Reviewer's report

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

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Reviewer: Juan Merlo

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Are there differences in birth weight....
Eva Sellström et al

The authors have performed a very good work and they master the wonderful database they analyse. However, I have a number of mayor (an a few minor) comments.

• In the “Introduction”, 2nd paragraph I read “..used to be ethical very homogeneous…” This expression seems to be an error. Certainly the authors mean “ethnically”

• I do not agree with the approach of the authors in the introduction staying “stress” to be a mayor mechanism behind low birth weight. Even if this was the case the concept of the authors is misplaced. Sweden because of its socioeconomic homogeneity would be the perfect scenario for investigating “stress effects”. Remember that Marmot just use this argument when se observe consistent socioeconomic gradients in health outcomes in the Whitehall study. A cohort that is composed by British male civil servants with an acceptable socioeconomic standard of life and therefore - Marmot argues- those gradient should be originated by the relative status in the social hierarchy and the stress that a low status occasions, rather than by material deprivation. On this background the assumption of the authors concerning low stress effect in Sweden is questionable. Rather, the relevance of this study is that it supports that neighbourhood related stress mechanisms are not a plausible explanation behind variance in birth weight.

In any case I would like to add a comment...

I do not question that “stress” may impair intrauterine growth but an alternative hypothesis could be that “stress” is only a marker of impaired material circumstances of the mothers and this material deprivation condition a pattern of intrauterine growth that provides advantages on a Darwinian perspective but that today - with a much larger life expectancy - lead to chronic disease later in life. The presence of “psychosocial stressors” at the neighbourhood is only a proxy of material deprivation which in turn compels a graded deterioration of the construction, maintenance and access to amenities which hinders healthy behaviours. This question is very relevant because the political actions to promote health are very different. According the psychosocial approach you can promote health by making the neighbourhood more beautiful and cleaning the streets... You could even find a medication that compensate the negative effects of chronic stress and help the individual to fit their living conditions.

Under the material perspective, however, you need to consider the structural determinants that condition both impaired residential circumstances and low birth weight. That is, instead of blaming the neighbourhoods (and the neighbours) you should blame the societal system that allows the existence of deprived neighbourhood.

• A main hypothesis of the author is that the ethnical heterogeneity at the neighbourhood is a source of stress. Do the authors know some study supporting this idea? In fact, it could be possible that being an emigrant minority in a neighbourhood of Swedish majority is more stressful tan living in a neighbourhood with many emigrants. Have the authors explored this circumstance?

• The concept of “visible minorities” originates from Canada and it has been recently introduced in Sweden. I wonder if this concept is really well established as the authors stay in the manuscript or rather the opposite. Have the author evidence of its use from other countries moreover Sweden and Canada?

In my opinion, the concept of “visible minorities” is inappropriate for epidemiological and ethical reasons.
Assuming the concept was useful the use of administrative registers for identifying “visible minorities” provides information of low validity. In fact, there are USA Americans of African, Asian and South European origin. There are also many Swedes that do not fit the stereotype of pink skin and blond hair. In your definition of “visible” you include South Europe and you stay that “visible” refers to the non-white immigrants from outside Europe… does it mean that people from Italy or Spain become non-white immigrants from outside Europe?

The risk of misclassification of the exposure is, therefore, high. May be, if there was a register on people’s “visibility” the “visible minorities” would be better identified… but this register would be immoral. “Visible minorities” is such an oversimplification that I can not find epidemiological arguments to support its use.

Using the “visible minorities” concept stigmatized the individuals by irrevocable characteristics attributed to one’s country of birth. These characteristics have obscure theoretical support other than simplistic ethnocentric considerations. The use of the “visible minorities” concept by societal authorities and researchers hinder the integration of the emigrants in the society and promotes segregation by justifying their stigmatization. Rather than on structural circumstances the focus is moved to the “visibility” of some people with is a teleological paradox as the aim of societal authorities and researchers - including the authors - is to promote health and integration.

I suggest dropping the “visibility” classification of people and for operational and analytical purposes to use of the World Bank Classification of countries. Rather than “visibility” criteria the World Bank use economic information on the gross national income (GNI) per capita of the countries. Rather than “visibility” criteria the World Bank use economic information on the gross national income (GNI) per capita of the countries ( see link below) http://web.worldbank.org/WBSITE/EXTERNAL/DATASTATISTICS/0,,contentMDK:20420458~menuPK:64133156~pagePK:64133150~piPK:64133175~theSitePK:239419,00.html

Alternative geographical criteria like (i) Sweden, (ii) North Europe, (iii) rest of Europe and (iv) other countries - being hugely coarse - is a practical categorisation

• The 3rd paragraph in the “Results” is confusing. It seems that the slope of year of birth varied at the neighbourhood level. Therefore you have certainly calculated the neighbourhood variance as a function of “year of birth”. Nothing is said on this aspect in the “Methods” part of the manuscript…

Also you stay that the ICC changes by year of birth (which is reasonable if the slope varies between neighbourhoods) but the values you give seems flawed “The ICC increases from 0.62 to 1.71” This means from 62% to 171%, which is impossible. You should indicate 0.62% and 1.71% (which are very low… see comment under)

• Also the very low neighbourhood (relative to the individual variance) makes without sense the study of the “proportional change in variance”… As Judith Singer commented(1) “You can explain a lot of very little…”

The most relevant conclusion id that the neighbourhood level can be disregarded in the Swedish public health policy as a source of inequalities in birth weight. However, as you known even very small variance can be enough to detect associations (that has a more “causal” interest)

• In the 3rd paragraph of the discussion you explain very interesting ideas concerning the use of variance analysis in public health. This ideas has previously described and I suggest you refer to this source (2) (3-6)

• Concerning the “Discussion” part, another caveat wit this study is the fact that there are worldwide differences in birth weight so comparing birth weight between neighbourhood with different ethnical composition has less to do with the effect of the ethnical composition of the neighbourhood. The ethnical composition of the neighbourhood is related to birth weight because it is simply expression of (residual) compositional confounding. Maybe a way of accounting for this fact is by standardizing birth weight by country of birth (a difficult task). Otherwise, the lower birth weight in neighbourhoods with Swedish minority tells nothing on “neighbourhood effects” even when adjusting by maternal height.

• This study would improve if it focuses on socioeconomic determinant of birth weight (after showing that - in Sweden - neighbourhood effects are minor) and drops the “ethnical approach”.

• Finally, observe that in epidemiology “cohort” study often refers to a defined group of people (the cohort) that are followed over time (and compared with another group who are not affected by the condition under investigation). The present study is a “cross-sectional” analysis covering a 10-year period

• The abstract must be modified according the mayor comments I have raised above.

1. Singer JD. Using SAS PROC MIXED to fit multilevel models, hierarchical models, and individual growths

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: Yes, and I have assessed the statistics in my report.

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