Reviewer’s report

Title: Poverty, urban-rural classification and term infant mortality: A population-based multilevel analysis

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Reviewer: Bernardo Hernández Prado

Reviewer's report:

This is an interesting paper exploring the association of poverty and urban/rural classification with term infant mortality at the US, and controlling for individual factors. The authors make a good description of the problem in the introduction, and clearly describe the methods and results.

Some specific comments:

1. In the abstract, mention the number of births included in the analysis, it is impressive and I think should be highlighted.

2. Background. I would suggest to revise the wording of the last paragraph (lines 87-90), where the authors state the purpose. It can be stated in a simpler and more direct way. The last sentences (lines 91-93) may be better suited in the methods section.


   a) Page 5, line 105, why only singleton births?

   b) It is important to give more details about the cases that are excluded from the analysis (page 5, lines 108-114). The excluded States are either poor (Alabama, Arkansas, Mississippi), or rich (Connecticut). More important, cases were excluded because of missing data (we will know 10% in the discussion). Why these cases were lost? Which variables are missing? Is missingness at random? Can a bias be introduced because of this missingness? It is important to give more details here, and expand the corresponding discussion in the discussion section.

   c) Is it possible to mention the cut-off for the urban-rural categories (lines 120-122)?
4. Results:

a) I would start with the description of covariates (table 1) and then start describing infant mortality rate (reverse last 2 paragraphs of page 7).

b) In this study, the individual variables can be located in the causal chain between poverty and term infant death (e.g. poor counties may have more tobacco consumption, less antenatal care, less maternal education, and therefore higher infant mortality rate). The authors do right in using a multi-level logistic regression approach to account for this. Using just a standard multiple regression approach may not be appropriate, because this fact may not be taken into account. But exactly because of that it is important for the authors to highlight the use of this type of model. When presenting the results and in table 2, models 2 and 3 are an interesting exercise, but I think more emphasis must be given in the results and discussion to model 4. This is the one in which we can see the association between poverty and urban/rural with term infant mortality properly adjusted by individual characteristics. This contribution has to be also highlighted in the discussion.

c) Given the huge sample size, it is relatively easy to find significant results. I think the discussion of results should concentrate more on the change in magnitude of the estimates, and not only in their significance.

5. Taking advantage of the huge sample size, would it be possible to assess some interactions between poverty and urban/rural categories? It could be a "model 5", including the covariates in model 4 plus some interactions, and would let us know if the effect of poverty is similar or different across different categories of urban/rural counties.

6. I think the authors can discuss more about limitations of the study: is it possible to have a bias due to missing information? is the measurement of individual characteristics homogeneous across different levels of poverty and urban/rural counties?

7. Figure 1: is it possible to include the confidence interval around the estimates provided in each bar?
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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