Reviewer's report

Title: Type 2 diabetes attributed to lower educational levels in Sweden: A burden of disease study

Version: 1 Date: 29 April 2011

Reviewer: Rasmus Hoffmann

Reviewer's report:

The topic of the article, the use of Comparative Risk Assessment methods to estimate the burden of diabetes due to low educational level in Sweden, is clear and interesting. Generally this approach is worth being used, shown and published more often. However, I think that the data used here are not sufficient to provide interesting and valid results, nor are the methods absolutely new or presented on such a high quality level that the article would be worth publishing even without valid results, for example as an illustration of a new method. I see major compulsory revisions needed in order to let this article be an advancement in the field of CRA. See details below:

Major Compulsory Revisions

1. A causality concept and evidence for causality should be discussed in the introduction (e.g. common causes, contributing causes, necessary causes etc.) All PAF calculation need to assume causality between risk factor and outcome measure. If this is wrong a PAF calculation becomes a misleading calculation exercise.

2. The lowest age group should be deleted: As it is now, this study can not give valid results for the lowest age group because of misclassification of persons who did not finish their education. Moreover, I recommend that the age dimension will be collapsed altogether because age-differentiation can only be made for the prevalences but not for the RR. The RR is not only assumed to be the same for all ages (which is probably wrong), it also comes from studies that look at different ages. I see that you partly compensate for this by estimating heterogeneity with random-effect models and sensitivity analysis, but the data basis for the RR stays weak (see below).

3. A related recommendation is that you provide confidence intervals for your results, which, I guess, will be very wide and highlight the problems with the data basis. This can be done with bootstrapping based on the standard errors of the RRs and prevalences. Both might be available to you.

4. p.11, l.6: no, there have been studies before (e.g. several papers by Tony Blakely and Nick Wilson around 2005/2006 who used population attributable risk percent (PAR) to study the impact of smoking on health inequalities, or Mackenbach & Kunst (1997 in SocSciMed) who used PAR as a measure of social inequality in health. Although these studies do not have exactly the same
outcome as yours (they look at health inequality and not at burden of disease due to low education) they comprise your calculations and partly even go further: in order to estimate the impact on health inequality of a risk factor, you need to use education as a risk factor for the PAF calculation and apply this to each social group.

5. p.15: low and middle income countries seem to be irrelevant for this study and should be left out of the conclusions. The same is true for the conclusions on the international level in the last sentence. This study has a national perspective on Sweden and the conclusions should rather come from the results: total order of magnitude for the burden, gender- and age differences.

To summarize the comments above: Table 1 shows how disparate the data basis for this study is: The studies chosen to calculate a pooled RR are incomparable in region, period, age range, gender, educational categories and control variables. Nobody can expect that all these dimensions are perfectly comparable in such a review but here the data basis suggests a too low validity of the results. To come up with reliable estimates for the impact of a risk factor, a better review is warranted. I wonder if you can use more studies from your review from 2001. Given that the use of social variables in CRA measures are not really new (although it has been rarely applied and is certainly interesting), the paper in its current form is neither a method paper nor a research article with valid results. At least one of these aspects should be improved, by either considerably improving the methodological insights (biases, assumptions of the PAF approach etc.) or by making the results more valid and interesting, e.g. by offering confidence intervals that would contribute to a saver interpretation of the few differences you can offer (gender, age and between measures).

Minor Essential Revisions

6. p.5, top: the justification given in the GBD literature for not including education is correctly referred, but I think an additional and a clearer argument is that causality between education and health has not been proven yet. Related to this: if we increase education in a population we can not simply assume that the effect of education will be the same; there might be diminishing returns of education because it not only has a health-effect on its own, but also a hierarchy effect within a social structure.

7. The last sentence of the CRA section is too vague: “incorporate (in)to” is too vague as a method description. Also the calculation of YLL, YLD and DALY could be explained in a few sentences: How did you get from the PAF to the life-table measures?

8. Next to the problem of having a very different choice of control variables across studies (Table 1), there is evidence that using adjusted RR in PAF calculations can also lead to substantial biases compared to the correct method of stratifying the analysis by category of the confounding variable (Darrow & Steenland 2011 in Epidemiology).
9. In the method section it remains unclear whether the same RRs that you obtained for the incidence of diabetes between educational groups are also used for mortality differences. This would not be a trivial assumption. Furthermore, which causes of death are studied? Only diabetes or also other causes that are influences by diabetes? (Or all cause mortality?) Are these causes competing risks?

Discretionary Revisions

p.5, l. 6: “shown”
l. 11: no comma
p.6: what is the theoretical minimum exposure for education?
p.7, l. 16: after mentioning that you use the most-adjusted, please refer to table 1.
p.8, l. 16: explain Dismod II
l. 20 “is” at the wrong place
p. 9 please provide the formula in better print quality.
p. 10, last but 3 line: throughout the paper the age range 15-19 vs. 16-19 is not consistent.
p. 12 bottom: the comparison between the original study and “this study” can be made clearer.
p. 13, l. 7: this is an unclear formulation; how can one third be unequally distributed?
l. 11: excess mortality can be 70% but a RR should then be 1.7.
p.14, l.5: shouldn’t it be “under”estimated contribution?
l. 18 “… their educational ambitions and their socioeconomic position”
In reference 2 no authors mentioned
In reference 13 The Lancet
For Table 1 the title or the column headers should say what kind of RRs are shown.
Maybe Table 3 is better placed after Table 1.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.