Reviewer’s report

Title: County-level heat vulnerability of urban and rural residents in Tibet, China

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Reviewer: Jonathan Levy

Reviewer’s report:

This manuscript seeks to develop indices of heat vulnerability in urban and rural residents of Tibet, to help in developing adaptation plans in response to global warming. This is a timely and relevant topic, since decision makers will need this sort of information to target and prioritize. The analyses in the manuscript involve selection of 10 demographic variables that are hypothesized to be related to heat vulnerability in Tibet, with factor analysis used to reduce the number of variables and determine overarching demographic constructs. While these analyses were reasonable and are very similar to previous work in the US (identical methods with overlapping covariates), they are a bit limited given the variables available and without insight about which constructs would be more associated with heat vulnerability and should therefore be weighted more heavily. In the absence of some sort of evaluation or validation, either with the study population in Tibet or elsewhere, it’s hard to know the value of these measures or how they can be used. Since the authors previously published multiple studies examining predictors of heat-related mortality and morbidity, it would seem possible to test out these or related indices in some manner; while the indices are ecological and may not apply at the individual level, some more explicit linkages may be beneficial and could help to build on the existing literature to a greater extent. Otherwise, the major advancement that this study provides is the geographic context for the analysis, which is important but a relatively incremental contribution given the use of essentially identical methods as prior studies but with more limited input data.

Major compulsory revisions:

1. P. 6-7: This section provides the list of 10 variables and some rationale for their inclusion. Some are logical and directly connected to studies in Tibet, while others are more remote (for example, loss of labor ability as a surrogate for cardiovascular or respiratory disease status, or evidence of the effect of small living spaces from Chicago with an assumption that this applies directly to Tibet). In addition, multiple variables overlap by definition and it is therefore not surprising that they load together in the PCA – households with only one room vs. households with < 8 m² of living space; % of people receiving living allowances among the total population, the elderly, and all households; and so forth. The work would be far more compelling if there were a richer set of variables explored. Is there any information on housing type? On occupations where one works outdoors? As it stands, the authors chose variables that fit into 4 basic domains, so it is somewhat tautological to show that they load onto 4
distinct factors.

2. P. 7: As the authors later acknowledge, the way that the heat vulnerability index is constructed is somewhat arbitrary, including the scoring system and the identical weighing of the factors. This scoring system is identical to Reid et al., so there is precedent in the literature, but it seems like the authors could try to take this further to make some novel methodological contributions rather than repeating the Reid method. As a small point, why not use z-scores directly instead of 1-6 scales? Why not weigh the factors in some manner that relates back to the epidemiological findings (i.e., as a function of relative risk)? There is nothing intrinsically wrong with the approach taken by the authors, but it also doesn’t add anything novel to what has been tried previously, and the outputs are therefore a bit disconnected from true indices of vulnerability.

3. P. 8: The results described here and in Table 3 are a bit of a black box. The authors present the vulnerability scores by county but don’t unpack the values for the reader. What differentiates the counties from one another? What factors score similarly to one another across counties and which have a broader spread? This type of discussion would help to get to the nuances of the data presented.

4. P. 13: Some of the text is a bit overstated given what the analyses are. For example, the authors posit that “the factors that put people at higher risk in urban areas differ from those acting in rural areas”, though what their analyses really show is that demographic correlations differ slightly in urban vs. rural settings (and the only major difference is that illiteracy is more associated with home size in rural settings and with age and disability in urban settings, which is interesting but some distance from heat vulnerability). Similarly, the study doesn’t really suggest the importance of multi-sectoral engagement – multi-sectoral engagement is clearly important, but it’s not clear how any of the work presented implies the importance of engaging numerous sectors outside of health.

Minor essential revisions:

1. P. 3: The authors reference their previous publications evaluating heat-related outcomes in Tibet and identifying vulnerable populations, but there is not an explicit connection between those findings and the indices developed. For example, the text states that those age 65+ and males were most affected, but the indices use a different age cutoff and don’t use gender. The text should be revised to emphasize how the previous work by the authors informs the development of indices, or explain why there are differences for ecological indicators relative to individual-level covariates.

2. P. 11-12: There is a fairly extensive discussion here about the capacity of Tibet to address climate change, including detail about public health sector capacity building. This is all valuable information but not at all related to the research done by the authors. The paper may be better off focusing on the task at hand and discussing the interpretation of the indices and how it can be improved.

**Level of interest:** An article of limited interest
Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.