Reviewer’s report

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

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Reviewer: Julian Perelman

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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?

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.

3. The patients transferred to another hospital should be removed from the sample (maybe this was done but the information is not provided).

4. I don’t understand why the authors used the “discharge + 1” to calculate the LOS. This should be explained.

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.
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.
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).
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.
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.
9. The separate analysis for the 65+ and 65- groups should be justified.
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.
11. The Figure 1 is redundant with the Table 2, I suggest to remove it.
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.
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.

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

Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.