Reviewer's report

Title: Neighbourhood immigration, health care utilization and outcomes in patients with diabetes in a metropolitan region (Canada): a population health perspective

Version: 2 Date: 22 October 2014

Reviewer: Laura Rosella

Reviewer’s report:

This is a study that explores health care utilization among a population of people living with physician-diagnosed diabetes using health administrative databases from 2004-2007 and explores variation with respect to neighbourhood immigration and deprivation derived from Census information.

Major Compulsory Revisions

1. The risk of diabetes and its relationship to diabetes is complicated by the fact that there are certain ethnic groups that are at higher “biological” risk of developing diabetes (for example, First Nations, South Asian, Latin American). Since the authors are using neighbourhood socioeconomic status defined by the census, they do not have critical information to provide context to this issues. In particular data on time since immigration and ethnic composition are critical data elements that are not included. The authors should define this limitation, and the omission of these data, on the results of this study.

2. The validation for the NDSS algorithm by Lix et al is not the most appropriate validation study, given self-report was used as a comparator. Please review the paper by Hux et al. Diabetes Care. 2002 Mar;25(3):512-6, which uses medical charts as a gold standard for the ODD (the Ontario version of the NDSS), as an appropriate validation study. Given that the ODD algorithm has been chart-validated, comment on the performance of the algorithm used in this study (which was slightly different) compared to that chart validated definition.

3. It is important to note that the NDSS definition is “physician diagnosed diabetes” and does not capture individuals who did not have a health care encounter related to diabetes. As such patients who are undiagnosed and potentially underservices (such as immigrants) may be under-represented in your sample, resulting in a selection bias. Please comment on this limitation.

4. Patients were selected between 2004 to 2007. It is unclear why only 2 years of follow-up data were included (this should be explained). Further, instead of using any event in the 2 years following, why not use all the data and take a survival approach? Further, without a survival approach the results have the potential to be affected by survivor bias.

5. The authors state that not distinguishing between incidence and prevalent
diabetes is not a limitation, but I have some concerns with this. In particular, it would be fairly straightforward for the authors to perform a sensitivity analyses according to ‘incident-only’ or ‘prevalent-only’. In addition, other important indicator would be the time since diagnoses, since we know that influences outcomes, and also relates to if the case is a prevalent or incident case.

6. The use of multilevel models is not entirely clear in the way the results are presented. The authors should clarify this presentation and consider reporting ICCs.

7. The Charslon-index (or the D’Hoore) used in this study may not be an appropriate discriminator for co-morbidity or health utilization, given that it only captures major health conditions. The authors consider a co-morbidity measure that is more refined, particular one that captures health events outside hospitals (such as John Hopkins ACGs). Further, I did not see the co-morbidity score appear in any of the descriptive or regression tables.

Minor Essential Revisions

1. I suggest removing the term “diabetic patients” and replacing with “patients or people living with diabetes”. The term “diabetic” should not be used as a noun, as indicated by the American Diabetes Association.

2. Ensure that the titles accurately reflect the content. For example table 6 should include details on the type of regression model where those estimates are generated from.

3. There are too many tables/figures. The authors should considering presenting the results more descriptively and/more move tables/results to the appendix. The maps and deprivation index figures should definitely be moved to an appendix since they do not provide results, only visual representations of the exposure.

4. If possible, include an interpretation footnote for the “scores” presented in table 3 (i.e. immigration score) so the reader can interpret what these values mean.

5. For the linear trends in table 4 the authors should describe the statistical model used in the footnote (i.e. linear regression) and should more clearly specify the outcome.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests