Reviewer’s report

Title: Type 2 diabetes prevalence varies by socio-economic status within and between migrant ethnic groups: analysis and implications for Australia

Version: 1 Date: 3 September 2012

Reviewer: Dianna Magliano

Reviewer’s report:

This manuscript describes the prevalence of type 2 diabetes in different ethnic groups compared to Australian born individual and determines whether higher prevalence can be explained by socio-economic status. It uses NDSS data and Victorian census data.

I think it is a nice piece of work. General and specific comments are listed below.

Major compulsory revisions

1. I find the whole manuscript unnecessarily long-winded and speculative.
2. Sometimes the language used throughout the manuscript is not sufficiently explicit. For example: “Effects of SES” were explored.

Minor essential revisions

1. The time frame for which the prevalence of diabetes refers to should be listed in the Abstract.
2. The word ‘international’ should be dropped from the abstract.
3. Round all prevalences to one significant figure.
4. The phrase “in both sexes”, should be replace with “in the total population”.
5. The word “considerable” is often used to describe results. I don't know what this means and should be replaced or deleted.
6. The conclusions in the abstract are too strong. This study has done no work on the potential implication of the differences they report and therefore the conclusions should be toned down.
7. How do the authors know that the participants with missing postcodes are not special in any way? Is there any bias here?
8. The authors could use NDSS registration date as a surrogate for diagnosis date and then work out age of diagnosis of diabetes. I know there is a very good correlation between the two variables.
9. Although reading this section several times, I cannot understand how the merged dataset was created. I am clearly missing something here. How did you prevent duplication of individuals in the NDSS and the census data? A diagram may help here.
10. How plausible is the assumption that country of birth is missing completely at random. What is the evidence for this?

11. ‘Language spoken at home’ should not be used and no data should be presented on this variable. The number of missing is just too great and it makes no sense to publish such data when there is such uncertainty.

12. The discussion is too long and unfocused.

13. My personal experience with the NDSS is that it represents a biased sample. Those with diabetes on the NDSS appears to be more severe.

14. The last sentence in the third last paragraph of the results need re-writing. I am not sure what it means.

15. I am not convinced how relevant the findings are outside Australia and the text relating to that could be edited.

16. This paper explores area-based SES and does not address individual-based socioeconomic position based on income and education. The discussion could include some text around this.

17. It is also my personal view garnered from discussion with the health department in the Northern Territory that indigenous Australians, especially those living in the NT refuse to register on the NDSS. Could this be the same in Victoria? The prevalence reported here is extremely low and this could be why. A review of the prevalence of diabetes in ATSI has been recently published and maybe used for comparison. (Minges et al DRCP, 2011).

Discretionary revisions

1. The authors could also code the participants for ARIA (area of remoteness) and include another marker of SES into the manuscript.

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

Quality of written English: Acceptable

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

Declaration of competing interests:

I declare that I have no competing interests' below