Reviewer's report

Title: Resource Utilization and Outcomes of Intoxicated Drivers

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Reviewer: David S Plurad

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While this work presents some interesting results, the draft in its current form does not support the authors' final conclusions. The supporting data would be fine for a descriptive paper; however, in an effort to lend credence to their hypotheses, the manuscript is over-reaching with the data they provide. To adequately support findings of significant difference between two groups, logistic regression analysis should be performed at a minimum. From the manner in which these data is presented, it appears that this was not done. There are two solutions; (1) perform logistic regression culling additional variables from the database for which to meaningfully compare the two groups, or (2) simply report that this is retrospective descriptive study. However, option (1) would probably more readily result in publication.

Background:
This section can be shorter by 2-3 paragraphs. Also, the hypothesis can be made clearer to the reader. The reason it is unclear is that the authors attempt to define 3 endpoints. This is a noble undertaking but it is much easier to prove or disprove significant changes in one outcome endpoint than 3. You can clarify by defining one primary endpoint and other secondary endpoints. The secondary endpoints may or may not need to be vigorously supported in the manuscript but the primary endpoint should be. My recommendation would be pick one endpoint (ISS, outcomes or utilization) and strongly support that with the data (ie: with logistic regression) and simply report your secondary endpoints after only bivariate analysis.

Methods:
The abstract is missing a methods section. In reality, the study population (as I can tell) is trauma admissions that have had an ETOH level drawn and not the # of drivers. This should be the study “n” and be reported consistently through the manuscript. You can report the % of patients from the total that had ETOH levels drawn but from the way the methods are written, it is not clear who the study population is. This is a weakness of the study as well discussed in the manuscript.

Are we truly sure that all the victims of the MVC’s are the drivers? Could they have been passengers? If so, your findings may not be altered since, presumably, both are subjected to roughly the same traumatic insult. However, it is again important to define the study population definitively.
Along this same issue, it is not clear how the authors are defining the comparison groups. The ETOH > 80 group are those with a level of 80 mg/dl while the ETOH = 0 group had no detectable ETOH. Is this really true? Then, those with an ETOH level 0-80 mg/dl were excluded. This is OK but that must be stated in the methods. This is clinically relevant since it appears that those with (+) ETOH but < 80mg/dl are different from those without a detectable ETOH level and those with >= 80-mg/dl. Most notably Plurad D [1] et al. and Talving [2] et al. show that outcomes in after MVC and isolated severe traumatic brain injury are significantly different depending upon the level of ETOH and not simply upon the presence or absence of serum ETOH.

Resource utilization is difficult to define but these authors make a good attempt. The authors do report charges in a dichotomous fashion (table 4). This is acceptable but this reviewer, as well as many readers, prefer that this variable be reported as continuous data and as a comparison of means +/- standard deviations. Further, it would have been advantageous to report actual $ amounts collected/ patient between the two groups. Particularly with regard to the economic data, the results are mixed. One is still left with the question, “which group actually costs less to take care of?” While the ETOH 80 group appeared to use less ICU, ventilator days, and less time in the ED, their compensation rates to the hospital were lower and were more frequently admitted to the ICU. Did these “cancel each other out?”

Results:
See above with regard to defining and reporting the study population. The “study n” is 987 and not 1,732 (# of “drivers’). The manuscript describes the “study group” as those that are ETOH > 80. This is not correct strictly speaking.

The issue of ICU admissions and LOS is again somewhat confusing and results mixed. While the ETOH 80 group had shorter LOS, more were admitted. It would have been more meaningful to report ICU days/ patient. We are still left to wonder if this “ICU utilization” is comparable between the groups. I am sure we could figure it out but why not make it easier on your reader. There is a similar issue regarding resource utilization (see above).

Complications and mortality are compared next. For unclear reasons, DVT and coagulopathy are reported. What other complications were determined? The trend toward decreased mortality in an interesting finding and should have been mentioned in the abstract. It is OK that it is not statistically significant. It is still and interesting finding. Overall, however, the veracity of these findings is limited by the lack of formal multivariable logistic regression analysis.

Discussion:
The concept of falsely elevated ISS is a difficult one to prove particularly since it is an anatomical score. I would suggest leaving it out of the discussion since there is much more pertinent and “provocative” data available. [1-2] How would you explain the higher rate of intubation in the ETOH > 80 group. One would assume that they had an overall decreased GCS due to intoxication. However, this variable is not reported and the reader is left to assume and, subsequently,
the conclusions are weakened.

The authors report, “we are aware of one other published report suggesting that alcohol …may be protective.” This is inaccurate: please see references. Should the authors review these reports, they may be able to find similarities with their data regarding a survival advantage.

The final conclusions are not fully supported by their data in the way it is presented. Logistic Regression should be performed to deduce how the significant difference (if they exist) impact upon the reported outcomes. Without this analysis, the manuscript is simply a descriptive study on ETOH intoxication after MVC’s.

References:


 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'