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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Version: 2 Date: 20 April 2007

Author's response to reviews: see over
Response to reviewers
I have responded to each reviewer and commented in order of appearance

Response to Juan Merlo

Introduction

1. The suggestions for improvement of the paper in the referees’s paragraph 2, we have tried to meet in the Introduction. So, the research questions follow from the theoretical framework outlined in the introductory section of the paper.

Method
The Method section is also rewritten in order to make a clearer description of the database and of the measurement of included variables. The Medical Birth Register provides information on marital status but no other socio-economic indicators are present. Actually, information on occupational status is lacking in about 25% of the women (1). The Multi-Generation Register provides information on country of birth of the mother. The variable is used to control for maternal ethnicity. Bibliographical references are checked. An extended description on neighbourhood variables is provided.

Answers to the referee’s further comments regarding the Method section:

I agree with the referee that inclusion of a “better” variable for individual ethnicity would have been more optimal. This is unfortunate, as it means that the confounding control is not satisfactory, leading to potential bias. We have discussed this in the Discussion section and mean that if we had a better variable to include in the multilevel model; more of the individual level variation in birth weight could have been explained. We have no reason to expect that the neighbourhood variables impact should be increased by a better confounding control here. We have tried to clarify this in the Discussion section. Further, maternal height predicts birth weight, and foreign born women can be expected to be shorter than Swedish born women. Spencer and Logan argue that maternal height can not be viewed as a purely biological factor that predicts birth weight of the infant. Rather, maternal height is a determinant intertwined with social factors and is frequently retained in multivariate analysis of birth weight while social factors fail to retain. We do control for maternal height as well as maternal ethnicity (i.e. foreign- or Swedish-born). In order to clarify this we have extended the Discussion on this point:

“Measurement of individual ethnicity is, however, fairly crude, i.e. foreign-born versus Swedish-born. This is unfortunate, as it means that the confounding control is not satisfactory and more of the individual level variation in birth weight could have been explained if the measurement of individual ethnicity had been more specific. However, we also controlled for maternal health as an individual level predictor of birth weight. Spencer and Logan argue that maternal height is a determinant of birth weight intertwined with social factors (2). As foreign born women are shorter than Swedish born women we conclude that the potential bias of lacking confounding control is minor and do not explain the small and non-significant neighbourhood effects in this study.”

Missing values on the smoking variables is displayed in Table 2. Instead of actual number of observations of the actual variable, we put in number (and percentage) of missing cases on the actual variable.
Gestational age is defined in the same way throughout the time period, by weeks of gestation. In Table 2, it is clarified how the variable is constructed: 22-28 weeks to >= 42 weeks of gestation.

Parity is defined as: 1= first child, 2-3 = second-third child, >4 = fourth child or more.

We have added a more complete description of the neighbourhood variables. Hopefully, this is an answer to the questions regarding those variables.

**Analytic approach**

The referee’s views are commented in order of appearance (the figure refers to the paragraph):

1. We have rerun the models in order to adjust for any time trend (described below). We have also changed the description regarding the outlining of the models so it should be clearer.

3. I leave the formulae at this stage as the other referee did not comment on them being too basic. It is of course simple to take them out…

4. Regarding the neighbourhood variation etc. The description is changed as to: “The between-neighbourhood variation was described as the proportional change in variance in the individual-level model and the neighbourhood-level model compared with the null model.”

5. The sentence: “…many individual level variables…”, was taken out as it obviously was confusing. And the theoretical framework does not contradict this procedure…

2 and 6. We agree with the referee that the approach was unclear and therefore we have developed the analyses further. To handle the fact that the observations are made one specific year 1993 – 2002, we specified an empty model and as a second model we introduced the year of birth as a random slope. This means that the estimates are adjusted for any time trend in the material. We could show that neighbourhood variation in mean birth weight is increasing by year of observation. This time trend was minor.

7. In order to further check our estimates we ran the models using mcmc procedures. The results were similar indicating robustness of estimates. This is also noticed in the Method section.

**Results:**

1 and 2. The correlation between neighbourhood variables is a problem acknowledged by many researchers. I have kept both variables as they can be argued to measure different theoretical constructs and their impact is the main focus of the study.

**Discussion.**

1. The referee has made an important point about the interpretation of our findings. We have therefore rewritten the first part of the discussion accordingly.
2. I acknowledge the referee’s point here as the text was unclear: The paragraph is rewritten as already cited (see above on Methods).

3. Oakes criticises observational neighbourhood effects studies for being just that, observational. And I agree with professor Merlo and Chaix in what they call trialism. To perform experiments is for several reasons not possible. Not least for ethical reasons. Who would want a society where human beings were randomized into their living conditions? Moving To Opportunity is a study impossible to carry out in a Nordic Welfare state. Observational studies are what they are; carried out under certain assumptions. This study is another neighbourhood effect study. In this study two correlated neighbourhood variables were used. The two neighbourhood variables, economic status and ethnic composition, are progressively coinciding and it is therefore difficult to disentangle their separate effects. On the other hand, as shown in table 2 and 3, there is substantial variability in neighbourhood characteristics (i.e. ethnic composition and economic status) within social class categories. This is in line with comments from professor Diez Roux who claim that lack of overlap is unlikely to be as important a problem in estimates of area effects from observational studies as is sometimes implied (3). Further, we have a large database and we checked the estimates in the multilevel regression model with MCMC procedure.

4. As the reviewer state, there are previous research on birth weight and contextual factors. As we have extended the theoretical framework we have also taken this research into consideration. See Introduction.

**Response to James Macinko**

Major Revisions (by paragraph):

1. The introduction is thoroughly rewritten and the theoretical framework is outlined in a more consistent way.

In this study we show that variations in mean birth weight between neighbourhoods are minor. We have relied on register data and the construction of neighbourhoods is based on natural geographic borders and homogeneity of housing. The question would be if we have the “wrong” geographic borders. For example if the neighbourhoods were too big. This could lead to dilution of effects. We do not believe that this is the case here. The neighbourhoods contain between 4000 to 9000 inhabitants which would be an adequate size to show any effects (4).

2. The reviewer is pointing at a very important issue and the interpretation of results is not straightforward. However, our aim was to investigate any neighbourhood effect over and above individual vulnerability. The interpretation we make is related to the macro context of society and we have rewritten the Discussion section as follows:

“This study is carried out in Sweden, which is described as a state with a pronounced responsibility for the welfare of the individuals, such as universal social benefits, gender equality seen in high female labour market participation, and a general and free maternal health care. We can not conclude that our negative results, i.e. the non-existent differences in mean birth weight in Swedish urban neighbourhoods depend on the welfare social policies in
Sweden. However, Chung and Muntaner could show that more protective types of welfare regimes, namely the group of Social Democratic countries, were able to provide a more population health-friendly environment to its citizens in the last 39 years (5). Thus, it seems reasonable to conclude that welfare institutions and benefits in Sweden might buffer against negative health outcomes, i.e. low mean birth weight, due to adverse structural organisation of urban neighbourhoods.”

Several other comments:
1. Hopefully we have made this point clearer in the rewritten Introduction. We have these two neighbourhood characteristics available in the database and can not create new variables. Birth weight is considered a measure sensitive to resources in society. Here we are investigating whether birth weight vary between neighbourhoods in a welfare state, where resources can be expected to be equally distributed. Even so there are factors within a neighbourhood which are unmeasured but are shown by geographers to follow the income pattern in society. It is therefore important to investigate whether there is variation in birth weight between Swedish neighbourhoods.

2. The reviewer is proposing an interesting approach. Unfortunately, we have no data to explore the potentially mediating effect of the quality of prenatal care in the neighbourhoods.

Minor revisions:
1. The manuscript is reviewed by a British English interpreter. And the concepts relating to the neighbourhood variables are scrutinized and changed.

2. The rationale for including neighbourhood ethnic composition is clarified in the Introduction and also the measurement is described in more detail in the Method section and in Table 1.

Response to Maria Melchior
The answers are sorted after each paragraph.

1. The Introduction is rewritten thoroughly, and the mechanisms of the associations are hopefully better outlined.

2. The details on ethnicity and poverty in the Swedish neighbourhoods are also described in the Introduction.

3. I have kept the classification with three categories in Table 2 and with 6 classifications in the multilevel models. The rationale is purely that presenting the eight categories in Table 2 is space-consuming. The eight categories in the multilevel models are important to monitor as this displays a linear association, though not significant.

4. The use of a continuous variable on birth weight has advantages. We have controlled for gestational age in the analysis. That means that the linear specification of the outcome captures potentially critical variation across the distribution of birth weight that cannot be explained by length of gestation. Low birth weight is a rare outcome which makes it more difficult to detect any neighbourhood variation.
5. In tables 4-6 (former tables 2-4) p-values are added to the actual estimates.

6. Conclusions are rewritten in order not exaggerate our findings. On the other hand, within our theoretical framework it is important to see that our results occur in a certain state with a certain welfare system.

7. Exclusion of cases is described in the Method section. Characteristics of excluded cases are similar to those included with respect to education, smoking habits, age, parity etc..

8. Neighbourhood ethnicity is replaced by the concept: ethnic composition.

9. I hope the issue on maternal height is clarified by the following paragraph in the Discussion:
   “However, we also controlled for maternal height as an individual level predictor of birth weight. Spencer and Logan argue that maternal height is a determinant of birth weight intertwined with social factors (2). As foreign born women are shorter than Swedish born women we conclude that the potential bias of lacking confounding control is minor and do not explain the small and non-significant neighbourhood effects in this study.”