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| Reviewer comments | Author’s response | Location |
| **Reviewer 1**  This is a very well-conducted study and the manuscript is written very well. The topic is very pertinent during the current COVID-19 times and the results are very interesting. Below are few concerns that authors should address.  What is LHIN? Please define abbreviations prior to first usage.  The authors should have provided monthly crude numbers for the 2020 and 2019 period, in the form of a table, to provide readers some perspective on which factors have reduced or increased during the lockdowns, for each of the following categories -substance related (excluding alcohol), alcohol intoxication, mood related (anxiety, PTSD, depression, and bipolar disorder), psychosis-related (psychosis, bizarre behaviour), situation related (situational disturbance, life crisis, concern for safety, and domestic violence), self harm related, and completion of Form 1 indicating involuntary detention for psychiatric assessment.  Why did the authors not include patient demographic information to examine the change in trend in mental health crises cases before and during the pandemic?  My assumption for seeing such a decrease in cases during the lockdown is that a huge chunk of the population living in these areas are students and those working in tech companies, who have allowed them to study/work remotely, and many would have moved out of these areas to other regions in the country (closer to family etc.) So, if it is observed that the other regions saw an increase in cases during the 2020 period, it might be due to influx of people from other regions. Is there a way to confirm this? If not more discussion is warranted on this in the ‘discussion section’ | We appreciate the reviewer’s kind comments.  LHIN (Local Health Integration Network) has been updated in the text.  We agree with the reviewer and have placed the monthly crude numbers for all diagnostic categories in a table.  While we agree that demographic data would be a useful addition, this is one of the inherent limitations of the aggregated NACRS data. We have acknowledged this in the Limitations: “**These include the inherent limitations of NACRS anonymous coded diagnostic data, which precluded detailed chart review and demographic analysis of age, gender, and ethnicity.**” | Introduction  Results |
| **Reviewer 2**  I strongly support the publication of this research asap. Great paper. | We appreciate the reviewer’s comments. |  |
| **Reviewer 3**  The research goal is not clearly stated. This is not clear what is the contribution of the research. Secondary data collected for 6 months. however, the findings are showing a comparison of 2019 and 2020 data. This is really confusing. Weak methodology and its all over the place. There were no COVID-19 cases in Canada in the year 2019. How the data is related to COVID-19? The findings of the research are doubtful and making no significant contribution. This manuscript does not meet any publication requirement. | The research goal is clearly stated in the Introduction, reiterated here in case this was missed: “**the primary objective of this study was to determine the short-term effects of lockdown on emergency mental health service use, including: mental health related emergency department (ED) visits, mental-health related calls to police services, and calls to a central crisis help line.**”  Contribution is stated in the Conclusion: “**These counterintuitive results may be useful in anticipating mental health emergency resource needs in future pandemic responses.**”  Comparison of year-over-year data is a standard method for comparison of changes between pre-pandemic and pandemic years.  “Weak methodology” is a fairly vague critique, as is the statement “findings of the research are doubtful.” If the reviewer has specific constructive concerns, we would be happy to address these in detail. |  |
| **Reviewer 4**  it is a good work! | We appreciate the reviewer’s comments. |  |
| **Reviewer 5**  interesting and expected findings of the study.  it will be interesting to know if the mental health calls/ cases spike up after the lock down period ends. | We agree! This is certainly a future direction, and speculation is that the second wave has not been the same as the first wave regarding demand for services. |  |
| **Reviewer 6**  The authors present results of the number of various mental health crises in Ontario Canada at two time points: during the beginning of the SARS-CoV2 pandemic and the year prior. While mental health awareness and mental health services are extremely important, the results presented are for a region in Canada which might not be generalizable to other populations. Below are a few comments for the authors to consider:  In the datasets was any demographic information collected? Such as age, race, ethnicity, gender, etc? Also was there a way to ascertain if one person had multiple ED visits or police calls?  Please add this description to the methods if available. If it is not available or was not collected this could be a potential bias and should be added as a limitation  Please add references about the statements made about Poisson regression under the data analysis section  Please add more detail on the Poisson regression models. Where the models Univariable or multivariable? If multivariable please state what covariates were added to the model.  Results  A traditional table 1 that describes the elements of the dataset is necessary. Please consider adding one to the manuscript with 3 columns: one to display 2019 pre pandemic numbers, pandemic numbers and a column of p-values comparing the two time periods.  P-values less than 0.001 should be reported as <0.001.  The authors report t values and a values in the text however they do not provide an explanation or contextual or what these numbers mean for their audience. How should the readers interpret the z value?  Consider adding p-values to the figures.  Discussion  The authors state: “As an observational study, although these findings can describe correlation but not infer causation, they will at least be hypothesis generating.” Whitley I understand the message the authors are trying to communicate it is slightly confusing as no correlations were reported in the text. Please consider rewriting this sentence.  One paragraph has a smaller font than the rest of the paper. Please consider changing it so ease feasibility for the audience.  Conclusion  This section has a similar issue of the font text being smaller than the rest of the manuscript | We agree that it is at least debatable whether the results can be generalized to other regions.  Demographic data is not available on the diagnoses, as this is one of the limitations of the aggregated NACRS data. We have acknowledged this in the Limitations: “**These include the inherent limitations of NACRS anonymous coded diagnostic data, which precluded detailed chart review and demographic analysis of age, gender, and ethnicity.**”  Quasi-Poisson regression is commonly used to model over-dispersed count data (Hoef 2007, https://digitalcommons.unl.edu/cgi/viewcontent.cgi?article=1141&context=usdeptcommercepub). For each category, we used a univariate quasi-Poisson regression with a log link function and year as the only predictor.  Crude numbers are now included in the supplementary materials.  We have changed p-values to <0.001.  Regarding context for the t, a, z values, we have modified the tables. Regression coefficients are the estimated slope of the model. Other values should be self-explanatory.  We have removed the word “correlation” and replaced this with the word “trends” for clarity: “**As an observational study, although these findings can describe trends but not infer causation, they will at least be hypothesis generating.**”  Font size seems to be the same in Discussion/Conclusion as in the remainder of the article. | Introduction  Results  Discussion  Conclusion |
| **Reviewer 7**  "Introduction" section.  Authors should include references to other studies about the immediate and delayed effect of COVID-19 lockdown, which may include "exacerbate loneliness and depression and contribute to mental health-related morbidity and mortality" (e.g., Mazza et al., 2020; Roma et al., 2020 - more information provided below).  Regarding the research question, the authors should explain more clearly why they hypothesized that "during lockdown, there would be a year-over-year increase in use of all selected mental health services" since they reported limited previous formal research and mixed results. In particular, the authors refer to three studies that have highlighted a decrease in mental health consultations in the United States and to one study that have underlined an increase in emergency calls from women regarding intimate partner violence in Europe. They should explain in more detail the results of the reference studies and, accordingly, they research hypothesis.  "Methods" section.  The authors should provide more information about the sample and its characteristics, including sample size (both the 2019 and 2020 groups) and size of each category (i.e., diagnostic and Police response categories of both the 2019 and 2020 groups), getting data into tables.  "Data Analysis" section.  Please move the figures reported below in this section.  Please move information about the institutional ethics approval in the "Study Design" section.  "Results" section.  Please bold in Table 1 and Table 2 the statistically significant differences. In general, authors should make tables clearer, providing 2019 and 2020 data for each categories or moving the corresponding figures reported below.  "Discussion" section.  In the Results section, authors stated that "Crisis calls were significantly decreased during the initial period of lockdown compared to the same period in 2019 (t=-4.36, p=9.21x10-4). Following the lockdown period, weekly crisis calls increased by 43% from May 31 – June 6 to the July 12 – July 18 in 2020", but they do not provide an explanation for this result. Is this a "monthly trend" that can also be found in 2019 or is it only characteristic of 2020? In both cases, the authors should provide possible explanations of the phenomenon.  "Conclusion" section.  "This observational study provides preliminary data to quantify the short-term mental health and social impacts of lockdowns [...]". This sentence is too general, not supported by the research carried out in this study. Authors should refer to the data obtained by their study and to the context in which they moved.  "References" section.  1. Mazza, C.; Ricci, E.; Biondi, S.; Colasanti, M.; Ferracuti, S.; Napoli, C.; Roma, P. A nationwide survey of psychological distress among Italian People during the COVID-19 pandemic: Immediate psychological responses and associated factors. Int J Environ Res Public Health 2020, 17, 3165. doi:10.3390/ijerph17093165  2. Roma, P.; Monaro, M.; Colasanti, M.; Ricci, E.; Biondi, S.; Di Domenico, A.; Verrocchio, M.C.; Napoli, C.; Ferracuti, S.; Mazza, C. A 2-Month Follow-Up Study of Psychological Distress among Italian People during the COVID-19 Lockdown. Int. J. Environ. Res. Public Health 2020, 17, 8180. | Given that this study relates to the pandemic first wave, we agree that these references re loneliness and depression (Mazza, Roma) would be appropriate. We have added these to the Introduction.  Our hypothesis that mental health emergency services would be strained is based on the known increase in depressive symptoms noted in previous studies. We note in the Introduction: **“The direct effects of lockdown can be expected to be exacerbated by the economic stresses of business closures, increasing unemployment, and recession12. As a result, lockdowns make additional community support for anxiety, mental illness, suicidality, alcoholism, substance abuse, and domestic violence an ethical imperative10,13.”**    We might intuitively expect these consequences to be reflected in increased demand for and use of social and mental health services, but there has been limited formal research10 and early reports have been conflicting.”  We have expanded our discussion of the three reference studies referred to in the Introduction.  We have added the monthly crude numbers for mental health diagnoses in the supplementary materials.  We have now moved the ethics to the beginning of the Methods section.  We have bolded statistically significant differences in Table 1 and 2.  The change point analysis now added detects the increase in police responses in June/July, which is largely driven by the change in crisis calls.  We have modified the conclusion statement to make this more specific to the findings of the study: “**This observational study provides preliminary data to quantify the early, short-term mental health and social impacts of lockdowns on demand for emergency services.**” | Introduction  Methods  Data Analysis  Results  Discussion  Conclusion |
| **Reviewer 8**  Comments for Authors:  Overview and manuscript strengths:  This manuscript looks at a number of different metrics related to mental health outcomes across different data streams (ED departments, police calls, and a crisis telephone line) within a region of ON, Canada. Despite significant discussion in the media and anecdotal evidence from some healthcare workers it is an important step to critically assess the empirical evidence to either support or refute the hypothesis that the pandemic has led to an increased use of mental health emergency services. This study examined the data for a 6-month period of time at the start of the pandemic and compared the outcomes within the same time period from the previous year (2019) to identify if there was an increase in mental health emergency service use.  Interestingly, the authors found that in the examined data, there was a decrease in most of the mental health metrics examined refuting the hypothesis that the pandemic (at least the first wave) caused an increase in care-seeking behaviour due to mental health concerns. This finding was unexpected but is critically important for helping to guide discussion moving forward.  Further comments and suggestions can be found below. Since the manuscript did not have page numbers or line numbers I was not able to easily indicate the exact location I was referring to. My apologies. You may need to search for them.  Minor comments:  Introduction:  • I would suggest changing social distancing to physical distancing throughout the paper since the terminology used has primarily been physical distancing in order to avoid the impression that people must forgo social interaction since there are still safer ways to engage in “social” activities.  Methods:  • define LHIN  • Mental health discharge diagnoses – is this done using ICD codes? If so, I would clarify and add the specific codes etc. that were included to generate the dataset. The work should be reproducible, and this would help significantly in that regard.  • Police service use – there is no mention specifically of mental health related calls (e.g. person experiencing a psychotic episode etc.) Is that because there were none or is that because it was excluded for a specific reason? I would say it is as important to outline what might have been excluded and why as well as what was included and why.  • Was there a reason you did not consider a negative binomial regression? Quasi- Poisson is an unusual term to me. How is this different statistically from a Poisson regression or a negative binomial (in order to correct for the overdispersion)? The data analysis description here is insufficient to understand how the data were analysed and the tables do not help to clarify. Is this a proper time series analysis (e.g. case counts/day over time with a year variable? Or are you just comparing the number of visits for each “code” over the entire time period in 2019 vs. 2020 (in which case I am not sure a Poisson regression is actually what you would want to use for your analysis). It remains unclear to me what was actually done here based on the methods.  • You appear to have further restricted the time periods for different types of analyses (e. g. March 17-May 4 which is only a subset of your total dataset). Again, it seems like a flow chart/figure would be helpful to show how many visits you had in that period etc. as well as some clarification/justification about why different time points were selected. It seems like you maybe have some more detailed objectives that have not been explicitly stated here given the different timelines.  • Figure 2- the text states this is over the same period as the previous statement but in fact is for the full time period where the previous comment is for only during the “lockdown period’. Again, it is not necessarily wrong but it is just unclear to an external reader.  • Figure 1: I would clarify that these are not hospitalized covid cases and rather community COVID cases in which case I might use a standardized rate.  • Table 1. this comes back to the statistical analysis (in that the description is not sufficient for an outside reader to understand what you have done). Were case counts for each “condition” examined individually such that these are individual univariate models with a year term? What is the predictor? Year? Month and Year? Coefficients? standard error? 95% Confidence interval? Did you need to log transform the count data? Should this maybe be zero inflated? Do you have many days with zero counts? I have lots of questions that can’t be answered based on the short description in the methods section.  Discussion:  • You indicate that you were surprised by the results since it showed a decrease in these visits and calls and then you indicate that there is current focus on deteriorating mental health in young people. BUT, your data don’t include any age breakdown. So in fact, you can’t assess the age effect. While I recognize the interest in age, given that you can’t answer any questions related to age the discussion might be more clearly framed if it focused on more generic population level comparisons.  • For instance, you use an example of American students (again drawing attention to an age effect that you don’t look at using your data as far as I can tell) Are there other examples here that are more population focused and not age-specific that might be more reasonable comparisons for your results?  • The comments/”hypothesizing” about greenspace etc. is interesting but are there any references that could be added to support this sort of claim? This seems speculative at best as a possible explanation for the findings. There are also other purely speculative explanations such as, this region has a high proportion of higher earning individuals with the ability to work from home (tech sector, post-secondary etc.) Therefore, there may also be socioeconomic differences in this region that might influence some of these outcomes (e.g. people are maybe less worried that they will lose their business or home, don’t feel as worried about exposure due to essential work etc.), Less population level marginalization. There is lots of literature that would support this sort of possible explanation based on social determinants of health that might make sense to try to work into the discussion. | We appreciate the reviewer’s comments.  We have changed the term “social distancing” to “physical distancing”.  The term LHIN (Local Health Integration Network) has been updated in the text.  We have added an appendix (Appendix 2) describing the specific ICD-10 codes used to create the mental health categories and updated the Methods section to reflect this.  We elected to exclude the broad “mentally ill” category, since it provides similar (although less robust) information to that already reflected in the hospital emergency visits.  Quasi-Poisson and negative binomial regression models have equal numbers of parameters, and either could be used for overdispersed count data. The variance of a quasi-Poisson model is a linear function of the mean while the variance of a negative binomial model is a quadratic function of the mean. These variance relationships affect the weights in the iteratively weighted least-squares algorithm of fitting models to data. Because the variance is a function of the mean, large and small counts get weighted differently in quasi-Poisson and negative binomial regression (Hoef 2007, https://digitalcommons.unl.edu/cgi/viewcontent.cgi?article=1141&context=usdeptcommercepub). We used quasi-Poisson because the negative binomial models were sometimes not able to converge on a model fit. For each “code”, we used a univariate quasi-Poisson regression with a log link function and year as the only predictor.  Emergency department visits, mental health diagnoses, and police responses during the 2020 lockdown (March 17 to May 4) were compared to ED visits, mental health diagnoses, and police responses over the same period in 2019. These timepoints were selected in order to compare the period of lockdown to a baseline level.  In Figure 2, we believe the text clearly states the time period displayed in the figure itself. We would be happy to modify this if this remains unclear to external readers.  Figure 1: We have now clarified in the caption that these are community COVID cases.  For each “condition”, we used a univariate quasi-Poisson regression with a log link function and year as the only predictor. Coefficients and standard error are now included in the table. Log link function means the data is log transformed when modeled. The data is not zero-inflated; the raw counts are now included in the supplementary materials.  We agree that mentioning young people may be too specific in the context of the data and have made the statement in the Discussion more general and have added an additional reference.  We have added a reference to a World Health Organization review on the positive effects of greenspace on mental health. | Introduction  Methods  Discussion |
| **Reviewer 9**  This is an interesting manuscript. I have mainly two concerns-  1. Technical- Please club all subheadings into one. Like in the methodology we don't need subheadings.  2. Conceptual-I did not found (any evidence) that it is mentioned that the mental health services were disrupted/closed, rather it is assumed that it was so as a mirroring phenomenon by phone calls. The phone calls are a proxy indicator of mental health services. I think the results+ conclusion is not convincing. As an example, if there are a lockdown and all food corners are closed still we can call helpline numbers to know the availability of foods.  It will be better if the authors could mention the limitations and research gaps of this study more elaborately. | We appreciate the reviewer’s thoughtful comments.   1. For readability, we have kept the subheadings at present, although we are open to reformatting if the journal requires it. 2. The reviewer is correct that all mental health services remained open, and there is no evidence that services were refused to any person. | Introduction(?): limitations and research gaps |
| **Reviewer 10**  The study is well written study on the short-term impact of the pandemic lockdown on mental health emergency services use in the Kitchener-Waterloo region of Ontario, Canada. Authors have concluded, the decrease in most types of mental health ED visits, mental health and substance-related police calls, and mental health crisis which is not so unique and is expected because of  pandemic lockdown . There are already many studies previously done on same topic, i feel the study does not add anything new to literature. There are shortcomings of coded diagnostic data, and data on completed suicides and deaths due to non-opioid overdoses is missing . | We thank the reviewer for his/her comments. We would disagree that a decrease in mental health service use can be seen as an “expected” consequence of pandemic lockdowns. In particular, popular media have often drawn attention to the opposite effect – the risk of mental health effects on those who are profoundly affected by the stress of the pandemic itself and the secondary effects of lockdown.  We agree that NACRS data has certain inherent limitations, which we have acknowledged in the Discussion. |  |
| **Reviewer 11**  I have evaluated this informative study entitled, "A multicenter study of short-term changes in mental health emergency services use during lockdown in Kitchener-Waterloo, Ontario during the COVID-19 pandemic."  This article describes that the COVID-19 pandemic and subsequent lockdown measures have led to increasing mental health concerns in the general population. We aimed to assess the short-term impact of the pandemic lockdown on mental health emergency services use in the Kitchener-Waterloo region of Ontario, Canada.  The authors have presented a good research article. The all-purpose writing of this manuscript is good with a creative idea. It is a good study; however, I have some suggestions for the authors to revise the manuscript. Overall, an excellent study that provides insights for the scholars. Before recommending your study for publication, you need to work on my suggestions. The suggested articles are published in leading SSCI journals. By following these studies, your article will be improved.  English level  It shows that English needs improvement. Please take English editing service to make it easy to understand for the readership of the journal.  Abstract of the study  I suggest authors to write good English in the Abstract, and it must be high quality. I advise the authors to recheck the abstract and fix minor grammar errors. The abstract must reflect high quality, as it the "FACE" of the study. It is better to write it in a structured format, such as the background, methods, results, and conclusion.  Introduction section  This section needs improvement, as it is too short. Expand it to 700 words. I strongly advise the authors improve introduction according to suggested articles in the introduction section. These research articles have identified health-related topics of the infectious disease COVID-19. I believe it will improve the quality of your work. I strongly suggested them to improve this section a bit more. I advise authors to revisit their literature section of the recommended studies and cite these studies to enhance your research study's quality to reach scientific merit for publication.  Su, Z., McDonnell, D., Wen, J., Kozak, M., Abbas, J., Šegalo, S., . . . Xiang, Y.-T. (2021). Mental health consequences of COVID-19 media coverage: the need for effective crisis communication practices. Globalization and Health, 17(1), 4. doi:10.1186/s12992-020-00654-4  Abbas, J., Dake, W., Su, Z., & Arash, Z. (2021). The Role of Social Media in the Advent of COVID-19 Pandemic: Crisis Management, Mental Health Challenges and Implications. Risk Management and Healthcare Policy, 14.  Maqsood, A., Abbas, J., Rehman, G., & Mubeen, R. (2021, 2021/11/01/). The paradigm shift for educational system continuance in the advent of COVID-19 pandemic: Mental health challenges and reflections. Current Research in Behavioral Sciences, 2, 100011. <https://doi.org/10.1016/j.crbeha.2020.100011>  Literation sections  I recommend the authors add suggested articles in the literature section. These research articles have identified health-related topics. I believe it will improve the quality of your work. I advise authors to revisit their literature section of the recommended studies and cite these studies to enhance your research study's quality to reach scientific merit for publication.  I want to see publish this creative work after some corrections. I have endorsed this study as; it deserves the merit for publication. However, I suggest the authors make minor corrections according to my advice. The authors add the latest citations about infectious disease. Please read the suggested studies and cite them in the introduction, literature, and method sections. How social media and internet use among students is helpful. Add few lines in the introduction and literature sections.  Shuja, K. H., Aqeel, M., Jaffar, A., & Ahmed, A. (2020). COVID-19 Pandemic and Impending Global Mental Health Implications. Psychiatr Danub, 32(1), 32-35. doi:10.24869/psyd.2020.32  Abbas, J., Aman, J., Nurunnabi, M., & Bano, S. (2019). The Impact of Social Media Lon earning Behavior for Sustainable Education: Evidence of Students from Selected Universities in Pakistan. Sustainability, 11(6), 1683.  NeJhaddadgar, N., Ziapour, A., Zakkipour, G., Abbas, J., Abolfathi, M., & Shabani, M. (2020, 2020/11/13). Effectiveness of telephone-based screening and triage during COVID-19 outbreak in the promoted primary healthcare system: a case study in Ardabil province, Iran. Journal of Public Health. <https://doi.org/10.1007/s10389-020-01407-8>  Yoosefi Lebni, J., Abbas, J., Moradi, F., Salahshoor, M. R., Chaboksavar, F., Irandoost, S. F., Nezhaddadgar, N., & Ziapour, A. (2020, Jul 2). How the COVID-19 pandemic effected economic, social, political, and cultural factors: A lesson from Iran. International Journal of Social Psychiatry, 20764020939984. <https://doi.org/10.1177/0020764020939984>  Su, Z., Wen, J., Abbas, J., McDonnell, D., Cheshmehzangi, A., Li, X., . . . Cai, Y. (2020). A race for a better understanding of COVID-19 vaccine non-adopters. Brain Behav Immun Health, 9, 100159. doi:10.1016/j.bbih.2020.100159  Materials and Methods  This section indicates how you arranged your article. You can see the suggested study and improve your method section. Improve your study and cite these studies in the results and methods. Improve results presentation according to suggested studies.  Abbas, J., Hussain, I., Hussain, S., Akram, S., Shaheen, I., & Niu, B. (2019). The Impact of Knowledge Sharing and Innovation upon Sustainable Performance in Islamic Banks: A Mediation Analysis through an SEM Approach. Sustainability, 11(15), 4049. <https://doi.org/10.3390/su11154049>  Abbas, J., Mahmood, S., Ali, H., Raza, M. A., Ali, G., Aman, J., Bano, S., & Nurunnabi, M. (2019). The Effects of Corporate Social Responsibility Practices and Environmental Factors through a Moderating Role of Social Media Marketing on Sustainable Performance of Firms’ Operating in Multan, Pakistan. Sustainability, 11(12), 3434. <https://doi.org/10.3390/su11123434>  Abbas, J., Aqeel, M., Abbas, J., Shaher, B., A, J., Sundas, J., & Zhang, W. (2019). The moderating role of social support for marital adjustment, depression, anxiety, and stress: Evidence from Pakistani working and nonworking women. J Affect Disord, 244, 231-238. doi:10.1016/j.jad.2018.07.071  Abbas, J., Aqeel, M., Jaffar, A., Nurunnabi, M., & Bano, S. (2019). Tinnitus perception mediates the relationship between physiological and psychological problems among patients. Journal of Experimental Psychopathology, 10(3), 2043808719858559. doi:10.1177/2043808719858559  Abbas, J., Aqeel, M., Ling, J., Ziapour, A., Raza, M. A., & Rehna, T. (2020). Exploring the relationship between intimate partner abuses, resilience, psychological, and physical health problems in Pakistani married couples: a perspective from the collectivistic culture. Sexual and Relationship Therapy, 1-30. doi:10.1080/14681994.2020.1851673  Discussion section  Briefly discuss the contribution to the scientific literature. Add few lines on contribution of this study how results are insightful for academic purpose. Improve this section. Please see suggested studies and cite them in this section.  Implications  Add this separate heading and discuss implications  Conclusion  Expand this section to 500 words. It should present a good picture of the study. I want to see this manuscript published as it has presented a good research topic, although it needs minor corrections, which can be fixed in the revised version. Pay attention of English quality to reach scientific merit. I accept and endorse this manuscript for publication after the suggested minor corrections. | We thank the reviewer for his/her thoughtful comments.  We would disagree with the suggestion for use of an English editing service. All authors are native English speakers, and we have not encountered any specific examples of grammatical errors in the manuscript – if any are evident, we would appreciate the reviewer pointing these out for correction.  We would note that the Abstract is already in structured format and invite the reviewer to point out any minor grammatical errors for correction.  We are open to specific suggestions on the Introduction – however, with the goal of keeping the manuscript under 4000 words, would disagree with the suggestion of an arbitrary 700-word quota for this section.  While we congratulate the reviewer on his research output, it seems that nearly all suggestions for citations include the author J. Abbas. Most of these articles focus on Iran and Pakistan, and have little, if any relevance to the topic at hand. I would argue that they would substantially weaken the focus of our article. We question whether there would there be a conflict of interest here? | Abstract  Introduction  Materials and Methods  Discussion  Implications  Conclusion |
| **Reviewer 12**  In this study, Dainton et al compared the trends of ED mental health service use, mental health related calls to local police and calls to regional crisis hotlines during a period impacted by the COVID pandemic and subsequent lockdown to a similar period in 2019. This is an interesting study and relevant to the public health consequqnces of decisions made in response to the pandemic. The authors do a very nice job in describing the various possibilities for their findings which ran counter to their initial hypothesis.  I hope the following suggestions will help to improve the manuscript:  1) In the introduction it would behoove the authors to elaborate on the impact of COVID-19 on the populations represented in the 3 regions represented. Was there a surge scenario? How long were hospitals overwhelmed. This will elucidate any competition for resources as an explanation for lower rates of ED visits, etc..  2) Is it possible that mental health events occurred outside of the ascertainment tools utilized? The authors allude to this as a possibility, but it may be a good idea to elaborate to some extent. For example, is it possible that individuals had more successful suicide attempts, rather than unsuccessful ones, that would not have been recorded?  3) In the methods section: what is meant by weekly COVID-19 Call Reports and how does that factor into the year to year comparison?  4) The possibility that the downturn in ED visits had something to do with any diversion policies instituted by healthcare organizations preparing for an onslaught of COVID-19 patients should also be handled. If the geographic area of concern was not hit with a surge, this would be less of a concern.  5) In the Results section the dates are not consistently reported. The description of Figure 1 in the first paragraph described weeks between 3/5 and 9/5 2020 and figure 2 is purported to report the mental health diagnostic categories for the same period. But the tables that are reporting the various categories represent data from 3/17-5/4 2020 only. Why the reduction in the window of observation for the Poisson regression comparisons. No explanation is provided. Please provide. | We thank the reviewer for his/her comments.   1. Given the overall decrease in visits and demand on Emergency services, we do not believe that competition for services would explain our results. Therefore, we have clarified this with the following statement in the Results section: “**Emergency services and capacity to provide clinical care were not overwhelmed at any point during the pandemic first wave in the region, and there was no system-level diversion of services.”** 2. We have further clarified that certain emergency MH events may have occurred outside of our ascertainment in the Limitations section: **“We also lacked coroner’s data on completed suicides and deaths due to non-opioid overdoses. We cannot exclude that patients may have sought emergent help from other sources both inside and outside the community, including outpatient services, outside crisis lines and virtual resources.”** 3. Weekly COVID-19 call reports (detailing responses to COVID-19 screening questions) were collected but not used. Therefore, we have removed this from the Methods. 4. Figures 1 and 2 show counts from March 5 to September 5. We ran the models on the lockdown period, which is shaded in red. Tables 1 and 2 show the results of those models. This should be better explained now in the results. These timepoints were selected in order to compare the period of lockdown to a baseline level. | Introduction  Methods  Results |
| Reviewer 13  Dear Author  Thank you for submission  I have one minor comment for this manuscript.  Appendix 1 can be omitted as this is not making much sense in this study. Congratulations for presenting it appropriately. | We thank the reviewer for his/her comments.  We agree that the Appendix is not critical to the manuscript and would be open to removing the Appendix in the formal publication if there are space constraints – however, we believe this may be relevant in defining the context of the lockdown and the nature of the restrictions that the study population were placed under. | Appendix 1 |
| Reviewer 14  This was time-series data; in my opinion, the research question could be better answered by a Changepoint analysis using the PELT method A LOWESS analysis could be done to get a clearer visual trend  The following points need expansion:  What "conditions" were included under situational crisis, involuntary admissions, alcohol, and substance-related admissions? Could there be overlaps in these conditions? Is it possible that some of the "conditions" presented at a significantly higher frequency than others (e.g., cases of opioid overdose, domestic violence, alcohol withdrawal, etc.)  Was the composite trend from all 3 centers (presented) similar to the trends from each of the centers (not presented)?  The authors did not examine and comment upon the "recovery" of mental health-related service in May-June 2020. A CPA can actually test this change statistically. And consequently, I urge the authors to comment on this in the discussions as well. | We have now included a change point analysis, as you have described.  The categorizations for each diagnosis are addressed in the Methods: **“substance related (excluding alcohol), alcohol intoxication, mood related (anxiety, PTSD, depression, and bipolar disorder), psychosis-related (psychosis, bizarre behaviour), situation related (situational disturbance, life crisis, concern for safety, and domestic violence), self-harm related, and completion of Form 1 indicating involuntary detention for psychiatric assessment.”**  The trends were generally similar for each of the three centers – however, we have chosen to present them as a whole, since the region/population is contiguous, and since mental health complaints tend to be preferentially brought to the Schedule 1 facilities (Grand River Hospital and Cambridge Memorial Hospital) rather than St. Mary’s Hospital.  We have now included a change point analysis for the period of recovery, as you have described. | Data Analysis  Discussion |