Author's response to reviews

Title: Factors associated with hospital length of stay and hospital charges of road traffic-related injuries in Iran

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Factors affecting hospital resource utilization associated with road traffic-related injuries in Iran

Dear Editor

Please find enclosed a revised version of our paper entitled “Factors affecting hospital resource utilization associated with road traffic-related injuries in Iran”. Based on the comments of the reviewers, changes have been made (and highlighted) to the manuscript. An outline of modifications is attached.

We hope that, in its revised version, you will find the manuscript acceptable for publication.

Yours sincerely,

Hassan Haghparast-Bidgoli, Soheil Saadat, Lennart Bogg, Mohammad H Yarmohammadian and Marie Hasselberg
Outline of modifications

Reviewer 1

1. Study design must be defined as prevalence-based cost-of-illness but covers only the first acute treatment.

In line with the comment of the reviewer, this is a prevalence-based cost-of-illness study, but it is only based on hospital costs and does not include other medical and non-medical costs (e.g. transportation costs, productivity loss...), which comprise a large proportion of RTIs economic burden (As we mentioned in the limitations of this study at the end of discussion section).

2. Sample must be clearly defined by ICD codes (as addition to period and hospitals). It seems not to cover out-patients. This must be clarified.

According to suggestions by the reviewer, we have added the ICD codes in the method section and also clarified that this study excluded outpatient cases (page 5, second paragraph).

3. Results in the main text and Table 1 are conflicted eg. blue-collar workers; 26% vs 29%.

In line with the comment of the reviewer, we have corrected the numbers. (page 6).

4. Table 2 should present the results by categories similar to Table 1. This is because in the main text, the authors have indicated statistically significant differences for many independent variables. Therefore, readers need to know values of each category.

According to the suggestion by the reviewer, we have added a new table (Table 3) showing the mean and median hospital charges and LOS based on socio-demographic and injury-related factors.

5. In the multiple regression analysis, normally we transform cost to log form to meet the assumption of normal distribution. The author must explain why they used ordinal scale. In addition, power of explanation (adjusted R square) and model diagnostics must be reported.

In response to the comment of the reviewer the log transformation did not result in a normal distribution of the dependent variables because of outliers and therefore we decided to use ordinal regression.
We also have added new information about goodness-of-fit of the final models under the Tables 4 and 5.
6. In the Results, it is not clear why "Use of safety equipment" was reported separately from the charge/LOS model. Normally, it should be put in the model as one of potential predictor variables.

The reason for separate analysis of safety equipment was that very few patients had used safety equipment (5% seatbelt users and 5% helmet users) and the power of the analysis is too low to draw conclusions based on them.

7. In Discussion, when severity has been put in the model, it has been controlled, so this must not be cause of difference in other parameters.

In the multivariate models, in addition to injury severity, we adjusted for other potential confounders (including socio-demographic variables).

8. In Discussion on effect of safety belt and helmet, please explore number of persons who have died in the events. These persons were not mentioned in the study. They may not include in the database.

In line with the comment from the reviewer, we have added the absolute number of deaths among the patients that did not use safety equipment in the result section (page 9).

9. In Discussion, it was stated that "Therefore the true hospital costs of RTIs are likely to be underestimated in this study." There should be more evidence or explanation of difference between cost and charge in Iran setting.

In line with the comment from the reviewer regarding “difference between cost and charge in Iran setting”, we have clarified this point in the methods section (page 6, first paragraph) and also in the limitations in the discussion (page 13, first line).

10. Since RTI is important national problem, the authors should present policy recommendation in the Discussion.

According to the suggestions by the reviewer, we have added some policy implications in the discussion section (page 13, second paragraph).

11. In Introduction, please rewrite the part of insurance schemes (i.e., public sector employees vs government employees, self-employed in the Social Security Organization and the Medical Service Insurance Organization.)

According to the suggestions by the reviewer, we have revised the explanation about the insurance schemes in Iran and have added more information (page 4).

12. Use of p value should be followed guidelines. In the paper, both p<0.010 and <0.016 are used.
In the line with the comment from the reviewer, we revised reported P-value figures in the results section.

**Reviewer 2**

1. The paper addresses the hospital resource utilization associated with RTI in Iran. The problem is that the dependent variables and the approach employed do not allow us to achieve the objective of assessing “hospital resource utilization associated with RTIs...”. Length of stay per se does not account for resource utilization since great differences could be observed inside a hospital –between different services- and amongst different hospitals. The other variable employed (total hospital charges) as studied, does not measure hospital resource utilization but expenditures made by individuals/households/insurance companies (all but the hospital per se).

We agree with the reviewer that length of stay and hospital charges (the dependent variables) have some limitations (as we also mentioned in the limitation of the study) and may not measure actual resource utilization, but we do believe that they are two proxies for measuring hospital resource burden due to injuries. These two variables are commonly used in the literature for measuring health care resource utilization. We have mentioned some examples of studies that have used these two measures in the method section (page 6, first line).

We agree that LOS varies between different hospitals (although in our study all hospitals are general public hospitals and share similar characteristics) and even between different services. LOS varies depending on severity and also type of injury, furthermore it is assumed that patients who stay longer in the hospital uses more services and resources. Total hospital charges are used as a complement because it reflects using expensive services/procedures (such as staying in ICU or having surgery...) that cannot be measured by LOS.

Although information about other resource utilization measures such as length of stay in ICU, using MRI/CT Scan or having surgery was collected as a part of the database, but because of the high number of missing data we could not utilise this information in our analysis.

Furthermore, we decided to revise the title and the aim of our study to reflect the reviewer’s comment.

2. In this sense, as recognized by authors in the last paragraph of page 11 and first paragraph of page 12, the study does not estimate costs but individual (or household) expenditures. The use of the word “cost” however is used as synonym of out of pocket expenditures (direct cost from the household’s/insurance companies’ perspective) and thus should be corrected. Hospital resource burden is not the same...
as out of pocket expenditures: the first takes the hospital perspective whereas the second the household/individual/insurance companies’ perspective.

In line with the comment from the reviewer, we have clarified the definition and also components of hospital charges in the method section (first paragraph, page 6). Hospital charges include costs of all services the patient received in hospital. The patient depend on his/her insurance status to pay the whole (if not insured) or part of (varies from 15% to 25% of the billed charges depend on type of insurance) the hospital charges. Moreover, as we mentioned in the method section and also in the limitations of the study, the hospital charges did not include all out-of-pocket payments (such as cost of drugs they have to buy outside hospital, informal payments...).

3. Since authors are using direct costs (expenditures) from the individual/household/insurance perspective, title would be imprecise, since this will not represent hospital resource utilization (which refers more to the resources employed by the hospital to attend injured patients). This is especially true if we consider that many patients were exempt from their payments... in this case, could we really say hospital resources were not used?

We agree with the reviewer that hospital charges may not measure direct hospital resource utilization but argue that this variable in combination with LOS could be used as proxies for measuring hospital resource utilization, in the same way as in previous studies.

4. The construction of the expenditure variable is not absolutely clear for me. It seems that patients were surveyed after the first 24 hours of their admission to the hospital. Hospital charges are commonly estimated immediately before discharging patients. Would it be possible to have surveyed injured patients before they were actually charged/billed by the hospital? How would the field work team managed this situation? In addition, in the discussion section (last paragraph of page 11), authors say that “total hospital charges are billed charges and do not reflect actual payments nor true hospital costs”. I do not understand the difference between billed and actual payments. Since authors state that they obtained expenditure information from a patient’s survey, I assumed actual payments were analyzed. This should be further clarified.

Based on the comment from the reviewer, we have clarified the definition of hospital charges and how it was collected in the method section (first paragraph, page 6). Information related to hospital charges were collected from the accounting records of each patient when they were discharged. We have also clarified differences between billed charges and actual payments in the method section (first paragraph, page 6) and in the discussion (page 13, first line). “total hospital charges are billed charges and do not reflect actual payments nor true hospital costs due to factors such as government subsidies to hospital services and
medicines, discounts and exemptions given to some patients by the hospitals and also not including all out-of-pocket payments by the patients.”

5. In the case of insured people it is not clear for me whether expenditures refers to what the insurance companies directly spent or what individuals/households paid from premiums, co-payments, etc. or both. This is something important to clarify. If it refers to individuals/households payments and if authors have information on individual/household’s income, the estimation of catastrophic expenditures would be desirable… By using this measure, authors could analyse whether payment exceptions are based on equity criteria or not. Since lower level of education is associated to higher expenses, it seems that no equity criterion was used.

Based on the comment from the reviewer, we clarified the definition of hospital charges in the method section (first paragraph, page 6). Total hospital charges are the amount billed by the hospitals when patients are discharged and include costs of all services that the patients received in hospital. In the bill, the share of payment by patient and the insurance company is determined. The share of payment by patients varies by type of insurance company (varies between 15% to 25% of total charges).

6. Another problem is that we do not have any information about the criteria used to subsidize or waive hospital charges. Only in table 2 authors say that because of their short LOS or poverty status, however it is not clear whether this is the authors assumption or if is a clear normal criteria employed by all hospitals. We also do not have information on what exactly do the hospital charges include and what does not.

In line with the comment from the reviewer, there are guidelines in governmental hospitals (or hospitals affiliated to the medical universities) for exemption/discount, but it is not clear if these guidelines are implemented universally or not. Based on the guidelines, people who are entitled to exemption/discount are the poor/destitute, close family members of the staff working in the hospital or the Medical University which the hospital belong to and also students of the medical university.

Moreover, now we have added details of the hospital charges in the method section (first paragraph, page 6).

7. It would be great if authors specify the non-participation rate.

In response to the comment from the reviewer, very few patients rejected to participate in the study, the reason for that was that all interviews were done face-to-face with the patients.

8. From the text it was not clear for me whether all patients attending hospital based ER services and discharged <24 hours were included in the study or not. Please clarify and explain the reasons of not including them (in case this is true).
To answer this question from the reviewer, all patients who stayed less than 24 hours in the ER were excluded from the database, because lack of reliable records for this group of patients. Moreover, in some cases, patients were referred to several hospitals before receiving any definitive care, either because of lack of a special service at that hospital or demand from the patient to be referred to a modern hospital.

9. **International readers could be interested in the differences in terms of coverage / quality / any other? of public insurance systems in Iran in order to better understand the results. For example, it is not obvious the difference (if any) of the Social Security Organization and the Medical Service Insurance Organization since they seem to cover similar type of populations.**

According to the suggestions by the reviewer, we have revised the explanation about the insurance schemes in Iran and added more information (page 4).

10. **Authors say at the end of the discussion section: “Finally we had no possibility to adjust total hospital charges based on the consumer price index for healthcare due to substantial (monthly and annually) fluctuations in the index in Iran.” I personally do not understand why, if such important fluctuations of the CPI exist, authors do not adjusted figures using the CPI (that would be exactly the reason to adjust figures). One IRR from 2000 does not value the same as one IRR from 2004. Since patients from all this period are analyzed, not considering the inflation could potentially bias their findings of the regression analysis performed and thus expenditures should be comparable from one year to another.**

In line with comment from the reviewer, we have adjusted the hospital charges with inflation and some changes occurred in the results that are presented in table 4 and also revision made in the text.

11. **Since dependent variables were analyzed in categories, authors should clarify what categories of expenditures and LOS were used in the regression analysis.**

In line with the comments of the reviewer, we have now added the categories of total hospital charges and LOS which have been used in the multivariate analysis in the data analysis section (page 6, data analysis, second paragraph).

12. Page 7 second paragraph of “Total hospital charges and LOS” reads: “Patients with insurance stayed longer and were charged more per hospitalization compared to patients without any type of insurance.” Authors should discussed potential reasons of this (is demand induced?, or do insured people received better services or of higher quality?).

Based on the comment received by the reviewer, we have now revised the explanation about the effect of insurance on LOS and hospital charges in the discussion section (page 10, fourth paragraph)
13. Patients who used seat-belt had higher hospital charges compared to patients who did not use seat-belt. This conclusion could be misleading. Since use of seat-belt is widely reported to be associated with socio-economic status, higher income could be the reason of the higher hospital charges and not the use of this dispositive per se.

After adjusting the hospital charges by inflation, the patients who used seat-belt has less hospital charges than the patients who did not use seat-belt, although the difference was not significant.

However, since very few patients had used seat-belt and helmet, the authors tried to interpret the results with caution and not draw any conclusion based on these results.

14. In the footnotes of tables 3 and 4 authors say that they did not included the “others” category group in occupation and road user type due to the heterogeneity of these groups. I do not think excluding these individuals from the full model analysis is the best way to approach this situation (analyze data). Authors could leave pedestrians as the comparison category (as they did with the final model) and include “others” as another category.

Regarding the comment from the reviewer on why we did not include the category “others” in the analysis, the authors believe that the heterogeneity of this group still remain a problem that would affect the analysis and therefore would like to keep it that way.

**Minor Essential Revisions:**

1. More details on how authors evaluated the goodness of fit of final models should be provided in the data analysis section.

Based on the comment from the reviewer, we have added information about goodness-of-fit of the final models under the tables 4 and 5.

2. I recommend not using the plus/minus sign when referring to standard deviation. Instead, I suggest the use of “SD=X”.

It has now been revised based on the reviewer’s comment.

3. I recommend the use of collision instead of the word “accident” (page 8, first paragraph of “Use of safety equipment (seat-belt/helmet)”.

It has now been revised based on the reviewer’s comment.

4. The examples for the “other” categories of occupation provided in parenthesis seem not to be mutually exclusive. For example, children vs students, housewives vs
unemployed. I think this could be rephrased in a way that it is clearer for potential readers.

The definition of the category “other” in the result section, has been clarified based on the reviewer’s comment. The category “children” has been changed to “children younger than school age” and “housewives” has been included in the category “unemployed”.

5. Add a comma after “unemployed persons” and a point after “etc” (first paragraph of results, page 6).

It has now been revised based on the reviewer’s comment.

6. In text authors say that 29% of patients had elementary and intermediate education, but in Table 1 this percentage corresponds to 49.6%.

It has now been revised based on the reviewer’s comment.

7. In the fourth line of second paragraph of page 9, there is an extra point that should be eliminated.

It has now been revised based on the reviewer’s comment.

8. Abbreviation “HI” of first paragraph of page 10 should be defined in the text and included in the list of Abbreviations.

It has now been revised based on the reviewer’s comment. “HI” has been changed to “health insurance” in the discussion section (page 10, fourth paragraph).

9. A point is missing at the end of first paragraph in page 11.

It has now been revised based on the reviewer’s comment.

10. In the footnote of table 2, the second sentence has an extra “were” that could be eliminated.

It has now been revised based on the reviewer’s comment.

11. I suggest eliminating the following sentence (page 9, in the last but one paragraph): “Men had longer LOS and higher hospitalization charges, but after adjusting for other variables, men had lower hospital charges than women”. This clarification seems to be not relevant. The following sentence: “this might be explained by a larger number of women in the lower socio-economic groups” actually contradicts the last part of the previous sentence. Authors need to clarify this.
After adjusting the hospital charges for inflation, there was no significant association between sex and total hospital charges. Therefore, the above mentioned explanations were deleted from the discussion.

**12. Why would authors expect higher ISS in the blue-collar and farmer groups? This finding should be discussed deeper.**

Although a higher risk of injury among manual workers (including farmers), is well-documented in the literature, there is little evidence if the severity level show the same social pattern. However, based on the information in this study and previous studies in Iran (mentioned in the reference list of the paper), the severity of injury is higher among these occupational groups (often characterized as lower socio-economic groups) compared to white-collar and other occupation groups.

**13. Given that different hospitals were included in the sample, authors should acknowledge that one day of hospitalization might not necessarily be the same for two different hospitals (depending on the level of each hospital).**

In response to the comment regarding potential differences in the measure “one day of hospitalization in the hospitals”, our database only included general hospitals affiliated to the Iranian Ministry of Health which indicates similarities in this measure since all hospitals follow the same rules and regulations.

**Discretionary Revisions:**

1. Since authors report a skewed distribution of total hospital charges and LOS (page 6, first paragraph), another approach authors could have used is to analyze the probability of having any expenditure at all first and then using this probability to explain the total expenditure. This could be done in two steps or simultaneously (such as using a Heckman’s approach). This could also provide information on the variables associated to having any payment.

2. Although short-term and long-term disabilities were captured in the questionnaire, only death was analyzed as health outcome in the paper. Information on disabilities should be presented, for example, when comparing the protective effect of safety equipment (seat-belt/helmet). This will add value to the analysis.

3. I think authors have different alternatives of improving the paper. One of them is to estimate the direct costs of RTI from the health system perspective…and even an analysis of indirect cost due to length of hospital stay would provide relevant information. Additionally, variables associated to total economic cost could be then explored. This information could be of better use in terms of policy and decision making. Another option could be to estimate out of pocket expenditures associated to RTI’s medical attention and the prevalence of catastrophic expenditures. This could represent a nice baseline to evaluate the impact of the new law implemented in terms of protecting households from impoverishment due to RTI.
We thank the reviewer for these comments and solutions for improving our work. We will consider the reviewer’s suggestions for our future research.