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Title	Acute mental health service use following onset of the COVID-19 pandemic in Ontario, Canada: a trend analysis
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Reviewer 1	Tyler Marshall
Institution	University of Alberta, Psychiatry
General comments and author response	General comments: This study examined whether rates of hospitalizations and emergency department visits for mental health concerns according to age/sex diagnostic grouping and intentional self-injury for individuals over 10 years of age. The research attempts to provide epidemiological evidence to help address a potential gap in knowledge: whether distress associated with COVID-19 is associated with increased several mental health outcomes. The paper is mostly well written overall and appears to provide pertinent surveillance data re the impact of the COVID19 pandemic on acute mental health utilization in Ontario. Comments by section: Abstract: 1) Method: “by” does this mean "accounting for" or "stratified"? RESPONSE: We meant, “stratified by”. This has been added. 2) Interpretation: Do these findings rather suggest decreased health service utilization? Is this contrary to expectations? RESPONSE: These findings do suggest decreased ACUTE CARE utilization and this was contrary to initial expectations. We have added to the first line of the Abstract Interpretation: “Contrary to expectations…” Introduction: 1) Well-written overall. If there is space, I think a sentence or two explaining the significance of this problem may help the reader understand the potential impact/relevance of this research. What did the authors expect to occur as a result of the pandemic? Was there an apriori hypothesis? Some commentary here will help frame the study question and link to the discussion. RESPONSE: We have added to the introduction, “Understanding this gap is important for health system planning and service delivery”. The results were unexpected. We have added a hypothesis (and rationale) in the last line of the introduction, “Given anecdotal reports of distress and concerns of the pandemic impact well-being, we hypothesized mental health- related acute care visits would increase in the early phases of the pandemic.” 2) Page 6 line 42: This sentence would be stronger if there was a citation to support this. I would be surprised if no acute mental health care utilization data has been reported anywhere. Some comments about this would be appreciated. (This comment relates to my comment above as well). RESPONSE: At the time of initial writing, we could find no published data on acute mental health care utilization in Canada. As the pandemic has progressed, more data is

	emerging and we have added two citations (one of which is a rapid review) and changed this to say “there are few reports, at a population level, on acute mental health care utilization…., rather than ‘no’ data. Methods: 1) Overall, there appears to be methodological coherence, suggesting the methods is likely appropriate for addressing the question. I think the manuscript quality would however be improved if the authors consulted the STROBE-statement checklist (EQUATOR) to guide reporting of these methods/results. If there is a clear rationale for not using this document, or if not applicable, please advise. URL: https://urldefense.com/v3/ https://www.equator- network.org/wp-content/uploads/2015/10/STROBE_checklist_v4_cross-sectional.pdf ;!!D0zGoin7BXfl!sgj0cnHJ-_Pm0tkRgVrWo1pEfVNEHiLwpMw05PsgXJXI9iUwUPz0XdqGj6dFct2ba0ra0rY$ RESPONSE: We have used the RECORD guideline for reporting of studies using observational routinely-collected data (and added reference to this at the end of the methods section). 2) Some more details with regard to the repeated cross sectional design its may be useful. If the measurements are repeated, is this not a longitudinal design? Cross-sectional is measured one point in time, to my knowledge. More information/clarity here would be helpful/less confusing for readers without epidemiological expertise. RESPONSE: We initially had described this as a repeated cross-sectional study as we were taking repeated cross sections of population level (monthly) visit rates. As per this reviewer and the Editor’s suggestion, we have modified this to instead describe our design as a “trend analysis of administrative population data” (abstract and first line of methods). 3) Is there a protocol available for this manuscript? If so, is it available? Knowing what was decided a priori will strengthen credibility of the methods & results, I think. RESPONSE: There is a Dataset Creation Plan available for this study that uses macros that are internal to ICES and we have written this in the “Data sharing” statement on the title page, should readers wish to view the full plan. The outcomes and analyses were determined a priori (including age groups, sex, diagnostic groupings) as our group regularly produces reports for the Ministry of Health of Ontario using these same categories and health system performance metrics (e.g., https://www.ices.on.ca/Publications/Atlases-and-Reports/2017/MHASEF and https://www.ices.on.ca/Publications/Atlases-and-Reports/2018/MHASEF). These reports contain technical appendices with our plan. However, this was modified to reflect the dates of the COVID-19 period with monthly (rather than annual or quarterly) analyses. 4) Page 7 line 18: I think the inclusion/exclusion criteria could be more clearly written to improve reproducibility of the results. For instance, were 106 year olds excluded apriori? Or did you plan to include anyone in Ontario with a valid healthcare card over the age of 9? If so, it may be easier to say that so it is less confusing for the reader. In the results, perhaps it makes sense to say individuals

	10-105 were included/retained for analysis (presumably no individuals over age 106 were identified). A separate section or even a supplementary table could be considered if the authors think this would be helpful/necessary. RESPONSE: We were interested in the population-level rates and it is standard practice at ICES to include individuals only up to 105, thus, those older than 105 were not excluded, per se, rather, they were not included. It is also standard at ICES to use those with a valid health card for whom sex and age was available as in order to be linked between ICES datasets, these identifiers are required. For clarity, we have added a flow chart for readers. 5) Can the authors provide some more details on what a valid healthcare card entails and if there were any individuals excluded from the analysis with reasons? (e.g., individuals who did not have continuous coverage). RESPONSE: All individuals linked at ICES must have a health card number and have been registered in the Registered Persons Database (Ontario's health insurance registry). These linked records were used as the population denominator (essentially all of Ontario), and are stable over time. As above, we provided a flow chart for readers to better understand exclusions. Statistical analysis 6) Page 9 line 32: I assume "rates" refers to incident rates or crude monthly rates? RESPONSE: This refers to the crude monthly rates, as outlined in the paragraph above this, "..We calculated crude monthly rates for mental health or addictions–related ED visits…" 7) Page 10 line 13: Do the authors mean "accounting" for sex/age dx grouping, or "stratified" by sex/age dx grouping? Some clarity here would help. RESPONSE: We have added "…and stratified by…" 8) Page 10 line 8: what alpha was used for statistical significance? RESPONSE: Alpha of <0.05 was used for statistical significance. This is written in the caption section of each of the figures. Results: 1) How many individuals were included/excluded in the analysis? A flow chart may be considered to help the reader follow the methods/analysis. This information may help with establishing generalizability. RESPONSE: We have added a Table 1 and a Flow Chart. 2) How did the authors establish age groups of 10-21 years and 14-21 years? Were these age categories defined apriori or did this emerge from the data? RESPONSE: Our age groups included 10-13, 14-17, 18-21, 22-24, 25-44, 45-64, 65-105. These were determined a priori as these are standard age groups used by our group for mental health system performance reporting. (See Chiu, Guttmann, Kurdyak, Healthcare Quarterly 23(3) October 2020: 7-11.doi:10.12927/hcq.2020.26340). We have added to the statistical analysis section that these were determined a priori. Discussion:

	1) Were the results unexpected? What were the authors expectations/hypotheses? RESPONSE: The results were unexpected. We have added a hypothesis (and rationale) in the last line of the introduction, “Given the anecdotal reports of distress and concerns of the pandemic impact well-being, we hypothesized mental health-related acute care visits would increase in the early phases of the pandemic.” 2) Page 11 line 33: this sentence, although likely true, sounds like hyperbole as written. Re- writing using different adjectives such as “substantial” may make more sense than “massive” which means big. A citation or data here (and citation) would be preferred. RESPONSE: We have changed this to “substantial” and added a citation. 3) How might the pandemic (and shift to virtual care) impact issues around reporting of mental health data? RESPONSE: We measured acute care visits (i.e. visits to EDs or hospitalizations) where patients were seen in person, so this should not have affected reporting of mental health data. Further, data feeds were expedited (rather than slowed) during the pandemic due to the urgency for data to inform health system planning during the pandemic. Internal ICES documentation (not publicly available) that the 90th percentile of all NACRS submissions are within 60 days and DAD submissions are within 73 days. 4) Page 13 line 40: how much do the authors think the issues with health administrative diagnostic data had on the interpretation of the results? How can this issue be overcome? Was it overcome? How? RESPONSE: The administrative diagnostic data limit our ability to confidently assign mental health diagnostic grouping with more granularity and therefore in our interpretation of findings, we cannot make conclusions/interpretations about the pandemic effects on narrower clinical groupings (e.g., distinguishing types of anxiety disorders [phobias, social anxiety, etc], distinguishing types of substance use disorders, etc). Given this, we intentionally used broad diagnostic groupings used for mental health system performance reporting (https://www.cihi.ca/sites/default/files/document/common-challenges-shared-priorities-vol-2- report-en.pdf). We overcame this issue by using high level diagnostic groupings. We have also stated this as a limitation in the Interpretation. We have also added, “Thus, analyses are limited to broad diagnostic groupings”. 5) Page 13 line 52: I am not sure that the data supports any speculation. If these data supports a novel hypothesis, please provide a clear rationale supported by evidence and other literature. RESPONSE: We have softened the language around speculation and rather suggested that the shift to virtual care may have improved access to ambulatory care. We have added references for this shift to virtual care and for improved accessibility with virtual care. 6) Page 14 line 13: No need to restate findings here. This has already been said. RESPONSE: As per the CMAJ Open guidelines on manuscript writing, the final

	section/paragraph of the interpretation should be a conclusion and state, 'Conclusion: This should include the main results of the study and implications (one paragraph), with a "Conclusion" subtitle". For clarity, we have added a "Conclusions" subtitle. 7) A sentence about how this might be useful for clinicians or policy-makers would be of interest. This would be woven into the last sentence (page 14 line 18) perhaps. RESPONSE: We have added to this last line that these data (and ongoing monitoring) are critical "…for health system planning and service delivery…."
Reviewer 2	Karen Urbanoski
Institution	Centre for Addiction and Mental Health, Social and Epidemiological Research
General comments and author response	This is a strong article that draws from population-based administrative health data to address a relevant and timely topic. Results are novel and the authors offer reasonable interpretation. It is a minor point, but is this study best described as a repeated cross-sectional design (as opposed to a time series)? Otherwise, I recommend that this article can be published as is. RESPONSE: Thank you for this very positive review. We have modified the study to a trend analysis of administrative population data, as per the Editor.