Reviewer’s report

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

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Reviewer: David Hondula

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The manuscript "Estimating policy-relevant health effects of ambient heat exposures using spatially contiguous remote sensing reanalysis data" presents an effort to advance our understanding of variability in heat-health risks by incorporating spatially-varying temperature data into a case-crossover analysis. The authors leverage multiyear health records available from New York State to examine how risks vary across a number of outcomes and independent variables. This work is framed around a goal of improving National Weather Service heat advisory criteria in the study region, which the authors note was accomplished as a byproduct of their research and collaboration with the NWS. Based on the health data they analyzed, the authors also report that rural and urban areas had similar risk for heat related illness, which is an important finding given a prevailing emphasis on urban areas in much of the environmental health literature related to extreme temperatures.

The manuscript address a topic that is relevant for this journal and the premise of the manuscript should be of interest to the journal's readership. The manuscript does not offer a transformative scientific advance, but does improve knowledge along several different topics of importance to the temperature-health research community.

Regrettably, I found a few shortcomings with the manuscript that constrain my ability to recommend it for publication in its current stage. At times, parts of the manuscript feel misaligned with the overall objective or incompletely address the objective. Not all of the study findings are well supported by or detailed in the methods and results sections. I also suggest that the authors missed some opportunities to contribute new knowledge about temperature-health effects in New York State. Most importantly, I think that there are two important threads to this work that get muddied into a bit of a confusing story. It seems as though the authors are trying to both inform weather service criteria with analysis of health records, and separately, to determine if spatially varying temperature data change and improve their ability to inform those thresholds. I recommend that they separate these goals (and the appropriate methods, results, etc.) in a future iteration of this manuscript. I hope that the authors will consider addressing these comments and wish them well in their ongoing research. Detailed comments follow below.

TITLE

The authors refer to "spatially contiguous remote sensing reanalysis data," but nowhere in the manuscript is it explained how and why it is appropriate to consider NLDAS "remote sensing"
data. I think it would be fine to simply refer to the data as a "reanalysis" product. (This same comment applies to all uses of "remote sensing" throughout the manuscript, unless there is a different data set included in the paper that is not well introduced).

ABSTRACT

1. The first sentence notes that weather service criteria were (historically) based on the frequency of heat events estimated by sparse monitoring data. While this may be true, is it also the case that the criteria were NOT based on analysis of health records? If so, it seems like the manuscript could be oriented around two objectives. First, to inform the weather service criteria with health-based thresholds, and second, to determine if there is value added from spatially resolved temperature data (and how that might influence the thresholds).

2. In the Background section, the authors note that they use "exposure data with a higher temporal and spatial resolution." I understand how they are using exposure data with a higher spatial resolution, but cannot determine where higher temporally resolved data are involved. The authors report most of their results in terms of daily maximum, etc., which is consistent with most contemporary literature.

3. In the Methods section, the authors say "maximum surface temperature," but I assume they mean "maximum near-surface air temperature given the data sets they are using? I believe they are working with estimates of air, rather than surface, temperature.

4. In the Results section, the authors report some of their results as risk ratios with confidence intervals, and other times only report percentage increases. Why are the results reported inconsistently?

5. In the Conclusion section, the authors note that thresholds were changed in the upstate region of New York State, but nothing in the sections leading up to the Conclusions referenced any type of spatially stratified analysis.

INTRODUCTION

6. Line 78: As noted previously, I am not sure if the remote sensing angle is appropriate to include in this manuscript. It also seems like a bit of a reach to claim that the application of remote sensing data has "overcome" these issues because of the very different nature of the physical variables measured by remote sensing tools (surface temperature) versus reanalysis products and station data (air temperature). The references provided to support this point seem marginally relevant at best.

7. Line 87: Determining heat-health thresholds to inform weather service criteria is not dependent on the availability of fine-scale temperature data; others have done so with the monitoring data that are more commonly available. This is where I believe the authors could separate their manuscript into two different objectives, first to establish thresholds, and second to
see if and how the incorporation of the reanalysis data change their conclusions about what the important thresholds might be (and for different regions, etc.). A few references related to threshold identification are below.


8. Line 102: The scale of the meteorological data set is mentioned here, but the scale of the health data is not. Could the authors mention that they used residential address-based health records?

METHODS

9. Line 140: Although it is a subtle point, the authors might want to note that their maximum temperature (based on hourly data) slightly differs from the maximum temperature data that would be reported from a typical monitoring station with min-max capabilities. This subtle difference can translate into differences in estimated temperature-health effects (see Davis et al. 2015, as an example).


10. Line 141: Could the authors explain how they calculated daily maximum heat index? I assume that they calculated the heat index for each hour of the day and then chose the maximum, but this is not articulated. In future work, perhaps the authors could look at the maximum, minimum, and mean of both dry bulb temperature and heat index?

11. Line 175: Here the authors specify that they calculated risk ratios based on temperatures in degrees C, but earlier specified that their temperature data were in degrees F. In which step of the process did the unit conversion occur?
12. In general, I think the methods section could use a few overview sentences that explain the different types of effect estimates the authors are going to produce. In line 180, they note that they assessed urban-rural differences, but exactly what this means is not entirely clear at this point in the manuscript. They also go on to report effect estimates across different demographic variables, and others, but this is never clearly established as part of the approach in the methods section. Please be sure that the methods section explains all of the different tests, models, and effect estimates that readers can expect to find in the results.

13. Are the authors combining the emergency department visit and hospitalization data into one time series for each different diagnosis code? If so, what is the justification for doing so?

RESULTS

14. Line 190: Following from the previous comment, the different health events that were assessed should be outlined in the methods section. It may be helpful to repeat them here, but I strongly urge that the authors establish their suite of tests first in the methods section.

15. Line 206: As was the case in the abstract, here the results are reported as percentage increases but elsewhere results are reported at relative risks. Would it be an improvement to report the results more consistently?

16. Line 217: Some of the text in this paragraph feels more appropriate for the methods section - a new methodological technique is introduced related to spline terms, etc.

17. Line 222: The text here feels fairly subjective amidst what is otherwise quite an objective analysis. The authors report "only shows small increases below" and "much steeper slopes at higher temperatures" and "slope levels off." Would it be possible to use a more objective technique to quantify where these important thresholds are and how the effect estimate changes at different temperature ranges? The Petitti reference above provides one possible approach, but also contains references for other techniques.

18. Line 226: I'm not sure that it is so interesting that the slope for the heat index is shifted to the right given that, during the summer months in New York State, the heat index will be higher than the dry bulb temperature. I think it would be more interesting to discuss how the model fit compares between different exposure variables. Of the four exposure variables the authors considered, which best fits the data?

19. Line 236: The language here is confusing - what do the authors mean by "elevated risk albeit smaller in magnitude." To what does "elevated" refer?

DISCUSSION

20. Line 248: It is not clear that the references provided here strongly support the claim that inter-individual variation is a "major" source of uncontrolled confounding in heat exposure
studies, and one reference is not even topically relevant as it focuses on air pollution. My sense of our contemporary understanding is that inter-individual variation in exposure (at least assigning exposures from spatially resolved temperature data, ignoring true personal exposure), could result in small to moderate shifts in effect estimates. A more appropriate reference might be:

21. Line 252: The discussion here seems a little too constrained in terms of the factors that should be considered in setting thresholds for NWS heat products and the message content. I agree with the authors that heat morbidity data are useful to include as part of the decision-making guidance, but what about other concerns? Heat products are not only issued to minimize morbidity, but also mortality. Would the results be different had the authors examined mortality data? How does variability in the thresholds found for different morbidity outcomes get accounted for? Beyond those concerns, what about effects outside of the health sector? And what evidence could the authors provide that more warnings would result in more behavior change, given the statistics reported about behavior change from heat warnings in other research, as well as concerns about warning fatigue?

22. Line 282: The claim that gender-related effects vary by study could be supported by a few references.

23. Line 291: This is the first time, as best I can tell, that information specific to New York City appears in the manuscript - the last sentence of the results section only mentions "rural and urban" areas more generally. Can the authors add to the methods and results section to more explicitly describe any separate analysis they ran for New York City? In the same section, why does the coastal location contribute to higher risk? And what basis do the authors have for claiming that the urban heat island effect results from evapotranspiration, which is a cooling process?

24. Line 338: By what objective basis did the authors come to conclude that a threshold of 35 C was appropriate?

CONCLUSIONS

25. Line 361: I do not believe that this study is the first to use spatially contiguous exposure data to inform heat-health efforts. See, for example, Shi et al. 2016.


26. Line 365: The claim about transdisciplinary research seems irrelevant to the rest of the manuscript; this conclusion is not supported by the methods and results.

FIGURES AND TABLES
27. Figure 2, it may be helpful to separate the different outcomes into different figures so the variability in the effect estimates for the outcomes other than heat stress are more easily discernible.

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