Author's response to reviews

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

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Author's response to reviews: see over
Dear Dr. Jesus M. Garcia Calleja,

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

Thank you for the useful feedback regarding our manuscript and for the opportunity to revise the paper for further consideration by BMC Public Health. We are pleased to present an updated version and our responses to each of the reviewer comments are outlined below. In particular, given the issues that Reviewer 2 raised with regard to the meaning of ethnicity and the classification of ethnic groups, the manuscript has been reworded to emphasize that the comparisons being made are between immigrants from various regions and the Australian-born population. Hence, where the manuscript previously referred to ethnic groups, the term ‘immigrant’ is now used - we agree that this is more appropriate, as the ethno-specific variable being analysed is country of birth. We acknowledge that immigrant status is but one possible component of the multi-dimensional ethnicity construct, and that measurement of ethnicity is fundamentally difficult. The issue of defining ethnicity in the Australian health context is in fact the focus of a separate manuscript under review elsewhere.

As a result of this change in focus from ethnicity to migrant health, the manuscript has been revised extensively. Many of the references have also been changed to reflect this.

We thank you for your consideration of our manuscript, and look forward to hearing from you.
Reviewer's report:
Reviewer: Dianna Magliano

Major compulsory revisions

1. I find the whole manuscript unnecessarily long-winded and speculative.

RESPONSE: The manuscript has been revised extensively and shortened. The discussion has also been modified to focus more on interpreting our findings and less on the potential (speculative) implications.

2. Sometimes the language used throughout the manuscript is not sufficiently explicit. For example: “Effects of SES” were explored.

RESPONSE: This statement has been removed from the Abstract and replaced with a sentence specifying that the distribution of type 2 diabetes by SES was examined.

Minor essential revisions

1. The time frame for which the prevalence of diabetes refers to should be listed in the Abstract.

RESPONSE: It is now noted in the Abstract that the prevalence estimates are for January 2010.

2. The word ‘international’ should be dropped from the abstract.

RESPONSE: The word international has been removed.

3. Round all prevalences to one significant figure.

RESPONSE: Prevalence rates and prevalence odds have been rounded to one significant figure.

4. The phrase “in both sexes”, should be replace with “in the total population”

RESPONSE: Where ‘in both sexes’ has been used to refer to similar results from sex-stratified analyses, the phrase has been reworded to ‘for both males and females’. This is to highlight that the results are sex-specific.

5. The word “considerable” is often used to describe results. I don't know what this means and should be replaced or deleted.

RESPONSE: The text has been modified wherever the word ‘considerable’ was used, and the word has been either 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.
RESPONSE: The conclusions section of the Abstract has been modified. It now summarises the key findings of this study – that there are large socio-cultural differences in the distribution of T2DM, and that migrant groups have higher prevalence of T2DM than do Australian-born people, across all socio-economic levels. The language has been modified to note that this health gap might have implications for public health efforts in Australia. We acknowledge that this work comprises descriptive epidemiology and that the potential implications of the differences reported are beyond the scope of this paper. Nonetheless, it should be noted that such implications may well exist.

7. How do the authors know that the participants with missing postcodes are not special in any way? Is there any bias here?

RESPONSE: Those with missing postcodes were excluded as it was possible that they were not Victorian residents. There were also some NDSS registrants with T2DM who had a postcode recorded but there was no corresponding IRSD score in the Australian Bureau of Statistics classification, and so they could not be assigned to an IRSD quintile. In total, 890 NDSS registrants with type 2 diabetes and 9069 people in the total Victorian population were excluded for these reasons. We don’t have any evidence that they were not special in any way. However, they comprise only a very small fraction of NDSS registrants with T2DM (0.5%) and of the total population (0.2%). As such, the effect of excluding them on the results would be negligible, and we feel it is unlikely that this would have introduced marked bias into the analysis.

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.

RESPONSE: In this study, we have used age at the time of data extraction. Alternate age-related variables were considered during the conduct of this study. As noted in the manuscript, self-reported age at diagnosis was missing for 39% of NDSS registrants, and so could not be used. The difficulty with using age at diagnosis is that the NDSS data file would be stratified by duration of T2DM while the census data would be stratified by chronological age. The two files would have different types of data and one could not be used to correct the other for age. We need to use the same data type to age-adjusting the NDSS data to match the Victorian population and so must use chronological age at download. We believe that the differences in age at data extraction that exist between immigrant groups still reflects the differing epidemiology between groups, and have carefully discussed the interpretations of these findings.

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.

RESPONSE: We regret the misunderstanding that has occurred due to use of the
term “merged”. We merged the data only in the sense that we subtracted the T2DM totals from the population totals to estimate the number without T2DM. The relevant paragraph in the manuscript has been rewritten to clarify this.

10. How plausible is the assumption that country of birth is missing completely at random. What is the evidence for this?

RESPONSE: As we don’t have any evidence for what the distribution of missing regions-of-birth might be, we used multiple imputation to estimate missing data based on the observed distribution. Ten datasets were imputed, for men and women separately, and the analyses of these gave similar results to our analyses ignoring these data, giving confidence that there is no selection bias inherent in ignoring the missing data. The details are provided in the text.

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.

RESPONSE: Reference to the ‘language spoken at home’ variable has been completely removed from the manuscript.

12. The discussion is too long and unfocused.

RESPONSE: As noted above, the entire manuscript has been shortened, reworked and the discussion focussed.

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.

RESPONSE: The text acknowledges that certain groups may be under-represented in the NDSS database, including those managed by diet and lifestyle rather than pharmacologically.

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

RESPONSE: We have rewritten the paragraph and hope that its point, that there were regional differences observed in the IRSD scores for most but not all regions, is now clearer.

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

RESPONSE: The sections of the abstract and discussion that reflect on the implications of this work have been rephrased, with an emphasis on the local Australian relevance. Reference to international implications have been removed.

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.

RESPONSE: The discussion has noted the limitations inherent in use of an area-based SES measure, and acknowledged that area-based SES may well differ from individual SES. Referencing recent Victorian work, the discussion has been expanded to state that diabetes prevalence in Victoria has been found to be associated with some individual-level SES measures.

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).

RESPONSE: We acknowledge the importance of analysing and reporting on Indigenous data where possible. The number of Indigenous registrants in our data is very small, rendering the study underpowered and the prevalence estimates obtained are consequently extremely low. We believe it is best not to report these estimates given the statistical power issues.

Additionally, as the focus of this paper has been changed from ethnicity to migrant health, Indigenous Australians are not considered separately. The Indigenous-specific results have been removed.

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

RESPONSE: We agree that the area of remoteness measure (ARIA) is a useful marker of SES and accessibility. However, the proportion of people with type 2 diabetes outside the metropolitan area in outer regional and remote areas is small; when further subdivided into the ten regions of origin, the numbers are likely to be too small to allow an adequately powered analysis.

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
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: 1 October 2012

Reviewer: Maximilian de Courten

Reviewer's report:

The manuscript by Abouzeid at al reports on the diagnosed type 2 diabetes prevalence in Victoria amongst people registered in the NDSS scheme and having information on their country of birth.

The researchers set out to investigate variations in (diagnosed) diabetes prevalence according to ethnic, socio economic and other demographic variables amongst the population in Victoria. The title of this manuscript is using the term migrant ethnic groups indicating that ethnicity was defined in this research according to the country of birth as listed in the NDSS database. Furthermore, countries of birth were re-categorised based on geographic proximity to result in nine very different regional groups. For instance it can be argued that the group of North West Europe, which has been traditionally one of the strongest regions for immigrants in Australia could be made up of people having the same ethnic origin than those in the group entitled Australia which includes residents born in Australia regardless their ethnicity. This highlights a problematic intersection between the various dimensions or aspects one can define ethnicity: along geographic origins, cultural or linguistic characteristic’s, or genetic concepts. All of these are mentioned at various places in the manuscript.

Therefore, the research findings and their interpretation need to be carefully examined and structured according to the (dimension of) definition used - because of the overlap in definitions. From the method section it appears that the key characteristics for the research was whether members of the NDSS were born in Australia or overseas. Hence the focus should be on immigrant status and the research centred around region of immigration, lengths of stay in Australia, acculturation, access to health services, and other factors which could determine disease prevalence amongst immigrants. And the discussion hence should focus around those factors. As comparison population then features those born in Australia; and those not having the information on their country of birth available have to be excluded from the analysis: around a third of the data, which needs to be investigated whether that could constitute a selection bias.

RESPONSE: We agree with the reviewer that ethnicity is a multi-dimensional construct, and that country of birth is but one aspect of ethnicity. The difficulties with defining and measuring ethnicity are well documented in the literature, and the specific issues of measuring ethnicity in health databases in multicultural Australia are the focus of a separate manuscript under review elsewhere (Abouzeid et al., forthcoming). We agree that the focus of the comparisons in this paper are in fact better defined as based on immigrant status and region of origin rather than ethnicity. The manuscript has been extensively revised accordingly, and the word ‘ethnic’ has also been removed from the title.

Those with missing country of birth have been excluded from the analysis, and
results of multiple imputation suggest that it is unlikely that excluding this group generates a selection bias. As there is no evidence for what the distribution of missing regions-of-birth might be, we used multiple imputation to estimate missing data based on the observed distribution. Ten datasets were imputed, for men and women separately, and the analyses of these gave similar results to our analyses ignoring these data, giving confidence that there is no selection bias inherent in excluding those with missing data. The details are provided in the text.

The other approach to ethnicity could come from analysing the language speaking at home is indicated amongst the NDSS registrants: correctly the researchers point out however that over 70% of individuals in that database do not indicate what language is being spoken at home, rendering this approach probably useless.

RESPONSE: We have removed all reference to language spoken at home from the manuscript, given the large number with missing data.

And underlying as well as intersecting with these different concepts of ethnicity are factors such as age, sex, and other determinants of diabetes including socio-economic status, which by itself is another composite and proxy variable. The second paragraph on page 6 is mentioning that "some facets of social economic status" are influencing the prevalence of diabetes, without specifying what facets the researchers had in mind. These should be listed and included in the logic model (see below).

Given these complex and ill-defined concepts it would be beneficial for the reader to learn more about the logic model underlying this research illustrating how the different variables in their various definitions putatively relate to each other. With that a clear description of the definitions used in the pragmatic application to the available data should be formulated much clearer in the method section of this paper.

RESPONSE: Recognising that age, sex and SES may modify the association between immigrant status and diabetes prevalence, we have tested for such interactions and the results are reported. As SES modified the association between region of birth and T2DM prevalence for some regions, age-adjusted prevalence rates are presented for each region, stratified by SES.

As the focus of the manuscript is now clearly immigrant status and region of origin, we believe that the definition of the ethnicity-related parameter under consideration is now clearer.

Great care also has to be given to the fact that only people with diagnosed diabetes made it into the dataset, and the rate of diagnosis might very well be different amongst the different ethnicities. This should be flowing into the discussion of the findings.

RESPONSE: This point is now noted in the section on strengths and limitations.
The researchers indicate in the first paragraph of the method section that approximately 80 to 90% of Australians with diagnosed diabetes are member of the registry, a number which might be much lower amongst immigrants as it is lower amongst the ATSI population (as correctly highlighted by the authors on page 18 of the discussion section).

RESPONSE: This suggestion has been adopted – it is now noted in the discussion that among those with diagnosed diabetes, the rate of registration with NDSS may vary between migrant groups.

The last sentence of the background section on page 7 is summarising the research approach is to say "we investigated ethnic, socio-economic and other demographic variations in Victorian T2DM prevalence..." Given the above, the reviewer would prefer the use of more narrow defined terms: the ethnicity for instance means immigrant status and diabetes prevalence is limited to those having the disease diagnosed and accessing the registry.

RESPONSE: The last sentence of the background section has been changed to note that the study investigates the prevalence of diagnosed T2DM in migrant and socio-economic groups in Victoria, using diabetes data for those with diagnosed disease registered with the NDSS. It is acknowledged in the limitations section of the discussion that those not registered with the scheme are not captured in these estimates, and that there may be socio-economic and cultural differences between those with diagnosed disease who are and are not registered with the NDSS.

Minor essential revisions:
1. Throughout the abstract the word “diagnosed” should be inserted in front of diabetes where it is actually meant.

RESPONSE: Suggestion adopted.

2. The background section introduces the concept of ethnicity with the help of the thrifty genotype hypothesis which is not only out-dated but has even been refuted by its original author. A more current and complex definition of ethnicity needs to be given.

RESPONSE: The first paragraph of the introduction has been rewritten to incorporate more contemporary theories of the complex aetiology of T2DM and metabolic disease in general. The paragraph now acknowledges the important interactions between genetic, developmental, evolutionary and environmental influences on disease risk. It uses the Predictive Adaptive Response theory to highlight how exposure to intrauterine deprivation may trigger epigenetic and developmental programming that confers a survival advantage in a deprived postnatal environment, but increases risk of metabolic disease in the event of postnatal excess, as may occur with some migrants to developed countries being exposed to obesogenic environments.

3. The last sentence of the first paragraph on page 6 is stating that the diabetes
prevalence amongst migrants may differ from that in the country of origin. This is central to the research presented and therefore the underlying factors need to be spelled out and discussed how they apply to this analysis.

RESPONSE: This idea is explored in detail in the discussion. It is also noted that for some of the migrant groups, the associations between SES and diabetes prevalence differ from the general patterns in the regions of origin. The nature of the data does not permit any further analysis or inference of causation. However, some factors possibly driving these observed phenomena are described and findings from the international literature cited.

4. The statistical analysis section describes in italic contrasts having been used without the reviewer being quite sure what it means.

RESPONSE: This sentence has been rephrased to explain that the contrasts command is a means of reducing the number of pairwise comparisons to control Type 1 errors. This technique was applied to test the significance of T2DM prevalence based upon the category of IRSD quintile.

5. The fact that on a third of registrants the information on country of birth was unknown (methods section) needs to be analysed in terms of how this could bias the findings.

RESPONSE: As noted above, those with missing country of birth have been excluded from the analysis. The results of multiple imputation suggest that there is no bias introduced by excluding this group.

6. Results: the crude prevalence data amongst ATSI with 1 % seems to be surprisingly low and warrants explanation/discussion.

RESPONSE: As noted above, all Indigenous analyses have been removed due to uncertainty surrounding the estimates.

7. Discussion: the statements that earlier screening for diabetes would be warranted for some ethnic groups has to be seen in the light of differences in risk factors amongst them. It is well-known that obesity rates are much higher in people from the Pacific, and obesity is already one factor within the screening approach. Once taken into account is the then still needs to single out certain ethnicities?

RESPONSE: We acknowledge the reviewer’s point regarding the importance of screening based on a number of risk factors, including obesity, and that risk factor distribution does differ between migrant groups. Unfortunately our data do not contain any information on body mass index, and so adjusting for the effects of obesity on diabetes prevalence among migrant groups is not possible. The available data suggest that for some migrant groups, there is a high proportion of younger people with diagnosed diabetes, and thus we conclude that screening should commence earlier in these high-risk groups.
8. Recommendations: given the inherent difficulty in defining ethnicity, the researchers should include from their experience some recommendations as to how to improve research in this field.

RESPONSE: We believe that this issue is now clarified with use of region of birth as an indicator of immigrant status, rather than as a proxy measure of ethnicity. As noted above, the difficulty in defining ethnicity and recommendations for how to improve work in this arena is the focus of another forthcoming manuscript.

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