Author's response to reviews

Title: Resource Utilization and Outcomes of Intoxicated Drivers

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Version: 2 Date: 7 April 2010

Author's response to reviews: see over
Editor-in-Chief, *Journal of Trauma Management & Outcomes*

**RE: Reviewer's report**

**Title:** Resource Utilization and Outcomes of Intoxicated Drivers  
**Version:** 1  
**Date:** 21 February 2010

Dear Editor-in-Chief,

I have carefully read the comments made by the reviewers and have attached to this letter the suggested changes made to the manuscript.

The authors wish to thank you and the reviewers for the time and effort made to evaluate this manuscript.

Respectfully submitted,

Robert A. Cherry, MD, MS, FACS
Reviewer: David S Plurad

Reviewer's report:
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.

The feedback regarding the statistical analysis and interpretation of the manuscript is greatly appreciated. Multivariable logistic regression analysis was not performed in this study. The culling of additional numerical or categorical predictor variables to determine the probability of occurrence is certainly credible. Practically speaking, we believe that this would best be performed as a prospective study because of the limitations inherent in a retrospective, registry-based study. The prospective collection of variables such as vehicle speed and type, daytime vs. nighttime vehicle operation, seat belt use, airbag deployment, driver experience, and distractions (ex. number of occupants) would also be valuable. For these reasons, we concur that the study should be categorized as a retrospective descriptive study. (See abstract and p. 4, highlighted copy)

We have also made revisions to the limitations of the study. (See p.9, highlighted copy)

In addition, we agree that any associations linked to differences between groups must be interpreted with caution and should not be “over-reaching” or appear to be definitive. We have therefore made revisions to the language used in the interpretation of the results. We have avoided the use of conclusive terms such as “significant”, “results”, and “findings”. Instead, we are using words such as “observed”, “described”, and “apparent” in the context of “supporting data.” We have modified or deleted statements that draw inferences or are over-reaching. These specific changes are both highlighted and underlined throughout the manuscript, as opposed to other changes which are highlighted only.

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.
The background section has been shortened to improve the flow for the reader and the context for the study. (See pp 3-4, highlighted copy)

We have identified outcome as the primary endpoint and describe the resources utilized (secondary endpoint) to achieve those outcomes. (See abstract and p. 4 highlighted copy.)

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?”

The abstract now has a methods section.

The study population is the number of drivers who have had an ETOH level drawn and had either 0 mg/dl or > 80 mg/dl (n=987). This has been clarified in the abstract and on page 6, highlighted copy.

Drivers were identified and differentiated from passengers based on EMS and/or emergency department records. This has been clarified on page 5, highlighted copy.
We agree that passengers and drivers may be subject to the same traumatic insult. The purpose of this study, however, is to describe the outcomes of the individual who was most accountable for the motor vehicle crash (i.e. the intoxicated driver), and the resources utilized to achieve those outcomes. (See abstract and p. 4, highlighted copy)

We have clarified that drivers without an ETOH level or those who had levels between 0 mg/dl and 80 mg/dl were excluded from the study (see pages 5, highlighted copy)

The references by Plurad and Talving have been added to the discussion on pages 8-9, highlighted copy.

Table 4: The variables are reported in a dichotomous fashion because many hospital Chief Financial Officers are interested in knowing how many “high dollar” cases present to their institution. The breakpoints listed for charges and costs are often used to define high dollar cases at our facility.

Table 4: We agree that the actual revenue collected would be ideal. However, this data has been difficult to access at our institution. Therefore we do not have profit/loss data for each group and are using charges as an indirect marker for actual revenue.

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.

We have clarified that the ‘study n’ is 987 on page and have deleted references to the “study group” as those with an ETOH > 80 (abstract and page 6, highlighted copy).

The point about reporting ICU days / patient because the ETOH group had more admissions to the ICU was rather interesting to us. Part of the limitation of this study is the fact that it is retrospective and registry based. We only included those patients that were considered registry qualifiers in our state. We do not know the actual number of patients that may have been treated and released, with or without a trauma evaluation. Therefore, we do not know the true ICU days / patient for either group. This limitation has been described on page 11-12, highlighted copy.
We compared all complications included in our trauma registry for differences between groups, and reported DVT and coagulopathy as the only variations. We agree that this gives the manuscript a tangential feel relative to the primary and secondary endpoints of the study. Differences in DVT and coagulopathy have therefore been removed. (See pages 6-7, highlighted copy)

We have included the trend in decreased mortality in the abstract. (See highlighted copy)

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:

We have removed the discussion about falsely elevated ISS. (See page 9 and 10, highlighted copy)

We believe that an explanation for the higher rate of intubation is beyond the scope of the data presented. We have clarified in our discussion that future studies should consider propensity scoring models that would predict the need for intubation and prolonged mechanical ventilation, and then assess for differences between intoxicated and non-intoxicated drivers. Variables for propensity scoring might include age, gender, ISS, abbreviated injury score for the head, Glasgow Coma Score, admission blood pressure and arterial pO2 (or oxygen saturation), and the presence or absence of rib fractures. This has been included in the discussion on page 12, highlighted copy.

The following statement has been deleted: “we are aware of one other published report suggesting that alcohol …may be protective.” (See page 11, highlighted copy)
We have reviewed the references by Plurad D [1] et al. and Talving [2] et al. The reviewer correctly pointed out, based on these studies, outcomes 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. This has been included in the discussion on pages 8-9, highlighted copy.

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'

Reviewer's report
Title: Resource Utilization and Outcomes of Intoxicated Drivers
Version: 1 Date: 26 February 2010
Reviewer: James Cushman

Reviewer's report:
1. Major Compulsory Revisions: None
2. Minor Essential Revisions: None
3. Discretionary Revisions:
The manuscript may be strengthened if the following questions are addressed in the discussion:
1. Of the 745 patients not tested for alcohol, what percentage were transferred from outside facilities and is there admission alcohol level data available for these patients?

Interfacility transfers were excluded from the study. This is mentioned in the methods section. (See page 5, highlighted copy)

2. Why were the remaining patients, i.e. those of the 745 that were direct admits but without alcohol level data, not tested for alcohol? Did your institution selectively test trauma admits for alcohol during the time of this study, and if so, is that process the same now?

“At our institution, blood alcohol concentrations are obtained on suspected cases of alcohol intoxication if the laboratory finding might affect therapeutic decision-making or result in a referral to a drug and alcohol counselor.” This statement is found in our methods section. There is no change in our current practice. However, blood alcohol testing may become mandatory at our trauma center later this year. (See page 5, highlighted copy)

3. How many of the EtOH >80 patients have an ISS score of 0 (i.e. no AIS injury?). It appears you compared means of ISS scores which might alter the analysis of the data if there was a large number of uninjured, but intoxicated patients in this group.
This is a good question. There were no patients with an ISS score of O. This is because we included only those patients considered registry qualifiers in our state.

**Level of interest:** An article of importance in its field  
**Quality of written English:** Acceptable  
**Statistical review:** No, the manuscript does not need to be seen by a statistician.  
**Declaration of competing interests:**  
I declare that I have no competing interests.