Reviewer's report

Title: Patterns, aetiology and risk factors of intimate partner violence-related injuries to head, neck and face in Chinese women: A retrospective study

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Reviewer: Sheila Sprague

Reviewer's report:

I would like to thank the editor and the authors for the opportunity to review this interesting study. The current study is a retrospective analysis of medical charts of patients who reported that they had experienced injuries due to intimate partner violence in Hong Kong.

General Comments:

The current study is a well-designed retrospective study of medical charts. The paper needs more detail and description, especially in the introduction and methods sections. The statistical analysis plan is unclear and should be significantly revised. The overall study will be of modest impact in the IPV field.

Specific Comments:

Major Compulsory Revisions

Abstract

1. In the findings section of the abstract, it is unclear what the comparator is. For example, “punching with a fist was exerted significantly more on the upper third of the maxillofacial region”. What is this compared to?

Introduction

2. The first paragraph of the introduction nicely introduces the concept of IPV and the consequences of HNF injuries. The remainder of the introduction could be reorganized to include a discussion of why HNF injuries specifically are important in IPV. A bit more background on the work done in the HNF injury field would be helpful along with a description of current guidelines (or lack thereof). Dentistry has a large amount of literature on HNF injuries and IPV.

3. The introduction does not make a case for why it is important to determine patterns of injury and etiology. Some information on this would really let the reader know why this particular study will make an impact.

4. Paragraph 1 line 4-5 states that “Of IPV-related physical injuries, over 90% have been found to involve the head, neck or face...” The first reference listed (Monahan & O’Leary, 199) reports that 35% of physical IPV injuries involve the head. Another study by my research group (Bhandari M, Dosanjh S, Tornetta P 3rd, Matthews D. Musculoskeletal manifestations of physical abuse after intimate partner violence. J Trauma. 2006 Dec;61(6):1473-9) reports that 40% of physical...
IPV injuries are HNF related. This is not an exhaustive search so I suggest that the authors complete a more thorough search of the literature on the topic and this sentence should be revised to reflect current literature.

Methods

5. Overall, the study is a well-designed retrospective study but the manuscript lacks important details. In the eligibility criteria section, the authors should state how many charts they reviewed and how many were excluded and the reasons for exclusion to give the reader an idea of how much bias could have been introduced into the screening and eligibility process. The use of two reviewers for eligibility, however, is excellent.

6. Paragraph 3 of the data abstraction section states that the ISS was used for determining severity. It is unclear how the authors applied the ISS to determine the severity. Did the ISS include all of a patient’s injuries or just the HNF injury? This should be clarified within the manuscript.

7. I have many questions about the data analysis, which overall requires more description. The data analysis paragraph states that a logistic regression was conducted for each region of injury. More information is needed on the nature of the analysis. For example, how many factors went into the model, which factors, and how did the authors decide which factors were used? Was a stepwise method used or another method? It is unclear what the comparator was in the regression analysis. Did the analysis compare patients with HNF injuries and those without? The authors should state this explicitly.

8. There is no mention of a sample size calculation or power analysis. The authors should state their rationale for including 223 charts.

9. Was the data analysis plan determined a priori or after the data was collected? I am concerned that there are a large number of analyses and no mention of any corrections for multiple testing. The authors did not provide a rationale for why so many analyses were carried out.

Results

10. Table 2 could have been done with a chi-square analysis instead of a regression. What is the rationale for a regression? It is unclear what this table adds to the paper.

11. Table 3 should include an indication of how many patients were included in the analysis (N).

12. It is also unclear what table 4 adds to the paper. Why not just add “multiple injuries” as a factor in the original regression. The authors may have issues with being underpowered for the analysis in table 4.

Discussion

13. The discussion states that “Based on the natural selection theory, cohabiting couples with lower levels of violence tend to move from cohabitation to marriage,
whereas couples with higher levels of violence tend to stay in cohabiting relationships.” I don’t believe that this is true. Often, violence men are not violent while they are dating then become violence once married as she is now “under his control”. This certainly doesn’t apply to many Eastern cultures such as India, where dating and cohabiting are less popular. One could also argue that cohabiting relationships are easier to leave than marriages, therefore there should be less violence in cohabiting relationships. Regardless, this sentence needs to be rewritten or requires a reference.

14. Paragraph 4, line 1 states that “IPV-related injuries tended to occur repeatedly among abused women.” This sentence is unclear and imprecise. The sentence should be rewritten to note that this is for physically injured IPV victims who present to AEDs, not the general population of abused women.

15. The conclusion states that the findings are “essential to help clinicians to improve diagnosis” for IPV victims. It is unclear how knowing about these factors will help diagnose victims. Many attempts have been made to create a predictive model of determining who is at risk for IPV, but none have been sensitive or specific enough. How does this study add to that body of literature? Some discussion of these past attempts would be beneficial.

Minor Essential Revisions

16. Introduction Paragraph 2, line 1: incorrect use of the word “despite”. The wording should be revised to make the author’s point clearer.

Discretionary Revisions

17. The first paragraph of the results section states that only three quarters of the participants were Chinese. Was being Chinese not an eligibility criterion? If not, then can this really be called a study of Chinese women? Perhaps the authors should rephrase the title.

18. In the Pattern of HNF Injuries section, it may be helpful for the authors to report actual numbers as well as percentages in this paragraph.

19. Figure 1 is very difficult to read in greyscale. Will this be published in colour? If not, the authors should consider making the figure into a table for the reader’s convenience.

20. Discussion Paragraph 4, line 4-5: The sentence can be reworded to include the next part as follows, “It is likely that the women will return to their abusive partners due to a variety of reasons such as, financial dependence, emotional dependence, and protection of their children.”

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

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:  
I declare that I have no competing interests