Author's response to reviews

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

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Response to reviewers
I have responded to each reviewer and commented in order of appearance.

Response to Juan Merlo
1. The introduction is rewritten in order to clarify our theoretical background. We are mentioning stress as one possible mechanism through which neighbourhood variations work, but putting emphasis also on discrepancies in health-related behaviours.

2. I acknowledge the critique from the reviewer regarding the use of ethnic composition of the neighbourhood as a neighbourhood level determinant. Therefore I have elaborated the models without the neighbourhood ethnicity variable and instead added n-hood economic status variables in all models. Accordingly the text is adjusted throughout the manuscript.

3. The idea of introducing a random slope in the model was only the fact that we had ten years of observation and a way to handle any possible change. I adjusted the text in the Method section as follows: The variables included one observation for each year. Therefore we calculated the neighbourhood variance as a function of year of birth:

4. Added % in the third paragraph in the result section.

5. References are added referring to the discussion on the importance of reporting ICC.

6. Added a section in the Discussion: The study design is cross-sectional. However, data on exposure to a neighbourhood context preceded the outcome under study. At the time of giving birth the women had all lived in their neighbourhood for at least 1 year and most of them for a longer period of time. The validity of these exposure data can therefore be considered good. Furthermore, the study relies on register data. The Medical Birth Register has been validated and found to have good validity. Also, registers linked to this
register are generally considered to be of good quality.

7. Adjustments are made to the Abstract.

Response to James Macinko
1. The introduction is rewritten in order to clarify our theoretical background. We are mentioning stress as one possible mechanism through which neighbourhood variations work, but putting emphasis also on discrepancies in health-related behaviours.

2. We added a section in the Method section in order to make this point clearer: In this study we have used mean birth weight as outcome variable. Many studies rely on a dichotomous measure of low birth weight, which is defined as a birth weight less than 2500 g. This is a convention that dates back to 1919. More recent research has shown that low birth weight is a threat to infant health especially if it is a result of intrauterine growth retardation. Furthermore, perinatal and infant mortality is lowest in births >= 3500 g. Besides, low birth weight is a rare outcome which makes it more difficult to detect any neighbourhood variation. In the analysis, we controlled for gestational age. Thus, to use a linear specification of the outcome has a greater possibility to capture potentially critical variation across the distribution of birth weight that cannot be explained by length of gestation.

3. This is a correct comment and we removed the following statement from the Discussion: Our findings indicate that interventions should be directed to maternity care units in disadvantaged neighbourhoods and target maternal risk factors influenced by maternal stress.

4. As suggested by the other referee we only used neighbourhood economic status as a neighbourhood level determinant and the models are elaborated without neighbourhood ethnic composition. Accordingly, the entire manuscript is adjusted.

5. Language check carried out by professional interpreter.

6. I have adjusted Table 1 and 2 in order to make the coding and recoding procedure of n-hood economic status simpler to follow.

Finally, I want to thank both reviewers for highly valuable critical comments and hopefully the paper has improved enough to be published.