Reviewer’s report

Title: Estimating policy-relevant health effects of ambient heat exposures using spatially contiguous reanalysis data

Version: 1 Date: 15 Jan 2019

Reviewer: David Hondula

Reviewer’s report:

I appreciate the authors' efforts to improve the manuscript. The authors have thoroughly addressed the majority of the reviewer comments and provided appropriate justification in most cases of disagreement. I find the new text to be much better organized, with a stronger and clearer articulation of a problem statement and subsequent methodological approach.

There is one substantive comment from my initial review that does not appear to have been addressed and I believe, if incorporated into the manuscript, would boost its impact and appeal. The broad question is - are spatially resolved data (and/or this particular type of spatially resolved data) advantageous over other approaches to estimate the temperature-morbidity relationship?

In the abstract, the authors state that reanalysis data provide "refined" exposure-response functions. Later, the authors conclude that they demonstrate the "expediency" of spatially resolved climate data for case-crossover temperature-morbidity analysis. (First, I encourage them to confirm that the definition of expediency aligns with their intentions). However, the claim of "refined" and "expedient" is a tough conclusion to make given the lack of an alternative. The authors claim on line 43 of page 18 that "they were not able to compare estimates with those derived from air temperature data as dense air monitoring data is not available for an extended time period in the area." Regardless of how many stations are available, it still seems as though the authors should be able to generate some type of alternate approach.

At absolute worst, as a very crude approach, they could come up with a "statewide average" of all of the monitors that are available (perhaps with some sort of weighting scheme), and then use a time series of those values to relate to statewide counts. Better, they could assign each morbidity case to the nearest air monitor, and perhaps exclude cases with distances above some threshold (100 miles?). I think it is imperative that the authors demonstrate that there is value in their approach, relative to other options. One other possibility is for the authors to use Daymet or PRISM data, which are interpolated from station observations. An outstanding addition to the manuscript would be a short section in the results with an accompanying table that shows the difference in overall model fit (measures like AIC, etc.) for different input data sets to represent exposure. I maintain that it is the authors' responsibility to demonstrate that their methodological approach (which is interesting and appropriately applied, in my opinion) offers some value or advantage relative to other approaches.
Other minor comments:

- Do the authors have any justification for their decision to combine the ED visit and hospitalization time series? I cannot find instances where this has been done elsewhere in the literature, but often see the two disaggregated. I respectfully suggest that "small sample size" is not appropriate justification for this choice.

- The revised methods and abstract do not seem to refer to the reanalysis data, which is a very important component of this manuscript.

- Introduction, first sentence: I believe the claim about "increasing variability in future temperatures" remains a matter of considerable debate, and it does not seem relevant to the topic of this manuscript. I suggest removing to focus on the heat argument exclusively.

- Page 10, first line: "these" may be a typographical error.

My compliments again to the authors for the many improvements to the manuscript that have positioned it much closer to readiness for publication.

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