Reviewer's report

Title: Are there differences in mean birth weight between neighbourhoods in a Nordic welfare state: a 10 year cohort study

Version: 2  Date: 25 May 2007

Reviewer: James Macinko

Reviewer's report:

General
The authors have provided a thorough revision and improved many aspects of the study.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. The authors need to lay out more clearly the argument they wish to make at the beginning of the article. For example, in the introduction they discuss the role of stress on birth outcomes, but they do not make a good case that this is the mechanism they think is at work, nor do they actually measure this exposure. As such, the argument becomes diluted. This could be strengthened by making connections to stress several places in the text. For example, is there evidence that people living in multi-ethnic neighborhoods in Sweden actually report experiencing more stress? Surely, there must be some literature on this topic that could be brought into this discussion.

2. There is still some difficulty understanding the importance of the measure of absolute birth weight. The authors could try to make a better case for why any improvement in birth weight is beneficial (or not). As it stands I think most readers would be more worried about thresholds (such as those that define LBW) than absolute weight since such indicators have important implications for adult health. Why is birth weight—in and of itself—an important outcome? This confusion is highlighted in the discussion section where the authors themselves describe the birth weight variation as “rather trivial in magnitude”. As a reader I really wanted to know not whether or not birth weight differed, but whether the likelihood of adverse birth outcomes differed. If this is not possible to measure, then it should be acknowledged and discussed more thoroughly.

3. The policy recommendations (“interventions should be directed to maternity care units in disadvantaged neighborhoods”) seem to be at odds with the study’s findings. If individual-level risk factors associated with the basic socioeconomic status of women in these neighborhoods is driving the poorer birth outcomes, then interventions should be targeted to improving the socioeconomic condition of the women as well as medical interventions. But this is inconsistent with the authors’ conclusions that the Swedish welfare state is working well. These implications need to be thought through more thoroughly and what appear to be contradictions need to be either resolved or acknowledged as such.

4. The argument that foreign-born mothers are visible minorities makes sense if they “look different” from the general population. But the definition is inconsistent. First, it defines visible minorities as those “born in the south of Europe or countries outside of Europe, excluding...” Then it says (page 5 paragraph 1) that “visible refers to non-white immigrants from outside Europe”. Is southern Europe included or not? If so, I had not been aware that Italians or Spanish would be considered “non-white” in Sweden--especially considering that each of these countries is quite multi-ethnic and includes many light-haired, light-skinned people! What is this measure really capturing?

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. The manuscript still suffers from serious English-language deficiencies that have little to do with American versus British usage. There are many typos and some non-standard usage that make the manuscript extremely hard to understand.

2. Bottom of page 4 of the text (page 6 of the manuscript), last paragraph. The categories (e.g. “low, very low of resources and poor neighborhoods”) are very confusing. Maybe they need some commas or other punctuation? The same applies to the categories for bivariate analyses presented on page 8 right before the section “analytic approach”. These groupings are very confusing and could be described more clearly.

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major
compulsory revisions

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

Quality of written English: Needs some language corrections before being published

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.