Author's response to reviews

Title: The area-based social patterning of injuries among 10 to 19 year olds
Changes over time in the Stockholm County

Authors:

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Outline of modifications

Reviewer: Elizabeth Mary Louise Towner

(1) The main problem with this article is that it is unclear to the reader what the overarching hypothesis is and why it is important - the whole article has to be read before this becomes more transparent. The problem is more that of the clarity of presentation, rather than the analyses that have been conducted.

Authors: The reviewer will notice that there has been a lot of rewriting aiming at increasing the clarity of the presentation. Also, considering the comments of two reviewers, we have rephrased our aims and research questions, not exactly as proposed by this reviewer but in a clearer manner, we hope. We have fewer research questions that proposed by this reviewer because we have clarified the difference between our research questions and the methodology employed to address them.

(2) There is a considerable body of literature related to health and place, which examines the ways in which the attributes of places have an effect on people’s health (for example place as a portion of geographic space, physical/environmental attributes of places and place as an area to which people attach certain meanings and action). Some of the explanation for the findings in this paper could perhaps be obtained from such studies. This paper does not refer to the broader geographical literature.

Authors: We agree with the reviewer indeed and we did include this aspect in the introduction as it is important to insist on the fact that not only injuries but other health outcomes may be influenced by area characteristics.

(3) The paper is also not clear about what can be learnt from the study. What
can the academic researcher glean from this?
Authors: Information about this is added in the discussion.

(4) How can the results be applied ¿ what practical implications does it have?
Authors: This aspect is now taken up in the discussion.

On page 15 the authors say: ¿the analyses were conducted under the assumption that residential mobility does not vary between parishes¿. How can this assumption be justified? It would seem highly likely that residential mobility would vary over the time period of this study.
Authors: We are sorry if the statement leads to confusion. We have tried to rephrase the sentence so that what is meant comes up more clearly. It is not a matter of individual people moving or not but, rather, one of differences between parishes in the extent to which people, in general terms, move.

The authors use the terms `over risk¿ and `under risk¿. It is not clear what they mean by this. Does this means `less than expected and `more than expected`?
Authors: The presentation of the results ¿ and discussion have been substantially rephrased to overcome this misunderstanding.

Reviewer: Ronan Lyons

1. I think it would help if the authors decided that the primary question was the stability of rates or gradients over time or the relative importance of economic deprivation or social fragmentation.
Authors: see first comment to the first reviewer.

In the abstract the primary conclusion does not relate to the aim but appears in the second sentence as a negative finding- "the association between those indices is consistent neither over injury cause nor time". This is a very unusual finding - a reduction in previously accepted inequalities in injury and so of great interest but it needs to be clarified.
Authors: The primary aim and conclusions have been rephrased. We agree that those findings are unusual and take them up in the discussion.

2. I also struggled a little with understanding some of the statistical analyses. I suspect the analyses have been carried out but the description of the tables may be incomplete.
Authors: We hope that the precisions introduced in the introduction and materials and methods will help understand what has been done. Even the error and repetition in Result are now corrected and makes the description of the tables
easier to understand.

On page 11 the final paragraph mentions simple correlations but I think partial correlations may be what are presented.

Authors: Corrected

Again the text at the top of table 12 referring to table 5 mentions that "during the second period.... once correction had been made for that of economic deprivation, nearly all causes correlated with economic deprivation". I suspect that the first mention of economic is an error and should refer to social deprivation?

Authors: Corrected

I wondered whether the poisson or negative binomial regressions could not be used to relate injury incidence to deprivation and social fragmentation categories simultaneously?

Authors: One of the co-authors, biostatistician by training is of the view that the correlation between the indices of socio-economic deprivation and social fragmentation is too high (r=0.83 and r=0.86). That makes estimation of index effects unstable. Thus, the choice of partial correlation.

3. The results in tables 3 and 4 are fascinating. There are some very unusual findings with much lower rates amongst the least deprived 5th but really no substantial gradient with deprivation+- in the other 4 fifths (e.g. violence related injuries in table 3)- a very different pattern to that seen in the UK where the biggest difference is usually between the most deprived fifth and the remainder.

Authors: We agree and take this up in the discussion.

The numbers of particular categories of injury are quite small with some unstable rates - it may be better to combine both sexes for some categories.

Authors: By aggregating sexes we would miss important differences in for instance in self-inflicted injuries and road-traffic related ones.

4. There are substantial differences in the deprivation/fragmentation classification of parishes between the two time periods with some areas moving by two categories on a five point scale. There appears to be greater social/economic mobility in Sweden than I have seen previously but this is not discussed in any great detail. I wonder what an analysis based on the earlier 1994 deprivation fifths looks like with 2004 data?

Authors: As we mention in the discussion, what is obvious is that some parishes have moved upwards and others downwards in quintiles. We do not know how
this looks like at the individual family level.

Overall, I find there is too many simultaneous changes in the analyses (areas being recategorised, partial or no adjustment for deprivation and social fragmentation) to understand whether the initial aim has been met.

Authors: We have rephrased our aims and research questions and also tried to make clearer the difference between the methodology we employ to achieve our aim and the research questions themselves.

1. It would be helpful to have a list of the descriptors of external causes of injury in the paper. e.g. the group V01-19 mixes both pedestrian injuries and pedal cycle injuries within what I think is an overall term for vulnerable road users. I do not know the situation in Sweden but in the UK pedestrian activity and injuries are more common in deprived communities (lack of cars) but I suspect cycling may not be so strongly linked with deprivation (cost of bicycles). Perhaps these should be split during analysis?

Authors: A new table is introduced (Table 3) that lists the descriptors of external causes of injury. Small numbers makes it difficult for us to split the vulnerable road users. But we have indicated in the text the % of injuries that are bicycle-related.

2. I struggled a little with the use of the term compositional when used to define area based characteristics. My previous understanding is that this term is used for individual characteristics (age, sex, educational level, social class, etc) and characteristics of neighbourhoods such as area based deprivation or social fragmentation scores would be considered as "contextual factors".

Authors: Corrected.

Reviewer: Larry Svenson

In the Discussion section, the authors should indicate that hospitalized injuries account for a small proportion of injuries among children. Many more children would be seen in the emergency department and/or physician office. The authors should comment that this likely means they are reviewing more severe injuries and whether they anticipate any change in the outcomes for less severe events.

Authors: Information about this is added in the discussion.

There is no indication why two time periods were used instead of using individual years of data.

Authors: Information about this is added in the methods section.
There is no mention of mortality data and why it wasn’t included in the analysis. Sweden has a robust vital event reporting system and this additional information may have added value to the inpatient information presented.

Authors: This aspect is raised in the revised version of the discussion.

The authors make mention of a possible ecological fallacy, which is appropriate. I would also be useful for them to comment on the concepts of homogeniety of the geographic units. Can one assume that all individuals within the geographic area share the same level of social fragmentation or deprivation? There is evidence in the literature the the heterogenity can have a significant impact on the outcomes of ecological studies. This should be addressed in the discussion in more detail.

Authors: We have raised this important issue in the discussion.

On Page 16, it would be nice to see a reference to the impact of coding changes (ICD-9 to ICD-10) on the computation of rates. Is there a coding effect that may influence the temporal findings?

Suggested references for author consideration:
Gilbride S et al. BMC Pediatr 2006; 6:30
Southern DA et al. Med Care 2005; 43(11): 1116-22
De Coster et al. BMC Health Serv Res 2006; 6:77

Authors: We thank for the interesting references. We have taken up 3 of them in the discussion.