Reviewer’s report

Title: The importance of increased awareness for delirium in elderly patients with rib fractures after blunt chest wall trauma: a retrospective cohort study on risk factors and outcomes

Version: 0 Date: 31 Mar 2019

Reviewer: Ceri Battle

Reviewer's report:

I would like to thank the editorial team for inviting me to review this manuscript. This well-conducted study investigates the incidence, risk factors and outcomes for delirium in elderly patients who have sustained rib fractures following blunt chest trauma. This is an important area for investigation as it has not previously been reported in this cohort of patients. Overall, this is a well written and interesting study for both clinicians and academics working in the field of elderly trauma. I have a few comments and queries, to which the authors are invited to respond.

ABSTRACT - well written and informative. Accurately describes the study in appropriate detail.

INTRODUCTION - this reads very well and is sufficiently comprehensive to inform the reader of previous literature. It is logical and flows well.

METHODS - overall, well written and easy to read. I just have a few suggestions / questions

Please add some basic hospital demographics (number of ED presentations per year, population covered by hospital) to inform the reader of the type of hospital in which the study was completed.

Are the SNAQ / KATZ tools routinely recorded in medical records and if so, give details of how they are completed and by whom. Also, please add some detail re their reliability and validity, if this information is available in previous research. I’m guessing it may not be for the study cohort (elderly patients with rib fractures), but is there data for similar cohorts?

Did you include dementia / conditions associated with cognitive decline / psychological co-morbidities - eg depression / previous psychosis as these may lead to increased risk of delirium in the elderly? If not, can I suggest adding a sentence to the limitation section

Was a CT thorax routinely available (especially in LET patients) and if not, did you rely on CXR results? Was there always imaging available for the LET patients? If not, were these patients not included in the study? This may be worth adding to your limitations section, as we know that imaging is not fool-proof and such studies are always at the mercy of this fact.
Did you use the imaging reports that were written at the time of injury, or did you re-look at CT / CXR findings? If so, who conducted this for you?

It may help the reader if you include some brief details of your anti-psychotics protocol.

What was your inclusion / exclusion criteria? It may help the reader if you explicitly state these at the start of the methods section. Did you exclude patients with dementia / cognitive decline?

In your outcomes section - what is the reliability / validity of the DOSS?

You state additional adverse events were included as an outcome, can you please list what complications you included as an adverse outcome briefly in your methods?

Statistical analysis - stats are not my strong point, but then this may be the case for a lot of the readers of your manuscript. Why did you use a p-value of 0.30 as a cut off for inclusion in the MV analysis? Just add a reference to justify your choice, as some previous guidelines state that all variables should be included.

In terms of your analysis, please can you outline how you selected your variables (ie were they chosen a priori and based on previous literature) and also, please state how you decided on your sample size? Consider the 'Events per Variable' guidelines. You seem to have studied many variables, for only a relatively small number of events (outcomes). If this is the case, please add a sentence to your limitations to acknowledge this fact.

Can you justify why you didn't impute your missing data.

RESULTS -

Please can you state what additional injuries were sustained by patients - or were they all isolated blunt chest wall trauma.

I got a bit confused as to why the age and KATZ scores were dichotomised (lines 205-208 and 211-215). Again, this may need to be simplified for readers who are not statisticians (like myself).

Overall, the results seem to address too many outcomes for this type of study. I think the secondary outcomes are potentially slightly confusing the flow of the manuscript. This is only a suggestion, but would the study be improved by simply studying the onset of delirium, without the secondary outcomes? I think the study is strong enough with just the primary outcome and you certainly answer your research question (within the paper's title) by just doing this.
DISCUSSION -

This section reads very well and is an interesting conclusion to a well designed and informative study. I particularly like the recommendations for practice section, as a clinician who manages these patients on a daily basis. If you do decide to reduce the number of outcomes investigated, then this may need a bit of editing in line with that.

My only comment regarding the discussion is that the limitations section seems a bit limited! This may be more comprehensive following my suggested changes to the manuscript.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I recommend additional statistical review

Quality of written English
Please indicate the quality of language in the manuscript:

Acceptable

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