Author's response to reviews

Title: Impact of socio-economic status on hospital length of stay following injury: a multicenter cohort study

Authors:

Lynne Moore (Lynne.moore@fmed.ulaval.ca)
Brahim Cisse (brahim.cisse.1@ulaval.ca)
Brice-Lionel Batomen-Kuimi (brice-lionel.batomen-kuimi.1@ulaval.ca)
Henry T Stelfox (tstelfox@ucalgary.ca)
Alexis F Turgeon (Alexis.Turgeon@fmed.ulaval.ca)
Francois Lauzier (Francois.lauzier@med.ulaval.ca)
Julien Clément (Julienclement@hotmail.com)
Gilles Bourgeois (Gilles.bourgeois@inesss.qc.ca)

Version: 4 Date: 10 March 2015

Author's response to reviews: see over
Revision of the manuscript entitled: IMPACT OF SOCIO-ECONOMIC STATUS ON HOSPITAL LENGTH OF STAY FOLLOWING INJURY: A MULTICENTER COHORT STUDY

Dear Editor,

We are pleased to send you a revised version of the above-mentioned manuscript. Reviewers’ comments were very informative and have helped us improve the article. Please find our reply below. We hope you will find the changes satisfactory.

Yours Sincerely

Lynne Moore, Brahim Cisse, Brice Lionel Batomen Kuimi, Henry T Stelfox, François Lauzier, Alexis F Turgeon, Julien Clément, and Gilles Bourgeois

**Reviewer #1**

*Major Compulsory Revisions*

1. I think the suggestions of the impact of SES on LOS is too strongly worded throughout the manuscript. (e.g. p. 5). It is highly likely that LOS is affected by unmeasured severity which may very well be correlated with SES, and as long as this potential concern is not addressed in the manuscript I would be very cautious about any claims of causality. This is an important limitation that currently is not discussed.

   *We agree that in any observational study, particularly based on retrospectively-collected data, residual confounding may be an issue. This potentially limitation has been discussed in more detail in the Discussion section (p14-15, 319-322). We have toned down the claims of causality (abstract, conclusion; Discussion, conclusion).*

2. Please explain the reasons the specific inclusion and exclusion criteria described on page 6, in particular leaving out less than 3 days stays, and hip fractures. How many observations were dropped due to these criteria?
These are the inclusion criteria of the registry (this has been clarified in the Methods section – p6, line 131). The goal is to include moderate to major trauma. Isolated hip fractures are generally considered a consequence of chronic disease, their outcomes are often dependant on comorbidities and many of them are treated in hospitals outside of the trauma system. For this reason, they are commonly excluded from injury cohorts for research purposes (clarified in the Methods section – p6, lines 134-136).

3. Why was 1 added to LOS?
*This was an error and has been corrected (p7, line 147).*

4. The SES index appears to be constructed by the authors. To fully understand how the indexes captures deprivation it is important to show how the different factors included in the index contribute to the index. Please provide this information
*We agree that this aspect of our study design was unclear. We used indices derived and validated by Pampalon and colleagues based on the work by Townsend. This has been clarified (p7, lines 150-152). The items included are specified and references are given (p7).*

5. It is unclear what the authors mean when they say on p. 7 that they used a mixed linear model to “generate mean differences in LOS”. Do you mean you estimated this in a regression model? If so, how was the model specified?
*The word ‘generate’ has been changed for ‘estimate’ (p8, line 167). We have added information on model specification (p 8, lines 170-179).*

6. The comments, p. 9, l. 197-199 about the difficulty of interpreting age/gender effects seem dissatisfactory – could an unstandardized measure be used instead? Or guide the read as to what the effect of might be?
*As mentioned in our response to comment 4, the SES indices we used were derived and validated externally. The values for social and material deprivation indices we obtained by matching trauma registry data via postal code were already standardized for age and gender. However, as age and gender are used as adjustment variables (adjustment is needed as the distribution of these variables differs to that in the population used for standardization), the association between these variables and LOS is not of primary interest. This is why we chose not to present measures of association for these variables (Table 2).*

7. I do not understand what the “Modelling deaths by attributing an LOS of 120 days...” means? Why is it necessary to attribute a long LOS to deaths?
*This is a strategy for including patients deceased to address the potential problem of survivor bias. Deaths are not included because their LOS is unknown (right-truncated). Under the reasonable assumption that severity is correlated with increased LOS, deaths can be attributed an arbitrarily long LOS and...*
included in analysis to evaluate the possibility of survivor bias. References have been added (p9, line 202-203).

8. In the discussion, (page 11), the authors suggest that further analysis is needed to "confirm that prolonged hospital says among patients suffering high material and social deprivation are "indeed inappropriate"". But there is nothing in the analysis that suggests it is inappropriate, and confirm sends a signal that there is. It may all be down to unobserved differences in severity, and as the author's point out lack of access to help in the community/home.
We have changed the wording of this sentence to be more cautious about causality and the interpretation of our results (p13, lines 280-282). Lack of access may be one of the factors explaining longer LOS in patients suffering social deprivation – if this is the case, the prolonged acute care stay would be inappropriate.

9. On page 12 the authors suddenly talk about the SES-readmission association which to the best of my knowledge hasn't been mentioned before. Are the authors referring to previous or forthcoming work on readmissions?
This is an error and has been corrected (p14, line 305).

10. Some of the policy implications on p.13 and 14 seem a little far-fetched: in particular that "interventions to reduce the influence of social disparities on LOS would in turn improve patient morbidity and mortality". I do not see support for this recommendation in the analysis.
This sentence has been toned down (p15, line 338).

11. I also don't think the results show that "consideration of SES in discharge planning and community care may lead to reductions in LOS to improve resource use and outcomes" as the authors don't investigate 1) whether such interventions would have an effect, 2) the cost of those interventions, 3) the effect of such interventions.
We have changed the section title to ‘potential policy implications’ and softened the wording in this paragraph. This section was intended to explore possible implications of study results for future research.

*Minor Essential Revisions
1. It's imprecise to talk of an "increase" in LOS for deprived patients as you do on p. 9. Please consider rewriting so they say that deprived patients have a "higher" LOS than the reference group (this occurs throughout the manuscript)
These modifications have been made.

*Discretionary Revisions
1. Please provide more detail/examples about what constitutes injuries
This information has been added (p6, lines 129-130).

2. Please provide more detail about what level I-IV centres mean
Reviewer #2
The paper measures the association between socioeconomic (SE) conditions and length of stay (LOS) among patients hospitalized for injuries. The paper is globally clear and well written, and the topic is potentially relevant. There are however major limitations in the theoretical background and the methods, which should be addressed. In particular, the data analysis should be reviewed using different ways to classify the SE variables and using different statistical models. I also suggest to examine the readmissions. These suggestions are detailed here-below.

1. The relevance of the issue should be better emphasized. As the authors notice, there is already a substantial literature showing the association between LOS and SE status. The authors should point the specificity and interest of replicating this study for injuries. Why do they expect the association to differ among these patients?

   Justification for evaluating the association between SES and LOS specifically for injury admissions has been provided in the Introduction (p5, lines 108-112).

2. The Methods should indicate if patients can be followed across admissions. A patient can be discharged early but readmitted thereafter. If there are data available, it would be valuable to perform an additional analysis on readmissions.

   We do have information on multiple consecutive admissions for the same injury and unplanned readmissions. These represent only 6.7% and 5.6% of admissions in the study population, respectively. We have previously demonstrated that total LOS is only slightly underestimated by index LOS.[1] While inter-hospital transfers are frequent in injury patients, these transfers are nearly all ED to ED which do not impact LOS. However, we have performed additional sensitivity analyses to evaluate whether using total LOS over index LOS influences study results. These have been added to the sensitivity analyses sections (Methods and Results). Note that total LOS was not used for the main analysis because linkage between the trauma registry and administrative data could only be achieved for 92% of the study population.

Previous research has suggested that low SES is related to higher unplanned 30-day readmission rates.[2] We chose not to perform sensitivity analyses including LOS for unplanned readmissions because these readmissions could be due to many factors including potential complications of the injury and subsequent injury. However, we anticipate that not accounting for additional hospital days due to unplanned readmission probably led to an underestimation of the association between SES and LOS. This has been added to the Discussion section (p14, lines 316-318).
3. The patients transferred to another hospital should be removed from the sample (maybe this was done but the information is not provided). These patients represent 5.5% of the sample who have now been accounted for in sensitivity analyses (multiple consecutive acute care admissions for the same injury).

4. I don’t understand why the authors used the “discharge + 1” to calculate the LOS. This should be explained. This was is an error and has been corrected (p7, line 147).

5. The rationale and construction of the SE variables should be much more detailed, for several reasons:
   a. “Material deprivation” usually refers to persons experiencing serious financial troubles. It is measured asking questions about the possibility to pay invoices, to go on holidays, to have three meals per day, etc. The indicators here clearly do not refer to material deprivation, and the indices are not “deprivation index” (see the literature using the Townsend index, the Carstairs index, and the like).
   b. Education and employment may signal the person’s material conditions, but also his social circumstances. Living alone may lead to social deprivation but also to financial trouble. Hence, the distinction between social and material circumstances is not convincing (or it should be extensively justified, on the basis of the social epidemiology literature). I would classify all these variables as reflecting the “socioeconomic status”.
   c. If the authors really want to maintain the material-social distinction, they should justify how each of them likely affect the LOS, and how these influences may differ. Otherwise, there is no justification for splitting the indicators in two groups.
   d. In the same line, the Discussion does not enlighten why the social deprivation has a greater effect than the material one.
   e. The principal component analysis should be justified. I am convinced that the original variables would provide more interesting results, showing the specific impact of education, employment, etc. It would be interesting to observe how each indicator influences the LOS, and to interpret the different findings. See above.
   f. The principal component analysis should be detailed. We should know why the authors only selected two components, the percentage of variance explained by each component, and how each original variable contributes to the component.

This aspect of our study design was not clearly explained but has been clarified in the revised version (Methods, p7-8, lines 149-164). As explained above (comment 4, reviewer 1), analyses were based on an SES measurement framework derived and validated previously and based on the Townsend index.[3-5] The separation into indices of material and social deprivation was based on principal components analysis, described elsewhere.[3, 4] These indices have been widely validated for use with Canadian census data and for injury populations. Further references have been added including Canadian
cohort studies looking at the association between SES and health outcomes that have used these indices (Methods, p7, lines 160-161). Potential explanations for the differential association between material/social deprivation have been exposed in the Discussion section (p12-13, lines 280-291).

6. The variables used as adjustors in the regression should be explained and justified. Note that some variables are very specific so that most of the readers will probably not be familiar with them (e.g., the GCS, the mechanism of injury, and the MAIS).
*Further details have been given in the Methods section (p 8).*

7. The linear model is usually not appropriate to analyze the LOS, which generally does not follow a normal distribution (it is truncated at zero and right-skewed). I suggest testing other models, namely using a log-linear or gamma distribution.
*We have provided more detailed justification for our choice of model (p 9, lines 185-189).*

8. It is unclear why the health payer and patient remoteness were not included in the analysis, if they are available. These variables also signal the patient’s SE status, and are thus very relevant for the analysis. Note that if they are not included in the analysis, they should be removed from Table 1.
*This is explained in the Methods section (p8, lines180-182). We feel that information on these variables is useful for describing the study population but they can be removed from Table 1 if the Editor considers it appropriate.*

9. The separate analysis for the 65+ and 65- groups should be justified.
*Justification has been added (p8, lines 182-184).*

10. The presentation of the Table 1 should be reviewed. Given the focus of the paper, it would be much more interesting to compare the prevalence of each variable between quintiles. For example, we would like to know the extent to which the proportion of elderly people, or people with comorbidities, is greater at quintile 5 as compared to quintile 1.
*We originally presented patient characteristics for all quintiles of social and material deprivation. This led to two very large tables with too many numbers for easy interpretation. The current presentation allows a comparison of the proportion of patients in the highest quintiles of deprivation across risk factor groups (e.g. 18% of 16-54 year olds suffer high material deprivation versus 25% of patients >=85 years of age). We feel that this presentation leads to intuitive interpretation.*

The Figure 1 is redundant with the Table 2, I suggest to remove it.
*The Figure 1 presents absolute values of mean LOS rather than the differences presented in Table 2 and offers a visual representation of the main results of the study. We therefore consider it a useful addition to the manuscript.*
12. The Discussion refers that the non-inclusion of sub-groups of deprived persons leads to the under-estimation of the LOS-SES association. It is unclear why there should be an under-estimation. We have clarified this (p14, lines 310-314).

13. The possible readmission of people with shorter LOS should analyzed. If no data are available for this analysis, readmissions should be mentioned in the Discussion. See response to comment 2 above. Note that in a study based on the same cohort of patients, hospitals with the highest risk-adjusted rates of readmission also had the longest risk-adjusted mean LOS.[6]

References