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Letter to the Editor

Regional variation in cardiac arrest for patients with sepsis

To the Editor,

We read with great interest the recent paper published by Desai et al. regarding their analysis of the regional trends in cardiac arrest for patients with sepsis.[1] We complement the authors on an interesting study however there are several limitations in the methodology of the manuscript, along with database limitations that need to be highlighted.

Firstly, there were significant changes in the National Inpatient Sample (NIS) during the study period that require acknowledgement as limitations. In 2007, the NIS captured data from 1044 hospitals, whereas the 2014 sample captured data from 4411 hospitals.[2] Additionally, the NIS was completely re-designed for the 2012 sample year, switching from collecting 100% of the admissions to 20% of hospitals to capturing 20% of admissions to all hospitals. Any trend analyses using the NIS which spans over this redesign must at least acknowledge the fundamental changes to the NIS sampling frame may have influenced the observed results. The authors attempt to characterize the incidence of cardiac arrest; however, they do not display the denominator for the subgroups of patients studied. This crucial information is required especially given the changes made to the NIS during the study period.

The statistical analysis presented by the authors is significantly limited by the lack of a multivariate analyses. The wide range of prevalence's of co-morbidities by region highlights the heterogenous patient population analyzed. In Table 2, every single co-morbidity, and severity of illness indicator was statistically significantly different across all regions, thus making any conclusion regarding outcomes with an unadjusted model dubious at best. A multivariate analysis would have allowed for stronger conclusions to be drawn about outcomes after adjustment for the many clinical factors associated with poor outcome from cardiac arrest. The authors also do not state that the appropriate survey techniques were employed while analyzing the NIS, which could affect the variances and confidence intervals around point estimates. In Figure 2a, no confidence intervals or error bars are displayed on the graph. Did the trend analysis include these confidence intervals and variances?

The authors state in the discussion that Do Not Resuscitate status is not available in the NIS. This is untrue, it is coded as V4986. Additionally, palliative care referral can be coded via V667, which may have been interesting to include in the analysis. While we think the hypothesis of this paper is interesting and of clinical interest, the lack of a rigorous statistical analysis precludes any strong belief in the conclusions drawn by the authors.

Conflict of interest

No authors declare any conflicts of interest

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