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Title	Temporal trends and differences in trauma centre hospital mortality across Ontario from 2005 to 2011
Authors	David Gomez MD PhD, Aziz S. Alali MD PhD(c), Barbara Haas MD PhD, Wei Xiong MSc, Homer Tien MD MSc, Avery B. Nathens MD PhD
Reviewer 1	Mark Hemmila
Institution	
General comments	Do you have insight into why there was a decline in mortality for Shock in ED and then a rise back to the 35% range. Was there a clinical trial taking place?
Reviewer 2	Dylan Pannell MD PhD CCFP
Institution	Canadian Forces, Health Services
General comments	Dr. Gomez, this is a very interesting study demonstrating some key variability in trauma centre mortality and therefore areas for improvement. However, as the authors have chosen not to identify the trauma centres specifically it places limitations on the ability of the individual institutions to decrease mortality amongst elderly patients and those with isolated TBI. Is there a process by which the centres evaluated in your study could have access to their performance in these areas in order that they may either develop instititional QI processes? By allowing the individual trauma centres to identify themselves in your study it may allow them to benefit from targeted strategies within their institution as well system-based strategies. Dylan Pannell MD PhD CCFP Captain Royal Canadian Medical Service and Division of General Surgery University of Toronto
Reviewer 3	Robert Stenstrom
Institution	St. Paul's Hospital, Emergency Medicine
General comments	This paper describes a large retrospective cohort/administrative database study of in-hospital trauma mortality in nine Ontario trauma centers between 2005 and 2011. Data for 26,421 adult admissions to these centers was obtained from the Ontario Trauma Registry Comprehensive Dataset. The stated objectives were: Primary: to evaluate differences in trauma center-related mortality over time. Secondary: To identify trauma centre-specific mortality to determine the extent of variation across centers. Findings were that trauma related mortality at these centers decreased over the study period and that there were differences between trauma centers, particularly for patients with isolated traumatic brain injury and elderly trauma patients The study is well designed, and the statistical methodology is rigorous. The conclusion that there has been an overall improvement in trauma center mortality over the study period is supported by the data and their analyses. That there is variability in survival between trauma centers, particularly with regard to isolated traumatic brain injury and trauma in elderly patients is also supported. What is less clear is whether these differences are attributable to something intrinsic to the trauma centers (practice patterns, quality of care provided, etc) and therefore modifiable, or extrinsic factors (referral patterns, transfers, time from injury, etc) that are not modifiable. Based on the study design and data source, it is impossible to determine which the case is or if it is a combination of intrinsic and extrinsic factors that explain the between-center differences. The argument that primarily intrinsic (modifiable) factors are responsible for the differences in mortality between centers would be strengthened if the reported relationships held in a secondary analysis comparing the two centers with the greatest difference in mortality (centers 1 and 9) and by excluding transferred patients. More specific comments: 1. The primary outcome of in-hospital mortality is vague in that there is no time period specified. Is it 28 day mortality, 60 day mortality, or all in-hospital deaths (in which case, what is the range of time when death occurred?). 2. Given the two major objectives of the study (trauma mortality over time and differences between trauma centers), the interaction between time (year of study) and trauma center should be elucidated by including an interaction term in the models. 3. It is unclear why the authors present the differences in trauma center mortality (summarized in table 2). They use "lower than, expected, and greater than expected mortality". Why not just report the actual odds ratios, from the models, highlighting those where the 95% confidence intervals do not include the null value?

	Also, based on table 2, it appears that two centers (1 and 9) account for much of the variability for in-hospital mortality. Some elucidation (via direct comparison of these two centers) of reasons for these differences should be described. Do does center 9 (higher than expected overall, multisystem, elderly, and isolated TBI trauma) receive more transfers than center 1, or are there other differences between these two sites specifically that could explain mortality disparity? 4. Patient transfers from other centers accounts for almost ½ of all subjects in the study. Patient transfers represent a very different group of subjects because they have survived long enough to be transferred (and more time will have lapsed since their injury, and they will have received their initial care outside the receiving trauma center – especially for the crucial "golden hour" of initial trauma treatment) , but are also sick enough to require transfer. Criteria for transfer likely differ between trauma centers and referring hospitals, depending on geographic catchment area, etc. Given the large number of subjects in this cohort (with the luxury of a large sample size), it would be more compelling if their results hold when transfers are excluded all together, since this group's data to some extent is "contaminated" by the fact that they have received some of their care elsewhere. It is unclear when the data found in the OTR-CDS is recorded – is it when the patient arrives at the trauma center? If this is the case, then the vital sign data and (m)GCS data for transfer patients will be qualitatively different than de novo arrivals to the trauma center, since more time has elapsed since the patient's injury. For example a (m)GCS of 2, two hours after injury versus 24 hours after injury are clearly not comparable. Although transfer status is included in the evaluation of trauma center specific mortality, the model generates an "average effect" across all of the centers. Because of the nature of the design of this study, residual confounding cannot be ruled out. The authors should at least run the analyses without transfer patients included to see if the same relationships hold. 5. The fit of the logistic and hierarchical logistic models was not assessed beyond examining observed-versus-predicted outcome plots. 6. How do the authors explain the finding based on Figure 1 that trauma mortality in patients with shock in the emergency department goes from about 38% in 2005 to 25% in 2008, then back up to 35% in 2011?
Author response	**Reviewer 1 comments:** Issue addressed above. **Reviewer 2 comments:** 1. Is there a process by which the centres evaluated in your study could have access to their performance in these areas in order that they may either develop instititional QI processes? By allowing the individual trauma centres to identify themselves in your study it may allow them to benefit from targeted strategies within their institution as well system-based strategies. At this time such process does not exist and all data were de-identified; only encrypted center identifiers were present. Our intent at the provincial level is to provide feedback and we are just at the nascent stages of this process **Reviewer 3 comments:** 1.The primary outcome of in-hospital mortality is vague in that there is no time period specified. Is it 28 day mortality, 60 day mortality, or all in-hospital deaths (in which case, what is the range of time when death occurred?). Issue addressed above. 2. Given the two major objectives of the study (trauma mortality over time and differences between trauma centers), the interaction between time (year of study) and trauma center should be elucidated by including an interaction term in the models We agree with the reviewer. Interaction terms were explored during model development and were found to not be significant. 3. It is unclear why the authors present the differences in trauma center mortality (summarized in table 2). They use "lower than, expected, and greater than expected mortality". Why not just report the actual odds ratios, from the models, highlighting those where the 95% confidence intervals do not include the null value? The reviewer brings up an important issue. Odds ratios with their 95% confidence intervals have been added to figure 1 (see above). 4. Based on table 2, it appears that two centers (1 and 9) account for much of the variability for in-hospital mortality. Some elucidation (via direct comparison of these two centers) of reasons for these differences should be described. Do does center 9 (higher than expected overall, multisystem,

	elderly, and isolated TBI trauma) receive more transfers than center 1, or are there other differences between these two sites specifically that could explain mortality disparity? We agree with the reviewer. The case of trauma centers #1 and #9 is of particular interest. After excluding transfers (sensitivity analyses shown above) both centers persisted to be outliers across most patient subgroups. Reasons for these differences are unclear at this point as data on varying structures and processes between these centers was not available at this time. This is an area of particular interest to my group and we will focus future research efforts towards identifying the source of this variability. This will likely require a mixed-methods approach. 5. Patient transfers from other centers accounts for almost ½ of all subjects in the study. Patient transfers represent a very different group of subjects because they have survived long enough to be transferred (and more time will have lapsed since their injury, and they will have received their initial care outside the receiving trauma center – especially for the crucial "golden hour" of initial trauma treatment) , but are also sick enough to require transfer. Criteria for transfer likely differ between trauma centers and referring hospitals, depending on geographic catchment area, etc. Given the large number of subjects in this cohort (with the luxury of a large sample size), it would be more compelling if their results hold when transfers are excluded all together, since this group's data to some extent is "contaminated" by the fact that they have received some of their care elsewhere. As suggested by the reviewer, sensitivity analyses were performed and are included in the revised manuscript. 6. It is unclear when the data found in the OTR-CDS is recorded – is it when the patient arrives at the trauma center? If this is the case, then the vital sign data and (m)GCS data for transfer patients will be qualitatively different than de novo arrivals to the trauma center, since more time has elapsed since the patient's injury. For example a (m)GCS of 2, two hours after injury versus 24 hours after injury are clearly not comparable. The reviewer is correct; data is recorded upon arrival to the trauma center. A transfer flag was included in all adjusted analyses. 7. Although transfer status is included in the evaluation of trauma center specific mortality, the model generates an "average effect" across all of the centers. Because of the nature of the design of this study, residual confounding cannot be ruled out. The authors should at least run the analyses without transfer patients included to see if the same relationships hold. As suggested by the reviewer, sensitivity analyses were performed and are included in the revised manuscript. 8. The fit of the logistic and hierarchical logistic models was not assessed beyond examining observed-versus-predicted outcome plots. Initially we ran a logistic regression model in which the discriminative ability was estimated using the c-statistic and the model fit was evaluated using the Homer-Lemeshow test. Calibration of subsequent multilevel models was assessed using observed-versus-predicted outcome plots. 9. How do the authors explain the finding based on Figure 1 that trauma mortality in patients with shock in the emergency department goes from about 38% in 2005 to 25% in 2008, then back up to 35% in 2011? Issue addressed above.