Peer Review File

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Reviewer A Comments

Comment 1
In this retrospective study, the authors analyze a dataset of approx 800 patients to determine the association between ICP monitoring and mortality in severe TBI patients. A few general comments.

-I would suggest going through the manuscript and making sure the grammar/sentence structure/etc conveys what you're hoping to get across. There were many times while reading the manuscript where I wasn't exactly sure what the sentence was saying, or where the flow of the message was difficult to understand.
Reply 1:
- Proofreading the revised manuscript was performed by a native speaker (Mr. Rachan Areelon).

Comment 2
-The study question is interesting, and the sample size is fairly large, but only from one institution. I think the study would be strengthened by including either multiple institution's data or by using a national/international trauma database. Given the variables of interest and study design, this would be very feasible to do in something like the National Trauma Data Bank (ACS/TQP), the National Inpatient Sample, NSQIP, etc.
Reply 2:
- I further discussed this topic in the revised manuscript.
Changes in the text: Line 281-290, Page 9

Comment 3
-What was the justification for only focusing on severe TBI? Could be interesting to see how this compares to moderate/mild TBI?
Reply 3:
- Because brain trauma foundation guideline suggested to perform ICP monitoring in severe TBI;therefore, this procedure has been performed commonly in severe TBI. Moreover, our data found that ICP monitoring hardly found in moderate/mild TBI.

Comment 4
-Have the authors considered looking at time to when ICP monitoring was started?

Time from field to hospital for the TBI patients? Isolated TBI vs. multi-system trauma?
Reply 4:
-We added time from field to when ICP monitoring, time from field to hospital, and multiple trauma in table 1. These variables were not associated with outcome.
Changes in the text: Table 1

Reviewer B Comments

Comment 5
This was a very interesting read. There are a few minor revisions:
line 47: change "monitoring was utilized in severe TBI patients was 4.1% in the present cohort" to "monitoring was utilized in severe TBI patients in 4.1% of the present cohort"
Reply 5:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 47

Comment 6
line 68: capitalized "Mild" should be lower case6:
Reply 6:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 70

Comment 7
line 73: "however, the effect of ICP monitoring on mortality has been discussed" is a bit unclear, maybe change it to "however, ICP monitoring may have not have an effect on mortality"?
Reply 7:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 80-81

Comment 8
line 99: delete "for the study"
Reply 8:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 100

Comment 9
line 147: adjust to "road traffic accidents comprising car crashes, motorcycle crashes and pedestrian injuries were the most common mechanisms"

Reply 9:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 184-185 page 6

Comment 10

line 148: adjust to "therefore, alcohol was detected in 35.2% of blood samples"
Reply 10:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 186 page 6

Comment 11
line 150: adjust to "ICP monitoring was performed on thirty-five TBI patients"
Reply 11:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 187 page 6

Comment 12
line 158: adjust to "the baseline characteristics which had an SMD greater than 0.1 were as follows"
Reply 12:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 197 page 7

Comment 13
line 193: change "wildly" to "widely"
Reply 13:
- I revised the manuscript (The sentence was deleted.)

Comment 14
line 200: change "experience" to "experienced"
Reply 14:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 247 page 8

Comment 15
line 215: change "did not afford" to "could not afford"
Reply 15:
- I revised the manuscript according to reviewer's suggestion.
Changes in the text: Line 268 page 9

Comment 16
line 216: adjust to "all treated intracranial hypertension protocols were performed"
Reply 16:

- I revised the manuscript according to reviewer's suggestions and proofreading the revised manuscript was performed by a native speaker (Mr. Rachan Areelon)
Changes in the text: Line 269 page 9

Reviewer C Comments

This reviewer thanks the authors for their hard work and dedicated efforts put into this research project. The paper aims to investigate the effect of invasive intracranial pressure (ICP) monitoring on mortality rates in Thailand and aims to understand and estimate the factors associated with this monitoring in severe traumatic brain injury in this setting. This research attempts to address a very relevant question for global neurotrauma and has possible implications to other resource-limited treatment centres.

Comment 17
General comments: the English grammar and sentence structure needs to be edited throughout the article.
Reply 17:
- The revised manuscript was proofread by Mr. Rachan Areelon.

Comment 18
Introduction: The authors make a broad statement regarding the GCS of most fatal cases in Thailand (hence presumably adult or all TBI cases). However, the referencing leads the reader to a study relevant to pediatrics only. Either rephrase the statement to refer to pediatrics only or find another reference to substantiate this claim.
Reply 18:
- The study population was adult TBI patients, so I revised the manuscript according to reviewer's suggestions
Changes in the text: Line 70-73 page 3

Comment 19
I would advise you discuss the importance of resource management in lower middle-income countries such as Thailand. The country is resource-restricted, and hence while ICP monitoring has shown improved outcomes in other countries, to justify its large expense, ICP monitoring in Thailand specifically needs to investigated. While this has been mentioned towards the end of the introduction, this reviewer feels it is one of the most important points and would consider stating this LMIC consideration in the first paragraph.
Reply 19:
- I revised the manuscript according to reviewer's suggestions
Changes in the text: Line 86-91 page3

Comment 20

General comment relating to the relationship between ICP and mortality: the introduction discusses other research articles that have explored the relationship between ICP monitoring and mortality. This is important for multiple reasons:

1. Those that receive ICP monitoring are biased towards a more severe head injury, but no anticipated unsurvivable injury. Of course, the choice of ICP monitoring or not is up to the discretion of the treating neurosurgeon, and available resources (both material and human resources).

Reply 20:

- Selection bias for treatment is commonly found in real-world practice, we try to control selection bias by propensity score matching. As a result, we found that selection bias to ICP monitoring in this cohort comprised age, hypotension, pupillary light reflex, midline shift, and DAI.

Comment 21

A large caveat that I believe deserves recognition in your introduction is that of treatment based on ICP monitoring. ICP monitoring alone is not anticipated to reduce hospital mortality rates. Simply monitoring patients will not theoretically improve outcome. What is fundamentally important is that the treatment of ICP is targeted and informed by its monitoring. The clinician can therefore make informed decisions regarding medications, ventilatory settings, body position, etc. based on the ICP. Hence, the guidelines are based on keeping the ICP less than a rough threshold of 20mmHg – without monitoring, the clinician cannot be sure the ICP is less than 20mmHg. That is the strength of ICP monitoring. What you are trying to investigate here is whether inserting the invasive ICP catheter into the patient's brain tissue adversely affects mortality. Please recognise this important note in your introduction and discussion.

Additionally, state that this is invasive ICP monitoring. There is growing popularity in investigating non-invasive ICP monitoring in the sTBI field.

Reply 21:

- I revised the manuscript according to reviewer's suggestions in discussion

Changes in the text: Line 273-280 page 9

Comment 22

Methodology:

Can the authors confirm that their inclusion was GCS<9 (i.e. 8 and below, not including 9). If so, please rephrase to GCS≤8 as is custom. Additionally, clarify the age group – presumably this is the adult sTBI population only?

Reply 22:

- I revised the manuscript according to reviewer's suggestions
- The study population were adult that I revised in the manuscript.

Changes in the text: Line 70-74 page 3

Comment 23
Please specify the ICP catheter used.
Reply 23:
- I revised the manuscript according to reviewer's suggestions
Changes in the text: Line 1113 page 4

Comment 24
Can the authors explain why a p value of 0.1 was used in the univariate analysis, and not the standard 0.05 level?
Please state the parameter list used in the modelling. It is important for the readers to understand what parameters were used in this analysis (the specific parameters for each model are not required).
Reply 24:
- Based on article of Bursac et al. and Chowdhury et al. and other academic platforms, selection of variables has various methods. Screening variables in univariate analysis with $p<0.1$ has been used to put in the multivariable model. Our institute (Department of epidemiology) has been prefer this protocol to identify the candidate variables.

- Bursac Z, Gauss CH, Williams DK, Hosmer DW. Purposeful selection of variables in logistic regression. Source Code Biol Med. 2008;3:17. Published 2008 Dec 16. doi:10.1186/1751-0473-3-17

- Chowdhury MZI, Turin TC. Variable selection strategies and its importance in clinical prediction modelling. Fam Med Community Health. 2020;8(1):e000262. Published 2020 Feb 16. doi:10.1136/fmch-2019-000262
https://www.researchgate.net/post/Variable_selection_and_multivariable_logistic_regression_model_determination

https://stats.stackexchange.com/questions/601249/cox-proportional-hazards-why-p-0-1-in-univariate-to-be-included-in-the-multiv

- So, we added further citation about this topic in the revised manuscript.
- Table 4 demonstrated the parameters were used for adjustment in each PSM methods.
Changes in the text: Line 158-160 page 5

Comment 25
It is my understanding that the matched versus unmatched cohorts arose because there were factors outside of ICP monitoring that were different in both groups, and hence a meaningful analysis of the effect of ICP monitoring could not be determined. If this is correct, please make this clearer in the methodology section. If not, please explain what exactly the matched versus unmatched groups were.
Reply 25:

- If I analyze by traditional multivariable analysis without PSM methods, the result is not reliable from significant difference of various baseline characteristics (Table 1).
- PSM methods will be adjusted these baseline characteristics (Table 3). Therefore, the effect of ICP monitoring could be determined.
- I added this topic in the revised manuscript.
Changes in the text: Line 291-296 page 9

Comment 26
Results: Typographical error line 146
Reply 26:
- I edited according to reviewer's suggestions
Changes in the text: Line 181 page 6

Comment 27
Figure 1: please spell out abbreviations used in the figure in the figure legend and spell out the default R variable names.
Reply 27:
I revised the figure 1 according to reviewer's suggestions
Changes in the text: Figure 1

Comment 28
Table 1:
Please re-organise this table into subheadings – mechanism of injury, presenting clinical features, type of brain injury, medications used, etc. The current format is confusing. Why is road traffic injury separate to "car crash", "MCA", or "pedestrian"?
Reply 28:
- I revised table1 according to reviewer's suggestions
- Additionally, road traffic injury variable comprised car crash, MCA and pedestrian in the operational definition.
Changes in the text: Line 124-126, Table 1

Comment 29
One of the biggest concerns that you have not addressed in this paper is therapy/ treatments for sTBI patients. The primary objective for ICP monitoring is to advise ICP management with treatment strategies. You would need to look at the treatments given to patients with and patients without ICP monitoring – theoretically, there should be a difference between the two groups. Perhaps consider this as a sub-analysis after you have identified the matched cohort. This reviewer believes that this analysis is within the scope of this study, and should be feasible if this information was collected as part of the retrospective database.
Reply 29:
- I addressed the ICP management in the revised manuscript.
Changes in the text: Line 130-142 page 5

Comment 30
Theoretically, treatment would be a large contributor to mortality, and essentially another way to measure the utility of ICP monitoring in this critical care setting. Ideally, this centre's treatment strategy would be described and explained in this paper.
Reply 30:
- I addressed the ICP management in the revised manuscript.
Changes in the text: Line 130-142 page 5

Comment 31
Discussion:
Inherent bias to this study, as mentioned, is that patients with DAI were essentially more likely to receive ICP monitoring. As DAI is associated with worse outcomes, it would bias the group towards a higher mortality. Please discuss.
Reply 31:
- I revised this topic according to reviewer's suggestions
Changes in the text: Line 247-282 page 8

Comment 32
Please include recommendations for the future – what do the authors believe is a next step from here? Investigating adjusted treatment strategies based on ICP monitoring should be explored.
Reply 32:
- I revised according to reviewer's suggestions
Changes in the text: Line 301-305 page 10

Reviewer D Comments

Thank you for reaching out to review this work.

The authors aimed to evaluate the relationship between invasive intracranial pressure monitoring and hospital mortality in patients suffering from severe traumatic brain injury (GCS upon admission ≤8).

I think that the authors approached the problem the wrong way round, and this for a few reasons.

Comment 33
1. Given that the authors' country is an upper-middle income region, maybe they

don’t have the same access to invasive ICP monitoring compared to a high-income country. It would be interesting to known the proportion of severe TBI patients who were admitted to the intensive care unit but did not benefit from ICP monitoring (the authors report the use of ICP monitoring in 4.1% of patients admitted with severe TBI).

Reply 33:

- Survival benefit was not significantly different between two group in the present study.

Changes in the text: Line 291-300 page 9

Comment 34

There are clear guidelines for the use invasive ICP monitoring in patients suffering from head trauma, regardless of its seriousness upon admission:

-Picetti E et al. WSES consensus conference guidelines: monitoring and management of severe adult traumatic brain injury patients with polytrauma in the first 24 hours. World J Emerg Surg. 2019 Nov 29;14:53.

-Stocchetti N et al. Clinical applications of intracranial pressure monitoring in traumatic brain injury : report of the Milan consensus conference. Acta Neurochir (Wien). 2014 Aug;156(8):1615-22.

-Chesnut R et al. Participants in the International Multidisciplinary Consensus Conference on Multimodality Monitoring. Intracranial pressure monitoring: fundamental considerations and rationale for monitoring. Neurocrit Care. 2014 Dec;21

Thus, the research question should not be the relationship between intracranial pressure monitoring and mortality, but rather the factors associated with mortality in severe TBI patients, including the presence or the absence of invasive ICP monitoring.

Reply 34:

- In our understanding, Pecetti et al. is the online questionnaires study with modified Delphi approach that recommended ICP monitoring in severe TBI that preferred patients who had GCS of 8 or less (no all TBI patients with any GCS).

- Stocchetti N et al. proposed consensus of ICP monitoring in TBI and the following recommendations: 1) in comatose TBI patients, in case of normal computed tomography (CT) scan, there is no indication for ICP monitoring; 2) ICP monitoring is indicated in comatose TBI patients with cerebral contusions in whom the interruption of sedation to check neurological status is dangerous and when the clinical examination is not completely reliable. The probe should be positioned on the side of the larger contusion; 3) ICP monitoring is generally recommended following a secondary DC in order to assess the effectiveness of DC in terms of ICP control and guide further therapy; 4) ICP monitoring after evacuation of an acute supratentorial intracranial hematoma should be considered for salvageable patients at increased risk

of intracranial hypertension with particular perioperative features.

- Chesnut R et al. is the study of the literature review.

- I think in the current studies still has been debated and it not clear recommendation from evidence-based medicine for the use invasive ICP monitoring. Therefore, if we demonstrate the relationship between intracranial pressure monitoring and mortality, the result leads to one of evidence for decision making to perform the ICP monitoring in the future.
Changes in the text: Line 86-91, 96-97

Comment 35
Consequently, I strongly disagree with the abstract statement « ICP monitoring has been recommended in guidelines for intensive care patients ».
Reply 35:
- I revised this statement according to reviewer's suggestions
Changes in the text: Line 34-35, 209-210

Comment 36
Conclusion of the review.
In its current form, this paper turns the research question in the wrong way which precludes drawing scientifically valid conclusions.
Reply 36:
- We tried to explore the relationship between ICP monitoring and mortality. From review of the literature, we found the lack of evidence about this research question. Although we conducted the retrospective study that have limitations from study design, we used propensity score matching approach for adjust confounder by statistical methods.
- As the authors' knowledge, the results from propensity score matching approach are more reliable than traditional multivariable analysis. So, we think this approach is not the wrong way to show the relationship between ICP monitoring and mortality because this approach is scientific method.
Changes in the text: Line 163-164 page 6

Comment 37
The paper needs proofreading from a native English speaker.
Reply 37:
- Proofreading the revised manuscript was performed by a native speaker.
Changes in the text: the revised manuscript

Reviewer E Comments

Thank you for the opportunity to review this manuscript.

I would also like to thank the authors for raising these important points concerning this matter. The use of propensity score matching is indeed a pertinent approach to alleviate the impact of confounding by indication. However, several concerns need to be addressed.

Comment 38
First of all, the sample size poses a significant problem. A pre-matching cohort of 35 patients in the ICP monitoring group is excessively small. Post-matching the sample size is remarkably inadequate for such comparisons, as indicated in table 3.
Reply 38:
- One of limitations of propensity score matching is the reduction of sample size. Therefore, we proposed various propensity score approaches that avoid to reduction of sample size, for example, propensity score covariate adjustment (Table 4).
Changes in the text: Line 281-290 page 9

Comment 39
Secondary there are issues related to the utilization of the GCS score. Questions arise about the timing of its acquisition, and it is not clear if patients were sedated or not.
Reply 39:
- Various extracranial causes lead to depressed GCS scores, such as hypoxia, hypotension, and medication. However, GCS score in the present study preferred to post-resuscitation GCS score with stable vital signs according to operational definition section. Additionally, sedative drugs should be avoided in TBI patient in general practice because these medications interfere to evaluate GCS score.
Changes in the text: Line 121-124 page 4

Comment 40
Lastly, the manuscript would benefit from language editing by a native English speaker. While I have highlighted some problems, there are more disruptive language issues that should be addressed for enhanced clarity and coherence.
Reply 40:
- Proofreading the revised manuscript was performed by a native speaker (Mr. Rachan Areelon).
Changes in the text: the revised manuscript

Comment41
Introduction
Line 63-64: Data from 2021 are available, I don't see the point in mentioning two numbers here; they are quite the same. In my opinion, you should either state if there

is a development which means including more years, or just state the more recent number.
Reply 41:
- I revised according to reviewer's suggestions
Changes in the text: Line 64,65 page 3

Comment 42
Line 73: I suggest that you cite the relevant guideline from 2017 rather than an editorial by Carney et al.
Reply 42:
- I revised according to reviewer's suggestions
Changes in the text: Line 130-142 page 5

Comment 43
Line 73: This sentence does not make sense, I am not sure if the authors refer to their "own" review within present study or if they propose that the cited articles are reviews. Either way, I suggest rewriting the sentence. Please make sure to cite the mentioned two papers appropriately.
Reply 43:
- I revised according to reviewer's suggestions
Changes in the text: Line 80-81 page 3

Comment 44
It is very fine to state the objectives, but I suggest including primary and secondary outcomes within the methods section.
Reply 44:
- I would like to state the objective in the last sentences of introduction section.
Changes in the text:-

Comment 45
Methods - Line 98: I find this sentence misleading, I would suggest something like "This was a retrospective cohort study based on data from medical records from a level 1 trauma centre in Southern Thailand."
Reply 45:
- I revised according to reviewer's suggestions
Changes in the text: Line 109-112 page 4

Comment 46
Line 98: Was it a Level 1 trauma centre?
Reply 46:
- Yes sir.
Changes in the text: Line 109-112 page 4

Comment47
Line 100: But didn't the study also include patients who were not subjected to ICP monitoring? It is unclear from this statement and should be clarified.
Reply 47:
- We excluded the two patients who underwent intraventricular ICP monitoring (ventriculostomy) to evaluate just intraparenchymal ICP monitoring.
Changes in the text: Line 116-118 page 4

Comment 48
Line 100: Was the GSC score evaluated on the scene (prehospitally)? Or at admission?
Reply 48:
- The patient's GCS score with stable vital signs after resuscitation at the emergency department (operational definition)
Changes in the text: Line 121-126 page 4

Comment 49
- Line 108: There are some methodological issues with this selection of GCS score, first, aren't the patients sedated and subjected to muscle relaxation based on the principles of neuroprotective management? Secondly, how was "stable vitals" assessed and by whom? Lastly, you cite an article based on a paediatric population who just did the same, this does not exactly qualify this quite peculiar method.
Reply 49:
- Various extracranial causes lead to depressed GCS scores, such as hypoxia, hypotension, and medication. However, GCS score in the present study preferred to post-resuscitation GCS score with stable vital signs by neurosurgeon according to operational definition section. Additionally, sedative drugs should be avoided in TBI patient in general practice because these medications interfere to evaluate GCS score.

- About study population, we revised about the sentences as follows: "The present study studied in patient aged 15 years or older who experienced severe traumatic brain injury (GCS score $\leq$ 8)"
Changes in the text: line 40-41,111-112 page 4

Comment 50
Line 116: Again, I don't like the use of review since it refers to another study design than this present study.
Reply 50:
- I revised according to reviewer's suggestions
Changes in the text: Line 147 page5

Comment 51
Line 119: use another word than treatment; monitoring is not a treatment per se, even though it is an invasive procedure.

Reply 51:
- I revised according to reviewer's suggestions
Changes in the text: Line 150 page5

Comment 52
Line 141: The authors report results using mean and standard deviation but do not mention if the data adhered to the Gaussian distribution or not. It would be surprising if factors like age, the thickness of SDH or diameter of contusion and midline shifts were all normally distributed.
Reply 52:
- Kolmogorov-Smirnov test was performed for normality test, and I revised according to reviewer's suggestions
Changes in the text: Line 174-175 page 6

Comment 53
Results
Line 148: Confusing language. Did 35% of all patients have a positive alcohol blood test? Were all patients tested? If not, what were the criteria for being tested? I do not know the unit mg% do the authors refer to mg/g or mmol/L or is it something else? Even without being sure about the units of measure, a standard deviation of 106 seems large, suggestive of a significant amount of variability within the alcohol levels. I do not think that means and SD is the most appropriate way to report this data.
Reply 53:
- Not all patient was tested (143/407), I revised it.
- We used median and IQR for blood alcohol level in table 1, however, we have to used mean and SD in table 3 because of R program.
Changes in the text: Line 186,Table 1

Comment 54
Line 155: Is it the presence of pupillary light reflex or the absence? and of both eyes or one eye? It's confusing.
Reply 54:
- pupillary light reflex was categorized into 3 groups for clinical proposes in neurosurgical field.
- fixed (absence) one eye prefers uncal herniation
- fixed both eyes prefers brainstem failure from any causes
- react (presence) prefers normal status

Comment 55
Table 3 shows that the groups end of being to small
Reply 55:
- One of limitations of propensity score matching is the reduction of sample size. Therefore, we proposed various propensity score approaches that avoid to reduction of sample size, for example, propensity score covariate adjustment (Table 4).

- We discussed about this topic in the revised manuscript.
Changes in the text: Line 281-290 page 9

Comment 56
Discussion
Line 191: ICP monitoring itself does not reduce ICP. Rewrite. Further, it looks like you are citing the third edition; please consider choosing the updated guidelines from 2017. Carney N, et al. Guidelines for the Management of Severe Traumatic Brain Injury, Fourth Edition. Neurosurgery. 2017 Jan 1;80(1):6-15. doi: 10.1227/NEU.0000000000001432. PMID: 27654000.
Reply 56:
- I revised as reviewer's suggestions
Changes in the text: Citation number 9, and line 78-80

Comment 57
Line 194-97: I agree these circumstances really flaws the study
Reply 57:
- I revised as reviewer's suggestions
Changes in the text: line 235-239 page 8

Comment 58
Line 198: How do you conclude that? Based on the increased likelihood of undergoing ICP monitoring in patients presenting with DAI? (table 2) This statement needs to be explained further.
Reply 58:
- I revised as reviewer's suggestions
Changes in the text: line 247-252 page 8

Comment 59
Line 213-14: This statement is written like the cited literature did not use "real-world" data; as mentioned, I do not like the term, you refer to observational studies (except for one survey, which does not make sense to include here) Please consider another way of expressing your findings in relation to the established literature.
Reply 59:
- I revised as reviewer's suggestions
Changes in the text: Line 281-290 page 9

Comment 60
You cannot draw such conclusions based on the available data and the design of the study. Beware of causal language for an observational study.
Reply 60:
- I revised as reviewer's suggestions
Changes in the text: Line 281-290 page 9

Comment 61
Figure 1 lacks an explanation of the abbreviation used in the labels of the x-axis.
Reply 61:
- I edited Figure 1 as reviewer's suggestions
Changes in the text: Figure 1

Comment 62
Supplementary
In the STROBE statement, item no 15: Controls are not selected based on their outcome – but on their exposure. It is a cohort study but not a case-control study.
Spelling etc
Reply 62:
- I edited it as reviewer's suggestion.
Changes in the text: No.15 of STROBE statement

Comment 63
Line 68. mild should not be capitalized.
Reply 63:
- I edited it as reviewer's suggestion.
Changes in the text: line 70 page 3

Comment 64
Line 78 – 81: Very long sentence; I suggest rewriting in a shorter, more concise version.
Reply 64:
- I revised it as reviewer's suggestion.
Changes in the text: line 86-87 page 3

Comment 65
Line 86 (and throughout the manuscript) is out-of-pocket the right term here? I am not a native speaker, but to me, it sounds very informal.
Reply 65:
- out-of-pocket expenditure (OOPE) is the formal term in the field of the health economic evaluation
- https://www.who.int/data/gho/indicator-metadata-registry/imr-details/4445
- https://www.ncbi.nlm.nih.gov/pmc/articles/PMC8147610/

Comment 66
Line 103: Don't you need a comma here? And is it arrival to the emergency department, the ICU or both?
Reply 66:
- I revised it as reviewer's suggestion.

Changes in the text: line 116-118 page 4

Comment 67
Reply 67:
Line 125: Shouldn't it be multivariable analysis here?
- I edited it as reviewer's suggestion.
Changes in the text: line 156 page 5

Comment 68
Line 145: The order within the sentence does not make sense. Rewrite.
Reply 68:
- I edited it as reviewer's suggestion.
Changes in the text: line 180-181 page 6

Comment 69
Line 147: I suggest using the same nomenclature accident implies that something happened involuntarily. I suppose the authors do not know that and therefore, I suggest using collision or injury. Please revise throughout the manuscript and within the tables as well.
Reply 69:
- I edited it as reviewer's suggestion.
Changes in the text: line 184 page 6

Comment 70
Line 150: the verb were is not appropriate.
Reply 70:
- I edited it as reviewer's suggestion.
Changes in the text:185 page 6

Comment 71
Line 153: The mean age was not older the patients were. Rewrite.
Reply 71:
- I edited it as reviewer's suggestion.
Changes in the text: 191 page 6

Comment 72
Line 167-68: Rewrite sentence
Reply 72:
- I edited it as reviewer's suggestion.
Changes in the text: line 194-195 page 6

Comment 73
Line 192: The last part of the sentence does not make sense.
Reply 73:

- I edited it as reviewer's suggestion.
Changes in the text: line 209-210 page 7

Comment 74
Line 193: The term "wildly" seems inappropriate here
Reply 74:
- I revised as reviewer's suggestions
Changes in the text: line 235-237 page 8

Comment 75
The use of the term intensive seems to be misunderstood, I suggest using invasive instead. I suggest avoiding the repeated use of "real life" in an observational study; this is implicit.
Line 231: plural or singular? Decide and revise.
Reply 75:
- I revised as reviewer's suggestions
Changes in the text: line 208-311 page 10

Reviewer F Comments

This manuscript evaluates the relationship between Intracranial pressure (ICP) monitoring and hospital mortality in severe TBI patients within the Thailand population. The authors used different propensity score matching (PSM) methods to adequately capture the effect on the outcome and eliminate possible bias Which makes the study very interesting. However, the study could be improved further if some shortcomings and clarification issues are addressed.

Comment 76
Specific comments:
Methods:
Is the aim of this paper to compare the different PSM methods to see which one is the best or to check the effect of ICP monitoring on mortality? The paper could benefit more if the research focuses on one and is discussed in detail.
Reply 76:
- I tried to perform various PSM methods and other propensity score adjustment to confirm the no association between ICP monitoring on mortality.
Changes in the text: line 163-164, 281-290

Comment 77
Could you explain why you used a p-value of <0.1 instead of the standard 0.05?
Reply 77:
- Based on article of Bursac et al. and Chowdhury et al. and other academic platforms,

selection of variables has various methods. Screening variables in univariate analysis with p<0.1 has been used to put in the multivariable model. Our institute (Department of epidemiology) has been prefer this protocol to identify the candidate variables.

- Bursac Z, Gauss CH, Williams DK, Hosmer DW. Purposeful selection of variables in logistic regression. Source Code Biol Med. 2008;3:17. Published 2008 Dec 16. doi:10.1186/1751-0473-3-17
- Chowdhury MZI, Turin TC. Variable selection strategies and its importance in clinical prediction modelling. Fam Med Community Health. 2020;8(1):e000262. Published 2020 Feb 16. doi:10.1136/fmch-2019-000262

https://www.researchgate.net/post/Variable_selection_and_multivariable_logistic_regression_model_determination

https://stats.stackexchange.com/questions/601249/cox-proportional-hazards-why-p-0-1-in-univariate-to-be-included-in-the-multiv

- So, we added further citation about this topic in the revised manuscript.
Changes in the text: line 160-161

Comment 78
Line 126-127, these variables were analyzed using multivariable analysis with the backward stepwise procedure. Is this a model-fitting step or a variable selection option?
Reply 78:
- Based on article of Chowdhury et al. and other academic platforms, selection of variables has various methods.
Changes in the text: line 160-161

Comment 79
Line 40, 849 TBI patients with Glasgow Coma Scale scores less than 9 were included retrospectively. However, Severe TBI is less than 8 in the GCS, and it will be nice to motivate the expanded range.
Reply 79:
- I revised as reviewer's suggestions
Changes in the text: line 40, 112,133

Comment 80
Results:
Line 49, after PSM, ICP monitoring did not affect mortality in all PSM methods. Did you try to correct with DAI to see if the results changed?
Reply 80:
- After matching, DAI was adjusted before analysis effect of ICP monitoring.

Comment 81
Line 195, Because the cost of the ICP monitoring catheter is not covered by all public health insurance schemes in Thailand ICP monitoring has been conducted less frequently than in previous studies. Have you tried to look at other confounders like insurance status/ type or socioeconomic status?
Reply 81:
- We did have data of socioeconomic status because study designs, So we discussed this topic in limitation
Changes in the text: line 301-305 page 10

Comment 82
Other:
The manuscript would benefit from language and grammar revision.
Reply 82:
- The revised manuscript was proofread by Mr. Rachan Areelon.

Comment 83
Explain Figure 1, what are we looking at and how should we interpret the plot.
Reply 83:
- I revised Figure 1 as reviewer's suggestions
Changes in the text: line 223-226 page 7

Comment 84
Table 1: The Mechanism of injury shows that your population is so different that you might not have a meaningful comparison no matter what you do. Did you try to use a more comparable group, say just MCA patients?
Reply 84:
- I would like to describe details of injury mechanism in this cohort. Therefore, MCA, car crash, and pedestrian injury were included as road traffic injury.
Changes in the text: line 125-126 page 4