

PEER REVIEW HISTORY

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ARTICLE DETAILS

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| TITLE (PROVISIONAL) | Quantifying the hospitalised morbidity and mortality attributable to traumatic injury using a population-based matched cohort in Australia |
| AUTHORS | Mitchell, R; Cameron, Cate; McClure, Rod |

VERSION 1 - REVIEW

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| REVIEWER | Lynne Moore University Laval, Canada |
| REVIEW RETURNED | 08-Jul-2016 |

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| GENERAL COMMENTS | <p>This paper describes an interesting study on the burden of injury in terms of mortality and healthcare utilization. I'm not sure that the study contributes significantly to advancing knowledge as the literature on this subject is rich but the adjustment for pre-existing conditions is interesting. I have the following major and minor comments:</p> <p>Major comments</p> <ol style="list-style-type: none">1. Data is 7 years old. This should be discussed in the limitations section.2. Matching on finer categories of residence related to socio-economic status would have been interesting (ecological measures of material and social deprivation). Can you comment on the consequences of not adjusting for socio-economic status in the Discussion section?3. As sampling was conducted by region, it may be appropriate to add a random intercept on region to regression models to account for clustering.4. Table 2 – specify that hospital length of stay means cumulative length of stay in the 12 months post-index admission; give p-values rather than z or chi-squared statistics in the table; it seems very surprising that the mean number of hospital admissions is higher than the mean number of ED presentations – can authors comment?; if the two cohorts were matched for age, why is the age-adjusted difference in mean LOS different from the crude difference? Can you elaborate on why the injured cohort has a higher mean age than the non-injured cohort?5. The first paragraph of the Discussion should summarize main results in relation to the objectives rather than provide a rationale for the study (already given in the Introduction).6. The comment that individuals with a chronic injury profile are less likely to consult for ensuing or other healthcare problems is interesting (p12, lines 33-37) and merits further discussion in line with observed results. Furthermore, it would be interesting to discuss the consideration of injury as a chronic disease.7. 'The experience of comorbidities was likely to be under |
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| | <p>enumerated using hospitalisation data, with the CCI developed to provide an indicator of survival, rather than of health conditions likely to affect in injury-related disability'. Under-reporting of comorbidities and the appropriateness of weights for the CCI are two different problems and should be discussed separately. Have you any reason to believe that underestimation of comorbidities differed between groups? How do you believe underreporting affected your results?</p> <p>8. Did you adjust for the number of Charlson comorbidities or the Index? To avoid the problem related to weights, Carlson comorbidities could have been modelled separately.</p> <p>Minor comments</p> <ol style="list-style-type: none"> 1. Avoid using acronyms that are not widely accepted (GBD, NSW, QLD, SA, RBDM, ASGS RA) 2. P7, lines 28-38: The ICISS is not calculated using survival risk ratios but rather survival proportions. In addition, the traditional ICISS is the product of survival probabilities rather than their sum. 3. Can you briefly justify the ICISS cut-offs used for minor, moderate and serious injury? What was the ICISS used for? The frequency of the ICISS in the injured cohort could be presented in Table 1. 4. 'Descriptive statistics were conducted and chi-square tests of independence and Wilcoxon Mann-Whitney tests compared the characteristics of injured individuals admitted to hospital with the comparison cohort, as appropriate' This sentence needs to be rewritten. 5. 'Linear regression was used to age-adjust for hospital LOS post the index injury hospitalisation'. Should read 'Linear regression was used adjust post-index hospital LOS comparisons for age' 6. '...used to estimate hospital admissions where the index injury was a likely contributory factor and was calculated by subtracting 1 from...' should read '...used to estimate THE PERCENT OF hospital admissions where the index injury was a likely contributory factor and was calculated by subtracting 1 from...' 7. Table 1 – information in the injury and non-injured cohort columns is redundant as we already know that a 1-1 matching was used. It would be interesting to present data in the Results section 'pre-injury health service use' in Table 1 along with information on injury severity (as mentioned above). 8. Table 3 – no need to indicate $p < 0.0001$ in footnotes as confidence intervals are given; Interesting to see that RR decrease with increasing age. 9. I like Figures 1 and 2. 10. When referring to 'comorbidities' I would specify 'Charlson comorbidities' since mental health conditions and dependence are comorbidities too (included in the Elixhauser classification system). 11. The sentence 'This study identified in the 12 months preceding the index injury hospitalisation, the injured cohort had twice the proportion of ED presentations and one and a half times the proportion of hospital admissions than their non-injured counterparts' needs to be rephrased. |
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| REVIEWER | Claire Jourdan Physical and Medicine Department, University Hospital of Montpellier, Montpellier, France |
| REVIEW RETURNED | 26-Jul-2016 |

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| GENERAL COMMENTS | General comments |
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| | <p>This study addresses one-year hospitalization, ED visits and mortality rates post traumatic injury, using administrative data, with comprehensive inclusion of cases (injuries requiring hospitalization) and matched controls from the general population. The method is seldom used and provides useful additional information on injury consequences. The adjustment on pre-injury comorbidities, and especially on mental health conditions and alcohol abuse, are important strengths of the study.</p> <p>Several questions are raised by the reading of the manuscript.</p> <p>The main change needed regards the lack of adjustment for counts of ED visits and for mortality rate. If I am not mistaken, results of adjustment models were only provided for hospitalization rates and lengths of stay. Similar analyses are needed for the other endpoints, otherwise no discussions or conclusions can be taken on the other outcome variables, and for instance references in title, abstract, discussion, and conclusion on higher mortality are misleading.</p> <p>A major potential limitation lies in the way data was collected for the control population. Comorbidities, alcohol abuse and mental health were collected from hospital administrative data. What about persons who were not hospitalized during the study period, probably the most part of the control population? Are there any guarantees that information on health conditions was as accurate for them? If not, it would be a major bias for the whole study, since comorbidities for the control population would be under-evaluated. This point needs to be clarified and discussed.</p> <p>In most of the manuscript, references are made on « burden » of injuries, or « health care utilization » or « morbidity ». In reality, only ED visits and hospital admissions were assessed, and much more data, especially on ambulatory care, diagnoses and impairments, would be needed to comprehensively address morbidity or health care. It would thus be more accurate to refer simply to “ED visits and hospital admissions” in the text, and in the title.</p> <p>It is also misleading to use phrases such as “long-term” consequences of injuries, since only the first 12 months have been addressed, and once again, no typically long-term outcome (for instance impairments) were reported. The study rather addresses acute and subacute hospital care.</p> <p>It would be more appropriate to refer to “injuries requiring hospitalizations” rather than injuries in the manuscript. Here, only these more severe injuries were addressed, so that it is not the burden of injuries but of severe injuries that is studied. A comment could also be added in the discussion on this inclusion criterion, with references on the proportion of total injuries that are represented by selecting only injuries requiring hospital admission.</p> <p>I was not totally certain about how the rate and length of hospitalizations was measured based on the methods. Initial hospital admission being the inclusion criterion of the injured sample, it seems logical that the index hospitalization was not included in the measure of rate and length of hospitalizations. Can this be specified clearly? If this is indeed the case, the discussion could be strengthened by specifying that the early burden (and cost) of care due to injury measured here comes in addition to the initial hospitalization.</p> <p>But it raises a question on the choice of the injury date for the start of the 12-months observation period. For the most severe injuries,</p> |
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| | <p>with presumably long initial hospitalizations, the date of hospital discharge would have been more relevant (as done in previous literature), in order not to under-evaluate number of ED visits and re-hospitalization. Information on length of index hospitalization of injuries, especially severe injuries, would be needed to judge on this possible bias.</p> <p>A limitation should be discussed. No socio-demographic data was available for adjustment, although literature shows that many social characteristics are related to risk of injuries, and are also related to morbidity and hospitalization. It is presumable that the injured and control populations were not identical regarding social and socio-economic characteristics. This potential bias needs to be discussed, along with appropriate references from the literature.</p> <p>Introduction P5 I3-11. This section and the objective need to be rewritten to specify that it is not the morbidity, the injury outcomes or health care utilization which was addressed, but hospitalizations and ED visits.</p> <p>Methods The methods part is not totally clear and hard to follow. Some suggestions are made to make it clearer. The methods might be easier to read with a different order of paragraphs: definition of populations – data sources – methods of data linkage – data collection of patients’ characteristics</p> <ul style="list-style-type: none"> - P5 I23. Change « data collections » to « data sources ». - P6 I3. Provide first a definition of the injured population and inclusion / exclusion criteriae. Define hospital admission – is it admission of at least one night or 24h? - P6 I 14-15. This subgroup of individuals >85 years represents 12% of the cohort, according to tables. It is necessary to provide comparative data on age distributions in the injured and non-injured sample for this subgroup, given the different matching methodology for them. - P6 I16. Sentence beginning with “All ED, hospital admission,…” provides information on data collection rather than on population selection. It should appear in another paragraph. - P7 I8-24. Provide more information on this scale, on which population and outcomes it was validated on and on how it measures comorbidities. Justification is needed for the choice of categorization of this scale. What was its distribution in both samples? - P7 I8-24. See global comments. How and with what accuracy were these data (comorbidity, mental health and alcohol) collected for the non-injured sample, especially those who were not hospitalized during the study period? Were hospitalization records as reliable for both samples? - P7 I28-38. Justify the categorization choice. - P8 I3. Change the paragraph title for something more specific than “health outcomes” (ex : ED visits and hospital admissions) - P8 I3-17. See general comments. Specify that the index hospital admission was not included in the count of hospital admissions. Were transfers to another hospital after the index admission counted as new hospitalizations or not? Did the measure of cumulative length of stay include the index hospitalization or not? Did the 12-month period start upon injury date or upon first hospital discharge (which would make more sense)? Is it possible to specify (here or in the results) the total length of stay of the index hospitalization, including hospitalization time post-transfers to different hospitals? This would provide useful information on direct consequences of injuries. |
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| | <p>- P8 I3-17. Is there data on causes of hospital admissions and mortality? This data would deserve a specific table / results paragraph, as it would provide additional information on attributable morbidity and mortality. If no such data was available, it could be discussed as study perspectives in the discussion.</p> <p>- Data analysis. How were covariables introduced in models? As quantitative variables or using the defined categories?</p> <p>Results</p> <p>- The gender and age distribution of the population seems a little atypical, injuries being usually more often sustained by males and some specific age groups (18-30 and > 55). Is it possible to provide a short commentary on this in the discussion, using recent comparison literature data on epidemiology of injuries?</p> <p>- More comments would be interesting, in the results and in the discussion, on the aspects of the figures. In fig1, the higher rate of hospitalizations is visible in the first 4 months, then seems to fall back to pre-injury levels. Are there mainly re-hospitalizations? Is there really an effect of the injury on the 12-months hospitalization rate or rather on the 4-months hospitalization rate? Should conclusions include these hypotheses? Likewise in fig2, mortality rates seem higher during the first 7 months post-injury only.</p> <p>- P10 I22-44. This paragraph could be more concise – it mostly repeats results provided in table 3.</p> <p>Discussion</p> <p>- The discussion could be rather oriented towards medium-term hospital and ED admissions post-injury, which is the main topic of the paper, rather than on the burden of injuries, which would require further information on health care and disability, and long-term data. Discussion could thus address aspects such as one-year costs of injuries, early and late complications, causes of re-hospitalizations, early consequences of injuries on previous health conditions, avoidable early deaths... according to authors' preferences.</p> <p>- P12 I43-47. I did not understand this sentence. Are the age distributions of the two samples not identical, given the matching process? Why is the non-injured group older? If this is so, it puts into question the whole matching and comparison method.</p> <p>- P13 I13. See comment on figures. The phrase “many months post-injury” is not accurate, the present data indicating rather an increased rate of hospitalization for the first 4 months.</p> |
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Lynne Moore

Institution and Country: University Laval, Canada Competing Interests: None declared

This paper describes an interesting study on the burden of injury in terms of mortality and healthcare utilization. I'm not sure that the study contributes significantly to advancing knowledge as the literature on this subject is rich but the adjustment for pre-existing conditions is interesting. I have the following major and minor comments:

Major comments

1. Data is 7 years old. This should be discussed in the limitations section.

Response: This study was a proof of concept that multi-state data linkage could be conducted in Australia and has been the first piece of research to prove that it is possible. When the research began in 2012, the 2010 hospitalisation data was the most recent hospitalisation data available for 12-

month follow-up; however, the underdeveloped state of the Australian linkage infrastructure and capacity mean that the linkage and provision of correctly linked data extracts to the investigators from all three Australian jurisdictions took four years, and the data was only supplied in 2016. Information has been included in the limitations section regarding the timeframe of the study.

2. Matching on finer categories of residence related to socio-economic status would have been interesting (ecological measures of material and social deprivation). Can you comment on the consequences of not adjusting for socio-economic status in the Discussion section?

Response: In Australia, socio-economic disadvantage is estimated using the Socio-Economic Indexes for Areas (SEIFA). SEIFA is a multicomponent area level measure of the SES of the community defined by an individual's postcode of residence. As the injured and non-injured cohorts were matched on age, gender and postcode of residence, the authors have effectively matched on ecological level of SES the reviewers were seeking. By matching by postcode, we used the best available option to try to ensure that the injured and non-injured cohorts are as alike as possible in terms of socio-economic disadvantage.

3. As sampling was conducted by region, it may be appropriate to add a random intercept on region to regression models to account for clustering.

Response: The data from each of the three states was examined separately and there was no wide variation in the rate ratios of hospital admissions between states, therefore a random intercept for state was not included in the model.

4. Table 2 – specify that hospital length of stay means cumulative length of stay in the 12 months post-index admission; give p-values rather than z or chi-squared statistics in the table; it seems very surprising that the mean number of hospital admissions is higher than the mean number of ED presentations – can authors comment?; if the two cohorts were matched for age, why is the age-adjusted difference in mean LOS different from the crude difference? Can you elaborate on why the injured cohort has a higher mean age than the non-injured cohort?

Response: That the hospital length of stay was cumulative for the period 12 months post the index injury admission has been added to Table 2. Table 2 has both the statistics and the p-values reported as a footer and the authors would prefer to keep both statistics and p-values reported. That the mean number of ED presentations is lower than the mean number of hospitalisations in the 12 months post the index injury hospitalisations indicates that some individuals are not presenting back through the ED for return, planned hospitalisations, such as for additional rehabilitation episodes, including 'Care involving use of rehabilitation procedure, unspecified', further procedures, such as 'Follow-up care involving removal of fracture plate and other internal fixation device' or for ongoing health concerns, such as renal disease, including 'Extracorporeal dialysis'. The authors plan to examine hospital readmissions by type of injury.

The age-adjusted mean hospital LOS is different from the unadjusted hospital LOS because older individuals were more likely to be hospitalised within the matched-cohort study. Overall, the injured and non-injured groups have the same mean age ie 52.0 years. However, the injured hospitalised cohort has a higher mean age compared to the non-injured group who were hospitalised ie. 64.6 years versus 59.0 years, respectively, ie not all of the non-injured group were hospitalised.

5. The first paragraph of the Discussion should summarize main results in relation to the objectives rather than provide a rationale for the study (already given in the Introduction).

Response: The first paragraph of the discussion has been modified.

6. The comment that individuals with a chronic injury profile are less likely to consult for ensuing or other healthcare problems is interesting (p12, lines 33-37) and merits further discussion in line with observed results. Furthermore, it would be interesting to discuss the consideration of injury as a chronic disease.

Response: The authors state in the manuscript that in the 12 months after the index injury hospitalisation, the injured cohort had twice the proportion of ED presentations and hospital admissions than their non-injured counterparts. The injured cohort also had three times the proportion of mortality within 12-months of the index injury hospital admission than the non-injured cohort.

7. 'The experience of comorbidities was likely to be under enumerated using hospitalisation data, with the CCI developed to provide an indicator of survival, rather than of health conditions likely to affect in injury-related disability'. Under-reporting of comorbidities and the appropriateness of weights for the CCI are two different problems and should be discussed separately. Have you any reason to believe that underestimation of comorbidities differed between groups? How do you believe underreporting affected your results?

Response: The limitations section has been revised to discuss these two limitations separately.

8. Did you adjust for the number of Charlson comorbidities or the Index? To avoid the problem related to weights, Carlson comorbidities could have been modelled separately.

Response: As specified in the method section, the authors adjusted for the number of Charlson comorbidities.

Minor comments

1. Avoid using acronyms that are not widely accepted (GBD, NSW, QLD, SA, RBDM, ASGS RA)

Response: The less common acronyms used internationally have been spelt out.

2. P7, lines 28-38: The ICISS is not calculated using survival risk ratios but rather survival proportions. In addition, the traditional ICISS is the product of survival probabilities rather than their sum.

Response: The authors contend that SRRs are the term commonly referred to in the calculation of ICISS. SRRs for each injury diagnosis are calculated as: number of people with the injury diagnosis 1 that survived divided by the total number of people with injury diagnosis 1, which is essentially the proportion. Equation 1 has been added to the manuscript. The authors do agree that it is the product or multiplication of SRRs to calculate ICISS.

3. Can you briefly justify the ICISS cut-offs used for minor, moderate and serious injury? What was the ICISS used for? The frequency of the ICISS in the injured cohort could be presented in Table 1.

Response: The cut-offs for minor, moderate and serious injury were devised in a study referenced in the report by Dayal et al 2008

http://s3.amazonaws.com/zanran_storage/www.nzips.govt.nz/ContentPages/107739153.pdf

Dayal and colleagues comprehensively investigated injury severity, hospital length of stay, injury diagnosis codes and correlations between these to validate the injury severity cut-offs. These were also examined by Dayal et al for a range of injury mechanisms. An example has been provided of how the cut-offs are used to estimate survival probability. Within the current study, ICISS was used to describe the severity of the injuries sustained by the injury cohort.

4. 'Descriptive statistics were conducted and chi-square tests of independence and Wilcoxon Mann-

Whitney tests compared the characteristics of injured individuals admitted to hospital with the comparison cohort, as appropriate' This sentence needs to be rewritten.

Response: The sentence has been modified.

5. 'Linear regression was used to age-adjust for hospital LOS post the index injury hospitalisation'. Should read 'Linear regression was used adjust post-index hospital LOS comparisons for age'

Response: The authors feel this phrasing is a matter of style and the suggested change was not made.

6. '...used to estimate hospital admissions where the index injury was a likely contributory factor and was calculated by subtracting 1 from...' should read '...used to estimate THE PERCENT OF hospital admissions where the index injury was a likely contributory factor and was calculated by subtracting 1 from...'

Response: Suggested text addition made.

7. Table 1 – information in the injury and non-injured cohort columns is redundant as we already know that a 1-1 matching was used. It would be interesting to present data in the Results section 'pre-injury health service use' in Table 1 along with information on injury severity (as mentioned above).

Response: The authors would argue that the gender, age and urban/rural profile of the cohort(s) should be shown and is necessary to describe the demographic characteristics of the study population(s).

8. Table 3 – no need to indicate $p < 0.0001$ in footnotes as confidence intervals are given; Interesting to see that RR decrease with increasing age.

Response: The authors would prefer to keep the p-value as a footnote to the Table for completeness.

9. I like Figures 1 and 2.

Response: Great.

10. When referring to 'comorbidities' I would specify 'Charlson comorbidities' since mental health conditions and dependence are comorbidities too (included in the Elixhauser classification system).

Response: The text has been modified to Charlson comorbidities, where relevant.

11. The sentence 'This study identified in the 12 months preceding the index injury hospitalisation, the injured cohort had twice the proportion of ED presentations and one and a half times the proportion of hospital admissions than their non-injured counterparts' needs to be rephrased.

Response: The sentence has been revised.

Reviewer: 2

Reviewer Name: Claire Jourdan

Institution and Country: Physical and Medicine Department, University Hospital of Montpellier, Montpellier, France Competing Interests: None declared

General comments

This study addresses one-year hospitalization, ED visits and mortality rates post traumatic injury, using administrative data, with comprehensive inclusion of cases (injuries requiring hospitalization) and matched controls from the general population. The method is seldom used and provides useful additional information on injury consequences. The adjustment on pre-injury comorbidities, and especially on mental health conditions and alcohol abuse, are important strengths of the study. Several questions are raised by the reading of the manuscript.

The main change needed regards the lack of adjustment for counts of ED visits and for mortality rate. If I am not mistaken, results of adjustment models were only provided for hospitalization rates and lengths of stay. Similar analyses are needed for the other endpoints, otherwise no discussions or conclusions can be taken on the other outcome variables, and for instance references in title, abstract, discussion, and conclusion on higher mortality are misleading.

Response: The authors did not adjust for counts of ED visits as this could have results in collinearity with the number of hospitalisations. Unadjusted and adjusted rate ratios are only provided for hospital admissions in the 12 months following the index injury. Survival was actually included as an offset (ie. the log of survival at 12 months) in the calculation of unadjusted and adjusted rate ratios. This has been added to the data analysis section.

A major potential limitation lies in the way data was collected for the control population. Comorbidities, alcohol abuse and mental health were collected from hospital administrative data. What about persons who were not hospitalized during the study period, probably the most part of the control population? Are there any guarantees that information on health conditions was as accurate for them? If not, it would be a major bias for the whole study, since comorbidities for the control population would be under-evaluated. This point needs to be clarified and discussed.

Response: The limitations regarding the identification of comorbidities has been enlarged upon in the limitations section of the manuscript.

In most of the manuscript, references are made on « burden » of injuries, or « health care utilization » or « morbidity ». In reality, only ED visits and hospital admissions were assessed, and much more data, especially on ambulatory care, diagnoses and impairments, would be needed to comprehensively address morbidity or health care. It would thus be more accurate to refer simply to “ED visits and hospital admissions” in the text, and in the title. It is also misleading to use phrases such as “long-term” consequences of injuries, since only the first 12 months have been addressed, and once again, no typically long-term outcome (for instance impairments) were reported. The study rather addresses acute and subacute hospital care.

Response: Modifications have been made throughout the manuscript to change the above phrases to: ‘hospitalised morbidity’ and ‘hospital service use’ and ‘12-month mortality’.

It would be more appropriate to refer to “injuries requiring hospitalizations” rather than injuries in the manuscript. Here, only these more severe injuries were addressed, so that it is not the burden of injuries but of severe injuries that is studied. A comment could also be added in the discussion on this inclusion criterion, with references on the proportion of total injuries that are represented by selecting only injuries requiring hospital admission.

Response: The title, aim, method and conclusion has been revised to specify ‘hospitalised morbidity’ and/or ‘injury-related hospitalisations’. The results describe the injured and non-injured cohort.

I was not totally certain about how the rate and length of hospitalizations was measured based on the

methods. Initial hospital admission being the inclusion criterion of the injured sample, it seems logical that the index hospitalization was not included in the measure of rate and length of hospitalizations. Can this be specified clearly? If this is indeed the case, the discussion could be strengthened by specifying that the early burden (and cost) of care due to injury measured here comes in addition to the initial hospitalization.

Response: Yes, that is correct, the index hospitalisation was not included in the calculation of rates or in the calculation of the cumulative hospital LOS. A sentence has been added to paragraph 12 of the method to clearly state this. The early burden of care post the index injury has been highlighted in paragraph 1 of the discussion.

But it raises a question on the choice of the injury date for the start of the 12-months observation period. For the most severe injuries, with presumably long initial hospitalizations, the date of hospital discharge would have been more relevant (as done in previous literature), in order not to underestimate number of ED visits and re-hospitalization. Information on length of index hospitalization of injuries, especially severe injuries, would be needed to judge on this possible bias.

Response: Yes, the authors did consider this exact issue and it is why case selection was made on date of hospital admission and not date of hospital separation. If date of hospital separation had been selected, this could have potentially resulted in the exclusion of injured individuals with very long hospital LOS, potentially the most severely injured.

A limitation should be discussed. No socio-demographic data was available for adjustment, although literature shows that many social characteristics are related to risk of injuries, and are also related to morbidity and hospitalization. It is presumable that the injured and control populations were not identical regarding social and socio-economic characteristics. This potential bias needs to be discussed, along with appropriate references from the literature.

Response: In Australia, socio-economic disadvantage is estimated using the Socio-Economic Indexes for Areas (SEIFA). SEIFA is generated based on the postcode of residence of the individual. As the injured and non-injured cohorts were matched on age, gender and postcode of residence, the authors have conducted the best available option to try to ensure that the injured and non-injured cohorts are as alike as possible in terms of socio-economic disadvantage.

Introduction

P5 I3-11. This section and the objective need to be rewritten to specify that it is not the morbidity, the injury outcomes or health care utilization which was addressed, but hospitalizations and ED visits.

Response: This has been rephrased as 'hospitalised morbidity' and/or injury-related hospitalisations and/or hospital service use, as appropriate.

Methods

The methods part is not totally clear and hard to follow. Some suggestions are made to make it clearer. The methods might be easier to read with a different order of paragraphs: definition of populations – data sources – methods of data linkage – data collection of patients' characteristics - P5 I23. Change « data collections » to « data sources ».

Response: Suggest change has been made.

- P6 I3. Provide first a definition of the injured population and inclusion / exclusion criteriae. Define hospital admission – is it admission of at least one night or 24h?

Response: The injury population definition has been moved from paragraph 2 to paragraph 4. Hospital admissions were all hospital episodes of care, regardless of hospital LOS. Paragraph 2 of the method has been revised to specify that all hospital admissions were included.

- P6 I 14-15. This subgroup of individuals >85 years represents 12% of the cohort, according to tables. It is necessary to provide comparative data on age distributions in the injured and non-injured sample for this subgroup, given the different matching methodology for them.

Response: The matching using the ≥85 year age group instead of age was conducted if required. Some older individuals were able to be matched on single unit of age. The age breakdown for the 85+ year age group for the injured and non-injured cohorts was actually the same. So the proportion of both injury cases and non-injured controls aged 85 to 90 years was 8.03%; 91 to 95 years was 3.08%; and ≥96 years was 0.86%.

- P6 I16. Sentence beginning with “All ED, hospital admission,...” provides information on data collection rather than on population selection. It should appear in another paragraph.

Response: The suggestion to make a second paragraph has been adopted.

- P7 I8-24. Provide more information on this scale, on which population and outcomes it was validated on and on how it measures comorbidities. Justification is needed for the choice of categorization of this scale. What was its distribution in both samples?

Response: The Charlson Comorbidity Index (CCI) is frequently used to estimate comorbidities, control for confounding and assess the impact of comorbidities on survival. The CCI was originally validated back in the 1987 – Charlson et al. A new method of classifying prognostic comorbidity in longitudinal studies: development and validation. *J Chronic Dis.* 1987; 40;373-383. Some additional information on the CCI has been included in the method section. The categorization of the CCI was based on Cameron C. et al. Differences in prevalence of pre-existing morbidity between injured and non-injured population. *Bulletin of the World Health Organization* 2005;83(5):345-52, so that comparisons could be drawn between the two studies. A reference has been added.

The distribution of CCI in the injured and non-injured cohorts is described in paragraph 2 of the results.

- P7 I8-24. See global comments. How and with what accuracy were these data (comorbidity, mental health and alcohol) collected for the non-injured sample, especially those who were not hospitalized during the study period? Were hospitalization records as reliable for both samples?

Response: This has been discussed in the limitations section of the manuscript.

- P7 I28-38. Justify the categorization choice.

Response: The categorisations for minor, moderate and serious injury were devised in a study referenced in the report by Dayal et al 2008
http://s3.amazonaws.com/zanran_storage/www.nzips.govt.nz/ContentPages/107739153.pdf

- P8 I3. Change the paragraph title for something more specific than “health outcomes” (ex : ED visits and hospital admissions)

Response: Heading changed to a specific title.

- P8 I3-17. See general comments. Specify that the index hospital admission was not included in the count of hospital admissions. Were transfers to another hospital after the index admission counted as new hospitalizations or not? Did the measure of cumulative length of stay include the index hospitalization or not? Did the 12-month period start upon injury date or upon first hospital discharge (which would make more sense)? Is it possible to specify (here or in the results) the total length of stay of the index hospitalization, including hospitalization time post-transfers to different hospitals? This would provide useful information on direct consequences of injuries.

Response: That the index hospitalisation was not included in the calculation of rates or in the calculation of the cumulative hospital LOS has been added to paragraph 12 of the method. No hospital transfers were not counted as new hospitalisations as specified in paragraph 12 of the method. The calculation of the 12-month period following the index injury admission commenced on the first day following hospital discharge. The mean hospital LOS of the index injury hospitalisation was 6.35 days (SD 16.0).

- P8 I3-17. Is there data on causes of hospital admissions and mortality? This data would deserve a specific table / results paragraph, as it would provide additional information on attributable morbidity and mortality. If no such data was available, it could be discussed as study perspectives in the discussion.

Response: The authors do have 12-month hospitalisation information by type of injury and also have examined mortality in more detail, including by cause of death. These are the subject of 2 additional manuscripts that have been prepared.

- Data analysis. How were covariables introduced in models? As quantitative variables or using the defined categories?

Response: The number of Charlson comorbidities (i.e. 0, 1-2 or 3+), mental health conditions (i.e., Y/N) and alcohol misuse and dependence (i.e. Y/N) were included in the model in an additive fashion in order to be able to take into account the impact of their inclusion in the model. The categorical nature of the variables has been specified in the data management and analysis section of the method.

Results

- The gender and age distribution of the population seems a little atypical, injuries being usually more often sustained by males and some specific age groups (18-30 and > 55). Is it possible to provide a short commentary on this in the discussion, using recent comparison literature data on epidemiology of injuries?

Response: While males tend to more often be injured in their younger and middle years, females tend to live longer and are likely to account for the higher proportion of females in this cohort. There were 48.2% of females aged ≥ 65 years compared to 20.7% of males aged ≥ 65 years. This information has been added to paragraph 5 of the discussion.

- More comments would be interesting, in the results and in the discussion, on the aspects of the figures. In fig1, the higher rate of hospitalizations is visible in the first 4 months, then seems to fall back to pre-injury levels. Are there mainly re-hospitalizations? Is there really an effect of the injury on the 12-months hospitalization rate or rather on the 4-months hospitalization rate? Should conclusions include these hypotheses? Likewise in fig2, mortality rates seem higher during the first 7 months post-injury only.

Response: Yes, Figure 1 is all hospitalisations, so re-hospitalisations following the index injury

hospitalisation. Further analysis has been undertaken by injury type looking at hospitalisations over time in the same way and the same pattern largely occurs to differing degrees by injury type. As requested additional information has been included in the results section to describe Figures 1 and 2.

- P10 I22-44. This paragraph could be more concise – it mostly repeats results provided in table 3.

Response: Paragraph 4 of the results has been shortened.

Discussion

- The discussion could be rather oriented towards medium-term hospital and ED admissions post-injury, which is the main topic of the paper, rather than on the burden of injuries, which would require further information on health care and disability, and long-term data. Discussion could thus address aspects such as one-year costs of injuries, early and late complications, causes of re-hospitalizations, early consequences of injuries on previous health conditions, avoidable early deaths... according to authors' preferences.

Response: These are all great comments and some are the subject of separate manuscripts that have been prepared or are in-preparation. Within this manuscript, the authors are only examining hospitalisations, ED presentations and mortality, not the full range of burden and costs.

- P12 I43-47. I did not understand this sentence. Are the age distributions of the two samples not identical, given the matching process? Why is the non-injured group older? If this is so, it puts into question the whole matching and comparison method.

Response: The age-adjusted mean hospital LOS is different from the unadjusted hospital LOS because older individuals were more likely to be hospitalised within the matched-cohort study. Overall, the injured and non-injured groups have the same mean age ie 52.0 years. However, the injured hospitalised cohort has a higher mean age compared to the non-injured group who were hospitalised ie. 64.6 years versus 59.0 years, respectively, ie not all of the non-injured group were hospitalised.

- P13 I13. See comment on figures. The phrase “many months post-injury” is not accurate, the present data indicating rather an increased rate of hospitalization for the first 4 months.

Response: The sentence has been revised.

VERSION 2 – REVIEW

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| REVIEWER | Lynne Moore Laval University, Canada |
| REVIEW RETURNED | 04-Oct-2016 |

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| GENERAL COMMENTS | <p>I thank the authors for their response to my comments. I still have some unresolved issues.</p> <p>1. The first paragraph of the Discussion should summarize main results in relation to the objectives rather than provide a rationale for the study (already given in the Introduction). Response: The first paragraph of the discussion has been modified</p> <p>The first paragraph of the Discussion has been modified, but not as suggested. I re-iterate that a summary of results in line with study objectives would be most appropriate.</p> <p>2. ‘The experience of comorbidities was likely to be under</p> |
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| | <p>enumerated using hospitalisation data, with the CCI developed to provide an indicator of survival, rather than of health conditions likely to affect in injury-related disability'. Under-reporting of comorbidities and the appropriateness of weights for the CCI are two different problems and should be discussed separately. Have you any reason to believe that underestimation of comorbidities differed between groups? How do you believe underreporting affected your results?</p> <p>Response: The limitations section has been revised to discuss these two limitations separately.</p> <p>It would be nice to add how you think this may have affected your results, as suggested in the comment above.</p> <p>3. Table 2 –give p-values rather than z or chi-squared statistics in the table;</p> <p>Response: Table 2 has both the statistics and the p-values reported as a footer and the authors would prefer to keep both statistics and p-values reported.</p> <p>I'll leave this up to the editor but in general p-values are reported in the main Table as they are easier to interpret than Z or Chi2 statistics. Reporting both Z or Chi2 statistics AND p-values is superfluous.</p> <p>4. 'Linear regression was used to age-adjust for hospital LOS post the index injury hospitalisation'. Should read 'Linear regression was used adjust post-index hospital LOS comparisons for age'</p> <p>Response: The authors feel this phrasing is a matter of style and the suggested change was not made.</p> <p>I disagree. You have adjusted for age and not for hospital LOS.</p> <p>5. Table 1 – information in the injury and non-injured cohort columns is redundant as we already know that a 1-1 matching was used. It would be interesting to present data in the Results section 'pre-injury health service use' in Table 1 along with information on injury severity (as mentioned above).</p> <p>Response: The authors would argue that the gender, age and urban/rural profile of the cohort(s) should be shown and is necessary to describe the demographic characteristics of the study population(s).</p> <p>I absolutely agree. The comment referred to the fact that it isn't necessary to present for both noninjured and injured cohorts separately as the information is the same for both groups.</p> <p>6. Table 3 – no need to indicate $p < 0.0001$ in footnotes as confidence intervals are given;</p> <p>Response: The authors would prefer to keep the p-value as a footnote to the Table for completeness</p> <p>Again, reporting p-values and confidence intervals is redundant but I'll leave this up to the Editor.</p> |
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Lynne Moore

Institution and Country: Laval University, Canada Competing Interests: None declared

I thank the authors for their response to my comments. I still have some unresolved issues.

1. The first paragraph of the Discussion should summarize main results in relation to the objectives rather than provide a rationale for the study (already given in the Introduction). Earlier Response: The first paragraph of the discussion has been modified

The first paragraph of the Discussion has been modified, but not as suggested. I re-iterate that a summary of results in line with study objectives would be most appropriate.

Response: As suggested we have now deleted the remaining sentences in this first paragraph that explain the rationale or purpose of the study. We have written this paragraph now as a summary statement of the main finding of the study in relation to the study objectives.

2. 'The experience of comorbidities was likely to be under enumerated using hospitalisation data, with the CCI developed to provide an indicator of survival, rather than of health conditions likely to affect in injury-related disability'. Under-reporting of comorbidities and the appropriateness of weights for the CCI are two different problems and should be discussed separately. Have you any reason to believe that underestimation of comorbidities differed between groups? How do you believe underreporting affected your results?

Earlier Response: The limitations section has been revised to discuss these two limitations separately.

It would be nice to add how you think this may have affected your results, as suggested in the comment above.

Response: The samples used in this study were population based, in that we had a total census of hospital admissions and a random sample from the population drawn from a total population list (i.e. the electoral role). Yes, using hospitalisations in the previous 12 months would underestimate the comorbidity, but, arguably would do so equally in both the injured and non-injured samples, so would not bias the study findings. The second issue of appropriateness of the weightings of the Charleston index, (especially as a predictor of morbidity rather than mortality) introduces a second potential error, but this too is likely to be non-differential in that it affects both samples equally and thus is unlikely to have introduced a bias that threatens the validity of the estimated effects.

3. Table 2 –give p-values rather than z or chi-squared statistics in the table;

Response: Table 2 has both the statistics and the p-values reported as a footer and the authors would prefer to keep both statistics and p-values reported.

I'll leave this up to the editor but in general p-values are reported in the main Table as they are easier to interpret than Z or Chi2 statistics. Reporting both Z or Chi2 statistics AND p-values is superfluous.

Response: See Editorial comment from BMJ Open.

4. 'Linear regression was used to age-adjust for hospital LOS post the index injury hospitalisation'. Should read 'Linear regression was used adjust post-index hospital LOS comparisons for age'

Earlier Response: The authors feel this phrasing is a matter of style and the suggested change was not made.

I disagree. You have adjusted for age and not for hospital LOS.

Response: The sentence has been revised: "Linear regression was used to age-adjust the hospital LOS post the index injury hospitalisation."

5. Table 1 – information in the injury and non-injured cohort columns is redundant as we already know that a 1-1 matching was used. It would be interesting to present data in the Results section 'pre-injury health service use' in Table 1 along with information on injury severity (as mentioned above).

Earlier Response: The authors would argue that the gender, age and urban/rural profile of the cohort(s) should be shown and is necessary to describe the demographic characteristics of the study population(s).

I absolutely agree. The comment referred to the fact that it isn't necessary to present for both non-injured and injured cohorts separately as the information is the same for both groups.

Response: Cohort descriptive information has been removed.

6. Table 3 – no need to indicate $p < 0.0001$ in footnotes as confidence intervals are given;

Response: The authors would prefer to keep the p-value as a footnote to the Table for completeness

Again, reporting p-values and confidence intervals is redundant but I'll leave this up to the Editor.

Response: See Editorial comment from BMJ Open.