**Reviewer’s report**

**Title:** Assessing the magnitude and uncertainties of the burden of selected diseases attributable to extreme heat and extreme precipitation under a climate change scenario in Michigan for the period 2041-2070

**Version:** 0  **Date:** 05 Oct 2018

**Reviewer:** Jeremy Hess

**Reviewer's report:**

Thanks for the opportunity to review this interesting paper on projected disease burden associated with climate change in Michigan. The paper is an excellent demonstration of how local demographic, environmental, and health data can be combined with ensemble climate model outputs to generate combined estimates of projected disease burden associated with climate change. While there a number concerns that need to be addressed, once revised the paper will be a strong addition to the literature.

My first query relates to the paper's title. Why is only climate, and not climate change, referenced? It seems a more suitable title would be "Assessing the magnitude and uncertainties of the burden of disease from climate change in Michigan," or, perhaps more precisely, something along the lines of "Projecting the magnitude and uncertainties of the burden of selected diseases from climate change in Michigan for the period 2041-2070."

Overall the methods are well presented and clear, but several points need clarification. For instance:

* Figure 1 is somewhat confusing. It might be easier to follow if it were oriented horizontally, with initial activities such as historical outcome count and population data collection feeding into exposure-outcome association determination, and this flowing into historical disease burden estimation, etc.

* It is not clear whether the historical exposures were modeled (i.e., derived from GCM hindcasts) or were taken from actual historical data.

* The authors mention that the climate projections are downscaled. What are the temporal and spatial resolution of the downscaled estimates?

* On page 5 line 107 (P5L107), the authors report generating counts of days above 32.2 and 35C. Was this maximum temperature? And were the categories mutually exclusive?
* The authors might elaborate on why they chose to use a threshold approach for temperature but a distribution approach to precipitation.

* The authors should clarify whether all of the historical exposure-outcome associations are examined using same-day relationships or if lagged associations were assessed and included in the projections.

* In regards to the AFs, the authors seem to make the assumption that the entire population is exposed. They should make this explicit.

* The authors use the term "heat-related" to refer to ED visits and hospitalizations. It would be helpful for them to elaborate on what this refers to, as there are various meanings for this phrase in the literature.

* The authors exclude deaths and ED visits associated with accidents in their analysis, though there is literature linking heat exposure with increased rates of intentional and unintentional injuries. The authors should consider including these outcomes in their analysis or explicitly state why these outcomes were not included.

* In the paragraph on the EH-mortality association, the authors list several variables that they included in their analysis, but only some of these variables are listed as the stratification factors for the AFs. It would be helpful for the authors to clarify how all the factors were included in the analysis.

* There is a typo on P8L160.

* On P8L176, the text mentions only temperature threshold t, but the paragraph heading refers to both EH and EP. Was the same method used for EP?

* In performing the AF calculations for EP, what populations were assumed to be exposed, i.e., did EP exposure accrue to a county, ZIP code, or the state as a whole? Was the spatial scale matched for the projected climate?

* On P9L186-7, a scaling factor C was used. I may be missing something, but if the unit of analysis is person-days, why is a scaling factor needed, and what is the scaling factor correcting for? (One way to address these concerns is to include units for each of the different variables in the text.)
* On P9L198-200, it is not clear why the authors chose to use cost estimates for hospitalization for heat illness to estimate costs for renal hospitalization.
* Are cost estimates adjusted for inflation?

In the Results section, some additional questions arise:

* In the EH causal pathway review, why did ischemic stroke not factor into the analysis? Basu and colleagues (2012) have identified an association in other settings.

* There is an imbalance between the review of causal pathways for EH and EP. Why is so much more attention paid to EP? This section can be substantially shortened, as a fair amount of the material is somewhat extraneous to the analysis.

* For each set of causal pathways examined, it would be helpful for the authors to summarize at the end of their discussion the health outcomes included in the analysis after the causal pathways were elaborated.

In the Discussion section, on P22L531, the authors might consider revising the statement to state that the estimates represent the "non-accidental effects of EH on ED visits as identified in the Michigan-specific causal pathway." On P26L589, the authors might consider inserting "… though these lower emissions scenarios are becoming statistically improbable given recent historical emission trends" after "…estimates."

Despite the large number of comments in the above, overall this paper represents a monumental effort to synthesize a large collection of environmental, health, and economic data, one that will be a very useful contribution to the literature.

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