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**Title:** Use of linked data to assess the impact of including out-of-hospital deaths on Canadian national 30-day in-hospital mortality indicators: a retrospective cohort study

**Authors:** Ania Syrowatka PhD, Mingyang Li MD PhD, Jing Gu MD PhD, Ling Yin MD MSc, Danielle Rice MSc, Yana Gurevich MD MPH

**Reviewer 1:** Dr. Paul Hebert

**Institution:** Ottawa Hospital Research Institute

General comments (author response in bold)

1. I commend CIHI and the research team for undertaking this work. Adding out-of-hospital mortality is essential in the coming years.

**Thank you. We appreciate the positive feedback.**

2. A focus on total mortality is critical in our current health care environment.

**We agree.**

3. The background to this study does clearly outline why a focus on total mortality is critical at this time.

**Thank you.**

4. Total mortality at 30 days and beyond are patient centered rather than in-hospital mortality that is institution centered. This is critically important as many of our health systems start moving the original institutions to the community or other institutions. In addition, community services have deteriorated so adverse outcomes may be increasing. Therefore I might suggest that you indicate that there is an ongoing shift in care from primary institutions to other care sectors.

**We have revised the following sentence to the introduction to add this content: “Likewise, in Canada, inclusion of both in-hospital and out-of-hospital deaths may provide a more comprehensive *and patient-centered* assessment of health system performance, which is becoming increasingly important as hospital lengths of stay are becoming shorter as care shifts from inpatient to other settings.”**

5. In terms of sampling strategy, I am unsure why AMI, stroke and surgery were chosen other than convenience. I might suggest that you better justify your choice of the 3 clinical programs.

**We have modified the objective as follows: “This study focused on three key *mortality* indicators reported by CIHI: 30-Day Acute Myocardial Infarction In-Hospital Mortality, 30-Day Stroke In-Hospital Mortality, and Hospital Deaths Following Major Surgery.”**

6. The increase in mortality (from in-hospital to total mortality) is clearly very significant. This said, it is significant because it is patient centered and sensitive to all care provided during a critical time early post event.

**We have added the following sentence to the interpretation: “These increases in mortality rates are important because they are patient centered and sensitive to all care received during the critical time following AMI, stroke or major surgery, and more accurately reflect the outcomes of these major conditions.”**

7. I might suggest that the comparisons between the two time periods is not very strong. If you wish to continue with a comparison amongst 3 major conditions across so many geographies between 2 time periods, I might suggest multivariable analysis to strengthen all inferences. Without it, I do not believe any of the inferences hold related to shifts between in-hospital and total mortality

**We have removed the inferential statistics based on the suggestions of the statistician who reviewed the manuscript.**

8. I believe that the first and last paragraph of your interpretation section are inconsistent. On the one you argue that out-of-hospital mortality added little to in-hospital mortality. On the other hand, you state that mortality has differing impacts on individual indicators.

**We have removed the sentence in the first paragraph of the interpretation.**

9. Total mortality will make a big difference in programs with late complications, where institutions are shifting care and for chronic diseases. In addition, it is much more patient-centered.

**We have added the following sentence to the interpretation: "In particular, reporting all deaths may be important for programs with late complications, where institutions are shifting care to other settings, and for chronic conditions."**

**We have also added that including all deaths is more patient-centered in a few places in the manuscript.**

10. I am not sure if 2 different time periods are the ideal approach to test in-hospital versus total mortality. I might have tried to find true positive and true negative effects. For example, examining stroke outcomes in urban versus rural patients as outcomes have shown to be different given access issues related to revascularization. Similarly, issues related to cancer surgery in high volume versus low volume sites.

**Thank you. We may try to address this in future work.**

11. Total mortality better reflects consequences of major disease.

**We have added the following sentence to the interpretation: "These increases in mortality rates are important because they are patient centered and sensitive to all care received during the critical time following AMI, stroke or major surgery, and more accurately reflect the consequences of major conditions."**

**Reviewer 2:** Mr. Ryan Strum

**Institution:** McMaster University

General comments (author response in bold)

Thank you for the opportunity to review this manuscript on the important topic of reporting 30-day mortality rates. This manuscript is organized, concise and structured well.

**Thank you.**

I have a few revisions to may improve the completeness and re-evaluation of the manuscript for publication consideration by the editors.

1. State why the fiscal years 2011-12 and 2016-17 were used. I assume there is a reason, such as data availability, but this is important to recognize. If not, there are potential limitations from selection bias, which restrict your conclusion of results  
**We have revised the following sentence in the methods: “This study reports on the two fiscal years 2011-12 and 2016-17 based on availability of linked data at the time of analysis.” We also added a sentence in the discussion: “The analysis did not capture data during the COVID-19 pandemic when trends of in-hospital and out-of-hospital deaths may be different.”**
2. Please state whether the death reports to the CVSD are mandatory for all provinces to submit. If not, the CVSD could underreport out-of-hospital deaths, leading to an underreporting of the results in the study  
**We have revised the following sentence to add this content: “Reporting of deaths to provincial and territorial Vital Statistics Registries and providing these data to Statistics Canada are mandatory” and provided a reference.**
3. The first paragraph of the Results section uses wording of ‘stable’, this reads odd in a health care context and death, possibility substitute with wording such as ‘remained consistent. \*This remark is purely a suggestion, will leave to authors to decide  
**We have made this change throughout the manuscript.**
4. Was not stated in the manuscript but needs to be confirmed: in the reporting of the cohorts, could a deceased patient have been counted in multiple cohorts? If so, this needs to be stated, and should most likely be reported as a separate column in the tables. Such as an AMI and major surgery death within 30-days; frequently patients with AMI will undergo major surgery, which could confound your results. If there is no ‘double-dipping’, then I state in methods each cohort is mutually exclusive  
**We have added the following sentence for clarity: “The cohorts used to calculate the three indicators were not mutually exclusive; patients may have been included in multiple cohorts if they experienced more than one exposure of interest (i.e., AMI, stroke, and major surgery), as described by the CIHI indicator definitions.”**
5. I ponder about the generalizability of these results as seems very specific to CIHI. I believe they are, but state this. Consider a statement that your work is generalizable to international administrative databases that use similar reporting measures and means of collection  
**We have revised the following sentence in the interpretation to add this content: “These findings support CIHI’s measurement approach and are generalizable to international health system performance indicators that leverage administrative health data and use similar methodology.”**
6. A limitation of the study not reporting is that these conditions have a relatively high mortality rate compared with less severe conditions, so generalizing your results to less severe conditions to analyze 30-day mortality out-of-hospital may not be accurate (i.e. respiratory admissions, non-major surgeries), the prognosis of less severe conditions could differ for out-of-hospital cardiac arrest rates  
**We have added this sentence to the limitations: “These results may not be generalizable to less severe conditions with lower mortality rates, where relative proportions of in- and out-of-hospital deaths may differ.”**

7. Table 2 reports statistical significance of  $p < 0.05$ , but was not stated in the statistical approach what test was used. Please state

**We have removed the inferential statistics based on the recommendations of the statistician that reviewed the manuscript.**

8. Finally, and possibly most notably, in my opinion, your final conclusion may be too strong given the evidence of the article in the line "These findings validate CIHI's measurement approach." In absence of a gold standard or use of validation techniques (convergent, criterion), I'm not certain this conclusion can be drawn, especially when you determined in your paper that CIHI's traditional reporting of 30-day mortality is inaccurate (though marginal) and is underreporting the true 30-day mortality rate. Consider using a different but strong word than 'validate'

**We have changed the wording to 'support' throughout the manuscript.**