Reviewer’s report

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

Version: 3
Date: 30 January 2015

Reviewer: Søren Rud Kristensen

Reviewer’s report:

*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.

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?

3. Why was 1 added to LOS?

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.

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?

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?

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?

8. In the discussion, (page 11), the authors suggest that further analysis is needed to "confirm that prolonged hospital stays 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.

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?
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.

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.

*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)

*Discretionary Revisions*

1. Please provide more detail/examples about what constitutes injuries
2. Please provide more detail about what level I-IV centres mean
3. Consider other wording than dose-response on p.10

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare that I have no competing interests