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Title: Emergency department outcomes for emergent diagnoses during the first year of the COVID-19 pandemic: a Canadian population-based study

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Reviewer 1: Dr. Melanie Bechard

Institution: Children's Hospital of Eastern Ontario

General comments (author response in bold)

This is an overall well-done study that aims to address a useful and important research question.

1. It would be helpful to explain why March through December was chosen as the period of analysis as all the diagnoses studied showed a return to baseline frequency of ED visits before September of 2020. This may make the results overly conservative and may conceal some of the effects of acute care avoidance. It would be helpful to discuss this in the limitations section too.

This period was chosen because there have been many studies that examined emergency department use and outcomes during the early part of the pandemic. We wanted to explore a longer period, which is why we examined March through December. We have added the comment made by the reviewer to our limitations, stating that “Some of our results may be overly conservative and conceal the effects of acute care avoidance early on in the pandemic due to reduced volumes because we looked at a longer period of time during the pandemic, over which volumes initially declined then returned to baseline”.

2. The second paragraph of the background (lines 24-54) implies that primary care physicians offering virtual instead of in-person visits may lead to delays in care. The evidence provided does not substantiate this generalized assertion. It is unlikely a patient with the time-sensitive conditions listed (i.e. truly symptomatic ectopic pregnancy or appendicitis) would be offered a virtual appointment; most primary care offices would likely advise patients to visit the Emergency Department if the patient could not be examined in-person. Rather than seeking alternatives to the Emergency Department, fear of COVID infection might have motivated patients to avoid seeking care altogether. In addition, patients with many of these conditions may be followed by a specialist (i.e. endocrinologists caring for Type 1 Diabetes). Many of these specialists were also inaccessible by in-person appointments. It would be more cautious to state that avoidance of seeking care and / or virtual visits (from either primary care providers or specialists) may contribute to delayed care and worse patient outcomes.

Thank you to the reviewer for this comment. We have edited the second paragraph of the background to reflect the reviewer's concern. We have focused less on issues of reduced in-person access to care and have included avoidance of care all together as both being possibilities for delays to care and, as a result, worse patient outcomes.

3. The authors should clarify if the statistical model adjusted for the presence of any medical comorbidities listed (yes/no binary), or if each comorbidity was treated as a separate covariate.

Each comorbidity was treated as a separate variable, this has been specified in the methods.

4. Figure 1: It would be helpful to add a box of shading around April-June in each year so readers can more easily compare similar time periods.

We have included a vertical line in all of the figures to note where the pandemic period starts so that the figures are easily comparable to one another. We did not want to make any conclusions about the April to June time period, therefore, these have not been shaded within each figure.

5. Table 2: It would be helpful to clarify elsewhere in the article why different covariates are chosen for ectopic pregnancy compared to the other diagnoses.

As per Table 1b, there were too few people with ectopic pregnancy with several of the comorbidities (i.e. CAD, CHF, etc), likely secondary to age. As such, we did not adjust for these comorbidities. There were less than 20 patients with these conditions and in some cases, no patients with that comorbidity.

6. There is a discrepancy in the word count for the abstract. It is listed as 486 words on the title page. It seems to be 268 words. It should be shortened slightly to a maximum of 250 words.

The abstracts word count on the title page has been corrected. The abstract word count has been shortened to 241 words.

7. Page 7 line 38-40 is missing a closing bracket

This has been added.

8. References #2 and #11 seem to be the same. When the reference appears later in the article, it should still be designated #2.

Thank you for pointing this out. We have revised the reference list and adjusted the reference numbering throughout the manuscript.

9. Reference #3 notes it was accessed "Jan 17 1, 2022"

This has been corrected to Jan 17, 2022.

Reviewer 2: Dr. Balthasar Hug

Institution: Luzerner Kantonsspital

General comments (author response in bold)

Title:

1. «Emergency department outcomes for emergent diagnoses during the first year of the COVID-19 pandemic: A Canadian population-based study» I would add

- "...for four selected emergent..." and
- "...A retrospective Canadian..."

These have been added to our new title, "Hospital admission from the emergency department for selected emergent diagnoses during the first year of the COVID-19 pandemic: A retrospective Canadian population-based study".

Abstract

2. Interpretation:

- First sentence: see conclusions below.
- Last sentence: “For other emergent diagnoses, there were no differences in outcomes.”-> “For the other selected emergent diagnoses...” Reasoning: see below in remarks on research question.

As per the reviewer’s suggestion, we have removed the first sentence of the interpretation and edited the last sentence to “For other selected emergent diagnoses...”

Research Question

3. P. 4, lines 45-54: “Therefore, the primary objective of this study was to use population-level data in a universal health care system to examine a marker of higher patient acuity on presentation, hospital admission from the ED, among patients with four emergent, non-COVID related surgical and medical diagnoses during the first year of the pandemic compared to a historical control period.” But the results from tables 1a-e show how many patients came for an ED consultation, not the number of hospital admissions as stated in the research question. And the first paragraph in the results’ section talks about the number of ED visits, not the number of hospitalized patients. Why are the authors writing about something that is not the main question here? The authors should follow their own research question and answer their main question first (the mentioned tables are very informative, but not the first-line results).

We have included data on the volume of ED visits for all of the selected complaints because it provides the reader with descriptive information that there were reductions in ED volumes initially during the pandemic. As such, we have left the data pertaining to ED volumes in the results to help readers interpret our results. However, if the editor wants us to remove all description of ED volumes from the manuscript, we would be happy to do so.

- The authors present the data from “four emergent” diseases, but the reader never hears anything about why these diseases were chosen. What was the reason e.g. for choosing kidney malfunction? The authors should explain why they chose these illnesses. Choosing these there might be a selection bias in explaining a characteristic of the pandemic and choosing others might result in different outcomes. The explanations offered on p. 6, lines 12-17 apply to many other diagnoses too.

As described in the methods, these diagnoses were selected a-priori by the research team. We have added this to the methods, as described above.

Methods

4. This is a retrospective study design. Please add this to the title “..... A Canadian, retrospective, population-based study”, see remarks on the abstract above.

As described above, this has been added to the title.

5. The methods’ information listed in the appendix (Data sources, Study participants and Covariates, written text but not the tables) should be moved to the main pa-per. These three sub-chapters contain important information for the reader.

Unfortunately, due to word count restrictions, we cannot move the information in the supplementary material into the methods section of the actual manuscript. This format is typical of similar studies using administrative data, where variable definitions are included in the supplemental material for interested readers.

6. In the first paragraph, p. 5, line 20 the abbreviation “ICES” appears without any explanation what type of organization this is or what the characters mean. Please write these abbreviations out where they appear the first time for the international reader. **ICES is no longer an acronym. In 2018, the institute formerly known as the Institute for Clinical Evaluative Sciences formally adopted the initialism ICES as its official name. This change acknowledges the growth and evolution of the organization’s research since its inception in 1992, while retaining the familiarity of the former acronym within the scientific community and beyond. Therefore, in the methods we have stated “Patient information was obtained from province-wide administrative health databases held at ICES (formerly the Institute for Clinical Evaluative Sciences)”.**

7. The authors do not mention any statistical software. What did they use? STATA? SAS? R? Python? This is fundamental information needed in the paper. Thank you for adding this.
SAS was used for analysis, this has been added to the methods.

Results

8. Table 2, last line: “Too few outcomes” in 30-day mortality: What does this mean? 2? 5 deaths? There is no explanation in the table or the manuscript what “too few” means. I would suggest the authors mention these numbers in the footnotes of table 2. The authors speculate about a higher number of ruptured appendicitis regarding the higher 30-day return rates (p. 10, last sentence). Reading this we would assume to see some mortality in the corresponding patient cohort.
As described above, for ectopic pregnancy there were less than 5 deaths overall and for appendicitis there were too few deaths for the model to converge. We have now reported the overall event rates in table 1a-e. In table 2, we have included a footnote stating that there were “Too few outcomes overall or in one category to run model”.

9. P. 10. Line 47: “...return hospitalizations after 30 days” Wouldn’t that be “within 30 days after discharge”?
This has been edited to within 30 days.

10. Tables 1a-e: Second column in tables: please add a header such as “Remarks” or the like.
The second column has categorization of some variables, where appropriate. These typically get placed under the variable and indented when in press. As such, we have not given this column a header, as it is not truly a separate column.

11. Table 2: footnote: please write out the adjusted variables (* remark). Just referring to other tables does not support understanding.
We have adjusted for 18 covariables in most models, and these are listed in the previous tables and in the methods section under covariables. As such, we have not listed out these variables again under table 2 due to the footnote becoming very long. If the Editor would like us to include these variables in the footnote for table 2, we would be happy to do so.

Discussion

12. Limitations' section: Please add the following aspects

- Restricted generalizability: the study was done in Ontario and the de-scribed results might not be applicable to other Canadian regions or countries.

This limitation has been included in the limitations section: “*Finally, given that this study was conducted in Ontario, the findings may not be generalizable to other regions/countries*”.

Conclusions

13. First sentence: Why mention the patient volume here? Again, this is not the main research question of the paper (see methods and results annotations above). I would answer the research question in the first sentence.

This has been removed from the first sentence of the conclusion.