Reviewer’s report

Title: Obesity prevalence in a cohort of women in early pregnancy from a neighbourhood perspective

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Reviewer: Lynne C Messer

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BMC Pregnancy and Childbirth
Title: obesity prevalence in a cohort of women in early pregnancy from a neighborhood perspective

This paper sought to explore the association between neighborhood deprivation and obesity among a cohort of pregnant women living in neighborhoods in Sweden. They found that residential neighborhood accounted for a small amount of obesity variability, and the relationship was affected by neighborhood poverty and maternal education.

I thought this was an interesting and well-written paper. I have a few comments, which I think if addressed, will strengthen the paper.

MAJOR COMPULSORY REVISIONS

I was not sure I agreed with the author’s confounder inclusion strategy. Not only does individual-level income contribute to (or the aggregation of which - constitute) the neighborhood-level exposure, but it may be that individual-level income is on the causal pathway between neighborhood-level ses and obesity. For instance, if living in a non-affluent neighborhood affects individual-level income (via job opportunities, social/employment networks, etc.), then including individual-level income represents over-control and inappropriate confounder inclusion.

My second general concern was that I think the authors dealt inadequately with the weight variables collected during pregnancy. The authors seem to want to conclude that the relationship between neighborhood residence and obesity prevalence among pregnant women is somehow representative of the non-pregnant population. I am not sure this is a safe assumption to make.

Further, while women may enter pregnancy obese or overweight, if they attend to practitioner guidelines and gain weight at a slower rate than their non-obese counterparts, then they will appear “on track” for weight gain (even if they gained no weight during pregnancy, because they were already overweight to begin with). I appreciate the authors mentioned this limitation in their discussion, but I think they need to do a better job addressing the relationship between when weight data were collected among pregnant women and their representativeness of the general population (and the conclusions they seem to reach in their study).
When describing the neighborhoods, it would be helpful for you to describe how the neighborhoods changed over time. Also, neighborhoods are not exclusively affluent, medium, or poor but are heterogeneous and comprised of individuals representing all of these conditions. I appreciate the economic status ratio as an exposure, but it is unclear how the authors dealt with the neighborhood heterogeneity.

In addition, it would be helpful to know when in pregnancy the data were collected. I appreciate the authors reported data were collected in the first trimester, but more detail on the timing of data collection would be helpful.

MINOR COMPULSORY REVISIONS

Methods.
On page 4, you refer to “cases.” It is not clear who this refers to.

I am not sure why the authors dichotomized their obesity outcome. It seems – especially given the numbers of women involved – that they could employ a more nuanced outcome categorization (e.g., underweight, normal BMI, overweight, obese).

Results.
To say, on page 9, that a clear gradient was apparent (in the relationship between education and individual-income and obesity) is not entirely accurate.

ANSWERS TO SPECIFIC QUESTIONS

1. Is the question posed by the authors well defined?
2. Are the methods appropriate and well described?
3. Are the data sound?
4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
5. Are the discussion and conclusions well balanced and adequately supported by the data?
6. Are limitations of the work clearly stated?
7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
8. Do the title and abstract accurately convey what has been found?
9. Is the writing acceptable?

The answers to all of the above questions are yes, with the possible exception of question 7. The authors do not note any other work on which they are building, which I take to mean they have not built upon other work, but am not entirely positive.
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.

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