

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	The association between youth homicides and state spending: A Chicago cross-sectional case study
AUTHORS	Mason, Maryann; McLone, Suzanne; Monuteaux, Michael C; Sheehan, Karen; Lee, Lois K; Fleegler, Eric

VERSION 1 – REVIEW

REVIEWER	Adrian Barnett Queensland University of Technology, Institute of Health and Biomedical Innovation
REVIEW RETURNED	07-Jul-2021

GENERAL COMMENTS	<p>This is a short paper looking at a single important research question. Hypothetical causes of a rise in homicides were gathered from experts and compared to the observed data. Budget cuts were a purported cause, linked via a reduction in social services. This is observational data, and the authors were cautious in their wording. It feels likely that this paper will receive additional scrutiny post-publication, so I would encourage the authors to document any other potential causes that they did not have data on.</p> <p>It wasn't clear if the experts were aware of the spike in deaths and if this may have influenced their thinking about potential causes. In other words, were the hypothetical causes gathered retrospectively?</p> <p>I think a seasonally adjusted version needs to be used. The peaks in the summer month are clear, and there are two large summer peaks in the key period. It's possible to create a seasonally adjusted count of homicides and then put that data through the control chart. Related to this is a possible adjustment for monthly temperature. It seems likely that either a seasonal or temperature adjustment would be better, rather than both. Adjusting for this is useful because for this analysis the authors are not interested in temperature or season.</p> <p>The model should adjust for the differing number of days in the month, e.g., 28 in February and 31 in August. This is a relatively small source of variance, but is easily adjusted for (e.g., divide all homicides by the number of days in that month and multiply by 30, to give a standard month size). This standardised count can then be seasonally adjusted.</p> <p>A few more details need to be given on the control chart. The plot shows shifts in the upper and lower control chart limits. How were the timings of the shifts determined?</p> <p>Minor comments - Page 3, strengths. Statistical significance is a low bar, especially</p>
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	<p>with such a large data set. Better to focus on the public health significance.</p> <ul style="list-style-type: none"> - Figure 1. One decimal place in this plot is enough. There was no label on the y-axis. The x-axis labels could be neater, perhaps "year-month"? - Line 69, the '5' for the reference looks like sigma to the power of 5. - Line 75, missing space before "homicides" - Data sharing says "No additional data" but sharing this time series and the experts timing should be straightforward and not subject to privacy concerns. - Table on page 12, typo: "guy buy back even[t]"
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REVIEWER	GJ González-Pérez Universidad de Guadalajara
REVIEW RETURNED	16-Jul-2021

GENERAL COMMENTS	<p>I believe that the paper addresses a problem of undoubted relevance; The analysis seems novel to me and offers a different perspective regarding the increase in the homicide rate due to the reductions in public spending for young people; however, I think that some aspects should be reformulated to improve its quality.</p> <ol style="list-style-type: none"> 1. Even when in the abstract it is stated as "Setting: City of Chicago, 2009-2018", in the text itself this is not mentioned, and for the reader unfamiliar with American geography -as can be the public that read BMJ Open- it is not clear if the study refers to the Chicago metropolitan area, Cook County or the city of Chicago; If only juvenile homicides are studied in the city of Chicago, it should be justified why the entire metropolitan area was not studied, which, although it involves three states, shares conurbation areas with a certain spatial similarity. 2. On page 5, rows 52 and 53 it states that "Data were collected from 2010-2019 and analyzed in 2019-2020. " From my point of view this is confusing and can be eliminated, since previously in the introduction it was mentioned that the 10-year period 2009-2018 was analyzed. 3. At no time is the magnitude of the youth population in Chicago mentioned, and I think it is necessary to have a more complete idea of the population being analyzed; Similarly, it would be good to justify why the youth population was considered to be those between 15 and 24 years old (and not until 29 years old, or from 12 years old, for example) 4. Is there no underreporting of homicides in Chicago in the period analyzed? If there were cases, for example, of homicides of unspecified age, was any statistical adjustment made? 5. References must be reviewed. The numbers have no order (first citation 14, then citation 12-14, where is citation 11? "USgovernmentspending.com" -pag.5, row 61- should be cited (it appears as citation 10 in references) but without superscript in the text 6. I think the "Study sample" can be eliminated: The article studies all homicides between 2009 and 2018, not a sample. 7. Although the authors point out that they "Describe all statistical methods, including those used to control for Confounding", I consider that they only state the method and do not describe it in sufficient detail to allow other readers to replicate the analysis used; Since using u-charts is not a broad domain method, I think a further explanation of it would be needed. On the other hand, I would ask the authors if they value using Joinpoint regression, as it could also provide valuable results.
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	<p>8. Was the possibility of doing the analysis also by sex and race / ethnicity not assessed? It would have been perhaps more relevant to see the evolution of homicides in the Afro-American population, which is possibly the one that most resents the lack of public support.</p> <p>9. The authors note that “We found statistically significant shifts in homicide rates over time”. I see that Confidence Intervals overlap; could we then say that the differences are statistically significant if the upper limit of the rate until May 2016 was almost similar to the mean for the period 2016-2017 when the rates reached their peak?</p> <p>10. The figure (U-chart) must have a title and heading on its vertical axis so that it is clear that it is talking about rates and not absolute numbers. Possibly in the Figure legend the symbols used in the graph should appear, and several of the statements should be clarified: for example, what does “CPS graduation rate” mean? Again, the paper will be read by an international audience that does not have to know these acronyms.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Dr. Adrian Barnett, Queensland University of Technology Comments to the Author:

This is a short paper looking at a single important research question. Hypothetical causes of a rise in homicides were gathered from experts and compared to the observed data. Budget cuts were a purported cause, linked via a reduction in social services. This is observational data, and the authors were cautious in their wording. It feels likely that this paper will receive additional scrutiny post-publication, so I would encourage the authors to document any other potential causes that they did not have data on.

It wasn't clear if the experts were aware of the spike in deaths and if this may have influenced their thinking about potential causes. In other words, were the hypothetical causes gathered retrospectively?

We included a sentence stating that experts were interviewed retrospectively (lines 59-60).

We consulted child health, violence prevention, and policy experts to retrospectively generate a list of potentially relevant conditions/events occurring during this time-period (Table 1).

I think a seasonally adjusted version needs to be used. The peaks in the summer month are clear, and there are two large summer peaks in the key period. It's possible to create a seasonally adjusted count of homicides and then put that data through the control chart. Related to this is a possible adjustment for monthly temperature. It seems likely that either a seasonal or temperature adjustment would be better, rather than both. I would opt for the seasonal adjusting as it is less work. Adjusting for this is useful because for this analysis the authors are not interested in temperature or season.

The model should adjust for the differing number of days in the month, e.g., 28 in February and 31 in August. This is a relatively small source of variance, but is easily adjusted for (e.g., divide all homicides by the number of days in that month and multiply by 30, to give a standard month size).

This standardised count can then be seasonally adjusted.

Thank you for this thoughtful comment. We realize that U charts or run charts are not a common method in public health research and therefore some of the features of this methodology may not be front of mind for reviewers. Run charts are premised on a need for a set amount of data points above or below the mean in order to make a determination that a

significant shift in the data occurred. In this case we examined one month time periods with significance determination set at six consecutive data points (months) above or below the mean. If we were to standardize across seasons that would mask seasonal variation in the data which is a key characteristic of homicide rates and potentially of interest to the reader. Adjusting for seasonality would create an artificial flatness to the data across each year. Since determination of significance is set at six unit duration (e.g. 6 consecutive months' worth of data above or below the mean), seasonality does not influence significance as it is not congruent with six-month periods. To help further appreciate this, it should be noted that the study includes 10 years of data and that the summer spikes (well documented in the criminal, sociological and medical literature) and demonstrated annually here, do not lead to changes in the mean outside of the time period when the state does not have a budget.

Regarding adjusting for days of the month. We agree that technically this could be done, however the nature of the variation in the data (variation of 100% in some instances) dwarfs any adjustment that may come from adjusting for the number of days per month. For example, any such adjustment would at maximum result in a 3% adjustment, but since we are talking about variations of 100% or more, this is a negligible. Given this and the word limits for such a brief article, we think keeping per month as the unit of analysis is the appropriate approach. This approach also allows the reader to better appreciate the seasonality of the data.

A few more details need to be given on the control chart. The plot shows shifts in the upper and lower control chart limits. How were the timings of the shifts determined?

Upper and lower control chart limits are re-calculated when the mean shifts, i.e. when there are 6 or more consecutive datapoints that are either above or below the mean. Both the mean and upper and lower control limits are calculated based on this data. For an excellent description of the use of control charts by the National Health Service, please see <https://www.england.nhs.uk/improvement-hub/wp-content/uploads/sites/44/2017/11/A-guide-to-creating-and-interpreting-run-and-control-charts.pdf>. We added this resource as a reference (10) on line 79.

Minor comments

- Page 3, strengths. Statistical significance is a low bar, especially with such a large data set. Better to focus on the public health significance.

On lines 25-26, we changed the language to reflect the public health significance of firearm homicide and noted that state budget absence is a modifiable condition.

Addresses the significant public health problem of firearm homicide and tests associations with a modifiable condition, absence and re-establishment of a state budget, and a corresponding spike and decline in Chicago youth homicide rates.

- Figure 1. One decimal place in this plot is enough. There was no label on the y-axis. The x-axis labels could be neater, perhaps "year-month"?

Multiple adjustments have been made to improve the readability of Figure 1- thank you for the suggestions. They include limiting to one decimal point, larger fonts, labels for the X and Y axis, and change to the axis labels to improve readability.

Line 69, the '5' for the reference looks like sigma to the power of 5.

We edited lines this to read (now on lines 90-91):

We set control limits at 3σ in accordance with established standards.

Line 75, missing space before "homicide"

A space was added.

Data sharing says "No additional data" but sharing this time series and the experts timing should be straightforward and not subject to privacy concerns.

We edited this with a statement saying that data are available upon request from the corresponding author.

Table on page 12, typo: "guy buy back even[t]"

We deleted this table, but corrected this on the figure legend. This now reads "Gun buyback event"

Reviewer: 2

Dr. GJ González-Pérez, Universidad de Guadalajara Comments to the Author:

I believe that the paper addresses a problem of undoubted relevance; The analysis seems novel to me and offers a different perspective regarding the increase in the homicide rate due to the reductions in public spending for young people; however, I think that some aspects should be reformulated to improve its quality.

1. Even when in the abstract it is stated as "Setting: City of Chicago, 2009-2018", in the text itself this is not mentioned, and for the reader unfamiliar with American geography -as can be the public that read BMJ Open- it is not clear if the study refers to the Chicago metropolitan area, Cook County or the city of Chicago; If only juvenile homicides are studied in the city of Chicago, it should be justified why the entire metropolitan area was not studied, which, although it involves three states, shares conurbation areas with a certain spatial similarity.

This is an important point and we have made edits to include more context.

In the introduction (lines 43-44), we added specification regarding the focus on the City of Chicago, located in the state of Illinois.

The city of Chicago, Illinois has a persistent youth homicide problem and recently experienced marked oscillations in youth homicides rates.

Additionally in the Methods section (lines 79-84), we clarify reasons behind our focus on youth in Chicago:

We limited our study to youth within the city limits of Chicago, Illinois as this city is an epicenter of youth violence in the region and has entrenched history of youth violence since the 2000s, which continues to the present.

2. On page 5, rows 52 and 53 it states that "Data were collected from 2010-2019 and analyzed in 2019-2020." From my point of view this is confusing and can be eliminated, since previously in the introduction it was mentioned that the 10-year period 2009-2018 was analyzed.

This sentence was deleted.

3. At no time is the magnitude of the youth population in Chicago mentioned, and I think it is necessary to have a more complete idea of the population being analyzed;

Again, an important contextual point. We added the following on lines 45-48:

Youth between the ages of 15 and 24 are overrepresented among homicide decedents in Chicago. In 2015, Chicago youth between the ages of 15 and 24 years made up 13.9% (378,766)⁵ of the City's population but accounted for 39.3% of Chicago homicide deaths.⁶

Similarly, it would be good to justify why the youth population was considered to be those between 15 and 24 years old (and not until 29 years old, or from 12 years old, for example). I

In explanation, we added the following on lines 75-84:

This age group was chosen based on the standardized age categories for teenagers and young adults in the U.S. Census.⁵ This age group is of particular interest because it is a specific human developmental period encompassing biological maturation changes, cognitive development, and a transition period of social roles, suggesting unique vulnerabilities and opportunities for violence prevention.⁹ We limited our study to youth within the city limits of Chicago, Illinois as this city is an epicenter of youth violence in the region and has entrenched history of youth violence since the 2000s, which continues to the present.

Is there no underreporting of homicides in Chicago in the period analyzed? For example, of homicides of unspecified age, was any statistical adjustment made?

This is a good point. We use the gold standard for counting homicides –death certificate data with manner of death determined by careful review of coroner/medical examiner and law enforcement reporting and determination. We are confident that the National Violent Death Reporting System from which we acquired the study data does not undercount homicide deaths. We do note, however, that from 2015 to 2018 there were eight homicide deaths with missing age information. This represents 0.1% of deaths over that period and is negligible given the annual number of homicides.

5. References must be reviewed. The numbers have no order (first citation 14, then citation 12-14, where is citation 11? "USgovernmentspending.com" -pag.5, row 61- should be cited (it appears as citation 10 in references) but without superscript in the text 6.

These have been corrected. We are sorry for the inaccuracies and errors.

I think the "Study sample" can be eliminated: The article studies all homicides between 2009 and 2018, not a sample.

This was deleted.

7. Although the authors point out that they "Describe all statistical methods, including those used to control for Confounding", I consider that they only state the method and do not describe it in sufficient detail to allow other readers to replicate the analysis used; Since using u-charts is not a broad domain method, I think a further explanation of it would be needed.

We appreciate this observation and agree that ideally we would include a detailed description of this method. However, word count limitations do not allow for an extended explanation in an article of this length. We were able to add the following on lines 86-91:

This method allows for identification of significant shifts in the mean of variables of interest. Run chart methodology requires a set amount of data points above or below the mean in order to make a determination that a significant shift in the data occurred. We examined one month time periods with significance determination set at six consecutive data points (months) above or below the mean. We set control limits at 3 σ in accordance with established standards.

Additionally, we include two references: Benneyan JC, Lloyd RC and Plsek PE. Statistical process control as a tool for research and healthcare improvement. Qual Saf Health Care 2003;12(6): 458-464 and National Health Service, *A guide to creating and interpreting run and control charts*. Coventry UK: Coventry House, University of Warwick; 2009 to assist readers in further understanding this method and its uses in health research.

On the other hand, I would ask the authors if they value using Joinpoint regression, as it could also provide valuable results.

We appreciate this suggestion and value Joinpoint as a method to examine trends over time. However, the focus of this paper was to apply run chart methodology in a novel way to allow for consideration of potential environmental influences on youth homicide in Chicago over time. Use of Joinpoint methods are beyond the scope of this study.

8. Was the possibility of doing the analysis also by sex and race / ethnicity not assessed? It would have been perhaps more relevant to see the evolution of homicides in the Afro-American population, which is possibly the one that most resents the lack of public support.

We understand this is a question many readers may have. However, as we point out in the Results section and in Table 1, Sex and Race/ethnicity are not evenly distributed in youth homicide decedents in Chicago. The vast majority of youth homicide decedents are male (92.9%) and Black (79.1%). This has been consistent over the years and there has not been an appreciable evolution in the sex and race/ethnicity distribution of youth homicide decedents during the study period. As such it not a viable line of inquiry for these data. That said, we do appreciate the comment and have included the following statement in the Discussion section:

(Lines 113-115)

In this study of Chicago youth homicides, we found that homicides are heavily concentrated among male and Black youth. Importantly, we found an association between increases in youth homicide and the absence of a fully funded state budget.

9. The authors note that “We found statistically significant shifts in homicide rates over time”. I see that Confidence Intervals overlap; could we then say that the differences are statistically significant if the upper limit of the rate until May 2016 was almost similar to the mean for the period 2016-2017 when the rates reached their peak?

In run chart analysis, the determination of significance is made solely based on whether the data points are above or below the mean. In other words, Confidence Intervals are not an indicator of significance as they are in other analytical methods. Further, as these data are a census of all youth homicides in Chicago during the study period, there is no need to estimate the parameters for significance determination as our data represent the true population values.

10. The figure (U-chart) must have a title and heading on its vertical axis so that it is clear that it is talking about rates and not absolute numbers.

We added vertical and horizontal axis titles: Youth Homicide Rate per 100,000 per Month to the vertical axis and Year-Month to the horizontal axis.

The figure title is: Chicago Youth Homicides, 2009-2018

Possibly in the Figure legend the symbols used in the graph should appear, and several of the statements should be clarified: for example, what does “CPS graduation rate” mean?

Again, we appreciate your comments to provide context as to the local setting and clarity regarding the chart. We have made these changes: CPS (Chicago Public Schools) is spelled out in first use and symbols were added to legend. To avoid duplication, we also removed table 1 as it is essentially the same as the figure legend.

VERSION 2 – REVIEW

REVIEWER	Adrian Barnett Queensland University of Technology, Institute of Health and
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	Biomedical Innovation
REVIEW RETURNED	13-Oct-2021

GENERAL COMMENTS	<p>I disagree with the authors that "seasonality does not influence significance as it is not congruent with six-month periods" using a six month reference time will not control for season. A six month window will still pick up seasonal changes, for example imagine the peak of summer being compared with the previous six months which would include winter months. The U-chart could easily flag this known seasonal pattern as special cause variation. I believe the authors want to control for season as the discussion says "Our study demonstrates that this spike extends beyond summer months" but the current methods cannot do that. However, in the response the authors state that adjusting "would mask seasonal variation in the data which is a key characteristic of homicide rates and potentially of interest to the reader". So it's not clear whether season is an important factor or a nuisance to be controlled for. One solution is to present both methods. It could well be sensible to control for season as that's a pattern that is well established whereas the authors are interested in the effects of policies. These policies could easily look effective (or ineffective) depending on when they were introduced in the annual seasonal cycle.</p> <p>The authors either need to adjust for season, or add a limitation saying that the results are not seasonally adjusted and that the observed shifts in the control chart could be wholly or partly explained by the clear seasonal pattern in the data.</p> <p>Minor comments</p> <ul style="list-style-type: none"> - the rates in table one could be presented with fewer decimal places - I think the first sentence of the limitations needs re-wording - The methods need to say that u-charts were used
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REVIEWER	GJ González-Pérez Universidad de Guadalajara
REVIEW RETURNED	28-Oct-2021

GENERAL COMMENTS	<p>As I had already mentioned in my previous review, I think that the paper addresses a problem of undoubted relevance and opportunity; The analysis seems novel and offers a different perspective regarding the increase in the youth homicide rate due to the reductions in public spending for young people. In general terms, I consider that my comments and recommendations were taken into account by the authors and we have a qualitatively better manuscript. However, there is one question that the authors should clarify in the document, even briefly (if it possible): the source from which the data was acquired (National Violent Death Reporting System) not undercount homicide deaths. Regardless of the last point, I think that the article should be published.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Dr. Adrian Barnett, Queensland University of Technology Comments to the Author:

I disagree with the authors that "seasonality does not influence significance as it is not congruent with six-month periods" using a six month reference time will not control for season. A six month window will still pick up seasonal changes, for example imagine the peak of summer being compared with the previous six months which would include winter months.

The U-chart could easily flag this known seasonal pattern as special cause variation. I believe the authors want to control for season as the discussion says "Our study demonstrates that this spike extends beyond summer months" but the current methods cannot do that. However, in the response the authors state that adjusting "would mask seasonal variation in the data which is a key characteristic of homicide rates and potentially of interest to the reader". So it's not clear whether season is an important factor or a nuisance to be controlled for. One solution is to present both methods. It could well be sensible to control for season as that's a pattern that is well established whereas the authors are interested in the effects of policies. These policies could easily look effective (or ineffective) depending on when they were introduced in the annual seasonal cycle.

The authors either need to adjust for season, or add a limitation saying that the results are not seasonally adjusted and that the observed shifts in the control chart could be wholly or partly explained by the clear seasonal pattern in the data.

Thank you for this suggestion. We have added a sensitivity analysis in the form of a negative binomial regression model.

We report this on lines 43-44 in the abstract: *Adjusting for seasonality, we found death rates were greater in the months without a budget compared to months with a budget ((95% confidence interval) = 1.48 (1.29, 1.70)).*

And lines 112-119 in the methods section of the manuscript:

To assess the association between the state budget and homicide rates while controlling for season, we estimated a negative binomial regression model. Overall homicide count serves as the dependent variable and a binary budget variable (months with a budget vs. months without a budget), season (coded as a four-level categorical variable with the following monthly groupings: December-January-February, March-April-May, June-July-August, and September-October-November) and calendar year as the independent variables. The log of the monthly population estimate serves as the offset. We also repeated this model while using a trichotomous budget variable (initial budget period, period without budget, period with budget reinstated).

And lines 139-147 in the results section of the manuscript:

The regression model with binary budget periods found that compared to having a budget, non-budget months were associated with a significant 48% increased death rate (incidence rate ratio (IRR) (95% confidence interval (95%CI)) = 1.48 (1.29, 1.70)), adjusting for season and year. The regression model with categorical budget periods found that, compared to the initial budget period, the no-budget period was associated with an increased risk of death (IRR (95% CI) = 1.37 (1.13, 1.66)). Also, the no-budget period was associated with an increased risk of death compared to the reinstated budget period (IRR (95% CI) = 1.59 (1.32, 1.90)). The predicted death rates per 100,000 persons (95% CIs) across the three budget periods were: 1st budget: 4.20 (3.92, 4.48), no budget: 6.69 (5.99, 7.39), 2nd budget: 4.57 (3.95, 5.19)

Minor comments

- the rates in table one could be presented with fewer decimal places

All values in Table 1 are now limited to one decimal place.

- I think the first sentence of the limitations needs re-wording

Thank you for catching the incomplete sentence. We have We have revised lines 190-191 to read: This study's limitations include our inability to control for every potential factor that may have influenced youth homicide rates over the study time-period.

- The methods need to say that u-charts were used

The methods section now includes the following text on lines 103-111: We applied standard statistical process control methods (U-charts) to identify significant temporal shifts in monthly

homicide rates. This method allows for identification of significant shifts in the mean of variables of interest.¹¹ Run chart methodology requires a set amount of data points above or below the mean in order to make a determination that a significant shift in the data occurred. We examined one-month time periods with significance determination set at six consecutive data points (months) above or below the mean. We set control limits at 3 σ in accordance with established standards.¹² Next, we plotted conditions/events onto the run chart to identify possible temporal associations between conditions/events and fluctuations in homicides.

Reviewer: 2

Dr. GJ González-Pérez , Universidad de Guadalajara Comments to the Author:

As I had already mentioned in my previous review, I think that the paper addresses a problem of undoubted relevance and opportunity; The analysis seems novel and offers a different perspective regarding the increase in the youth homicide rate due to the reductions in public spending for young people. In general terms, I consider that my comments and recommendations were taken into account by the authors and we have a qualitatively better manuscript.

However, there is one question that the authors should clarify in the document, even briefly (if it possible): the source from which the data was acquired (National Violent Death Reporting System) not undercount homicide deaths. Regardless of the last point, I think that the article should be published.

Thank you for your comments. We have made edits in two places in response to this suggestion.

In the abstract on line 31, we have revised the Participants sub section to include the addition of: Homicide count data come from the National Violent Death Reporting System.

In the Methods section under the Data Sources subheading we revised to read as follows including the addition of a citation on the extensive use of use NVDRS for homicide research.

Data sources Homicide data were drawn from the Illinois Violent Death Reporting System, part of the National Violent Death Reporting System (NVDRS).⁷ NVDRS data come from death certificate and coroner/medical examiner, toxicology and autopsy reports and are used extensively in homicide research.⁸

VERSION 3 – REVIEW

REVIEWER	Adrian Barnett Queensland University of Technology, Institute of Health and Biomedical Innovation
REVIEW RETURNED	16-Nov-2021
GENERAL COMMENTS	<p>The authors have answered all my queries about the seasonal issue.</p> <p>Just a few very minor points.</p> <ul style="list-style-type: none"> - replace "case study" key word with "homicide" - the 2nd strengths bullet point and line 73 talks about a "novel" application of U-charts, whereas line 103 describes them as "standard". I think they are standard. - it's not clear why the data can't be openly shared with the paper given that it is routinely collected with no ethical concerns