Reviewer's report

Title: Treatment outcomes and factors affecting treatment outcomes of new patients with tuberculosis in Busan, South Korea: A retrospective study of a citywide registry, 2014-2015

Version: 1 Date: 28 Aug 2018

Reviewer: Ramnath Subbaraman

Reviewer's report:

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Please overwrite this text when adding your comments to the authors.

This study is a helpful, clearly written, and straightforward retrospective analysis identifying risk factors for poor TB treatment outcomes in this population. The dataset used is substantial in size and quality and the research methodology is straightforward and well-executed. I have no major critical comments or concerns regarding this manuscript.

I have only the following minor comments, which will may provide better clarify to the reader regarding the study findings:

1. The authors make it clear the RR-TB or MDR TB were excluded from this study. How were other forms of drug resistance - in particular INH-mono-resistant TB - handled? It would be interesting to see if INH-mono-resistant individuals had a different outcome in the regression analysis, as they may have been included based on the case definitions here.

2. Please explain the regional deprivation index more in a few sentences in the methods section. In general, what does it represent? Does it use geographic location (e.g., zip code or pin code) as a proxy for socioeconomic status? How well does it serve as a proxy for income differences or socioeconomic status?

3. Please provide a bit more clarity on what it means to be notified twice or more. If I am reading this right, does this serve as a proxy for transfer of care to a different site?

4. Why do you think those in Q5 (the poorest wealth quintile I believe) had lower mortality? This is a surprising finding; however, it might be explained by the higher loss to follow-up in Q5. Could it be that mortality might be low in Q5 because some of the loss to
follow-up might represent deaths? The authors thoughts on this in the discussion would be helpful.

5. In the Discussion the authors cite studies supporting the benefits of DOT. However, given recent systematic reviews - see Tian et al., Pasipanodya et al. and Karumbi's Cochrane review - the authors might be more cautious and acknowledge at minimum that the data on the benefits of DOT (versus self-administered therapy) are debated.

6. Can the authors comment on why proportion cured may have been so much lower in 2015 as compared to 2014?

7. Finally, foreign individuals seem to have poorer treatment outcomes. Could the authors provide more insights into what kinds of countries these individuals come from in general and what their socioeconomic status is like in general? This will help the readers to better picture and understand the implications of these findings.

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

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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