Reviewer’s report

Title: Association between socio-economic status and hemoglobin A1c levels in a non-diabetic primary care population.

Version: 1  Date: 30 August 2013

Reviewer: Sara Willems

Reviewer's report:

Thank you for sending me this study. This is a potentially interesting study addressing a clinical relevant topic: including SES to clinical decision tools.

I would like to present the authors some issues to came up when I was reading this paper:

Minor essential revisions:

The abstract:
The abstract is not very clear to me. Especially the purpose of the study contains little information. This abstract would not motivate me to read the full paper.

Introduction:
Based on this introduction it is not clear to me what the point is of this study. The introduction does not convince me this is an important study. This part reads as a number of independent paragraphs where the reader is asked to “jump” from one topic to another. Providing a clear “red thread” for the reader, building the introduction up from a clear problem to the research question is essential.

Major Compulsory Revisions:

Methods:
The methods section misses more detailed information.

E.g. which are the covariates taken into the model? Why not including other indicators such as physical activity, smoking, etc.? Or are they included? The method section in the abstract seems to indicate they are (“we adjusted for covariates associated with an increase in the risk of incident diabetes”) but I cannot find them in the tables nor in the text.

Is this method suited to answer your research question. I am not convinced it is.

There might be a major selection bias in the patient group under study. I might be possible that there is a clear independent influence of SES on Hbg A1c, so clear that the lowest SES patients are not in the non-diabetic group but in the diabetic group. The low SES patients left are hardly representative for the low SES patients consulting in the practice and for whom the diabetes risk score is calculated.
Would it not be more appropriate to use other study designs such as calculating the diabetes risk score with and without SES for all patients fulfilling the requirements for such a test and see what that gives. I need to be convinced your method is suitable.

Conclusion:
The ecological phallacy – a major limit of this study- is, to my opinion, underestimated in the conclusion. How could this have influenced your results?
I believe the researchers are too strongly convinced that SES should not be included in clinical decision support tools. This cannot be derived so strongly from the results.
I also miss a broader reflection on the importance of knowing the patient’s SES for clinical decision making and/or for the treatment of their diabetes.

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

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
'I declare that I have no competing interests'