmission from other potential vectors. The evidence so far points to Ae. aegypti but work is still ongoing to incriminate other mosquito species (some of which are present in the state of Texas). If the authors could acknowledge this and how it would affect their modelling and simulation it would strengthen their manuscript.

2. Lines 152-159: The authors state that The risk of ZIKV emergence following an imported case will depend on the likelihood of mosquito-borne transmission. For emerging diseases like ZIKV, the public health and research communities initially face considerable uncertainty in the drivers and rates of transmission, given the lack of field and experimental studies and our case study, we estimated county-level transmission
potential using a recently published model [20], but emphasize that these estimates should be refined as the understanding of ZIKV improves.

I have to say that my first reaction is that if there isn’t enough information to provide an estimate based on current knowledge in counties in Texas then couldn't it not be considered too early to produce estimates? Maybe the authors could emphasise that this framework will have much greater value in the future when more parameters of ZIKV transmission are known.

3. The county transmission rates (Ro) - lines 163 - 170 - include a temperature dependent formulation of the Ross-Macdonald model which is required given the influence of temperature on mosquito abundance and EIP. Could the authors provide a few more references to demonstrate this relationship between temperature and arbovirus transmission? Brady et al. and Muir et al. look at mosquito survival but what about temperature and arbovirus EIP? I do appreciate that there is substantial uncertainty for measuring Ro (lines 179-181) and this is not the main focus of the model but the influence of temperature on EIP should be addressed here given it's often used to explain why certain arboviruses are not likely to be transmitted in certain parts of the world without an increase in temperature.

4. What is the estimated EIP for ZIKV and where do the authors get this from for their model? The supplementary information section 4 (seasonal analysis) has the following sentence: "The extrinsic incubation periods of ZIKV in Ae. aegypti and the mortality rate of Ae. aegypti are temperature dependent [16, 17]." These papers refer to DENV not ZIKV so this needs to be addressed. If EIP for ZIKV in Ae. aegypti mosquitoes in a Texas genetic background is currently not known then the authors should state this.

5. Lines 175-179: mosquito abundance data appears to be taken from Kramer et al. 2015 which outlines the global distribution of Ae. aegypti. Could the authors just clarify how they were able to get county level mosquito abundance from this reference? In the SI, 1.2.1 the authors state ‘Using a high-resolution predicted global distribution of Ae. aegypti occurrence probabilities [7], we averaged the 5km2 estimates across the extent of each Texas county. We followed the methodology in [8] and assumed mosquito abundance is distributed as a Poisson variable; therefore, we obtained a proxy for mosquito abundance as \( \lambda = -\ln(1 - \text{occurrence probability}) \)." Ae. aegypti is an incredible invasive mosquito species so I would have thought a more detailed surveillance reference on a county-by-county level is needed. A quick search on government websites indicates that some Texan counties have no documented cases of Ae. aegypti. www.dshs.texas.gov/TaskForceID/docs/TAB4aIDTaskforce_0416_Sidwa.pdf. Do the
authors have access to any county-specific data that could be included to strengthen their mosquito abundance estimates?

6. Discussion - Measuring the risk of outbreaks will also depend on the current levels of mosquito surveillance and vector control programs. Can the authors comment on this in the state of Texas as a whole and at the county level? Similarly, from the gov’t website it would indicate that active mosquito surveillance programs are not widespread in the state of Texas. www.dshs.texas.gov/TaskForceID/docs/TAB4aIDTaskforce_0416_Sidwa.pdf.

7. SI 1.1.1. The authors state that “Given the geographic and biological overlap between ZIKV, DENV, and CHIKV, we use historical DENV and CHIKV importation data to supplement ZIKV importations. We believe this decision is justified, as an earlier version of our importation risk model fit only to DENV and CHIKV data predicted the county ZIKV importation distribution well [3]. "I would question this given CHIKV (in epidemic form) is predominantly transmitted by Ae. albopictus and ZIKV has human to human sexual transmission potential. Therefore, I would be careful to use historical data for DENV and CHIKV in the context of ZIKV.

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?
If not, please explain in your comments to the authors.

Yes

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