Reviewer's report

Title: The effect of seasonal changes and climatic factors on suicide attempts of young people

Version: 0 Date: 25 Jul 2017

Reviewer: Stephen R. Hill

Reviewer's report:

This paper examines an interesting area of research but does not offer anything particularly new - there are many extant correlational studies that explore this relationship. In addition there are a number of questions about the analyses used and several claims made within the manuscript are incorrect. Thus, in it's current state we do not believe this paper is ready to be published.

Specific Points

1. It is unclear why the authors have used the mean temperature over the preceding 10 days in half of the analyses - no rationale is given for this which makes it seem arbitrary (and suspicious people might wonder whether this was a post-hoc decision used to 'track down' a statistically significant effect). A lagged analysis over a variety days would provide a more comprehensive picture.

2. It's not clear what how the term 'temperature' is used in the study - is this daily average, daily high, or something else?

3. No descriptive statistics are provided for data. It is stated that Spearman's rho was used 'just in case' there are nonlinearities in the data - why weren't statistics for normality and skewness explored and reported?

4. It's not clear how seasonality is 'computed' - if it's just a simple quadratic function derived from the 2010 data then it's hard to know why we should take the deviations from it seriously. A quadratic function is an unusual choice that won't produce the repeating sinusoidal function that we would expect of a seasonally changing variable like temperature. It fits the data within the single year okay, but would obviously be implausible with a longer series. Mean temperatures using a longer series of data should be calculated, or the variation modelled using a trigonometric model (which would be more standard for dealing with seasonality in a time series analysis).
5. The temperature analyses do not control for other factors like sunlight/radiation. In research we have conducted we found that controlling for radiation eliminates the effect of geographical temperature variation even though the effect of radiation was not significant, so a lack of bivariate relationship between radiation and suicides is not a sufficient reason for doing more sophisticated analyses that control for other potential confounds.

6. The study uses time series data but there's no attention paid to the possibility of autocorrelated errors, the presence of which would cause downwardly biased standard errors.

7. There is no information given about the size of the geographical area in which the data were collected or temperature variations across that area.

8. It would be helpful to have the effects stated in terms of increase in suicide rate per degree of temperature change/difference.

9. Seasonal changes might be confounded by non-temperature-related changes in calendar-related cycles - social, economic etc. This should be acknowledged or controlled for (as best as possible). The effects of temperature anomalies / irregular variation in temperature (data with seasonality removed) should be unconfounded though (natural experiment sort of situation).

10. Description of statistics needs more detail (e.g., there are no chi square scores provided).

11. It is not clear why the season-gender chi-square analysis has df3 - what exactly is being analysed here? If this is analysis of data in a 1x2 or 2x2 contingency table the df would not be 3.

12. No explanation is provided about why some of the climatic data was missing.

13. The claim on p. 3 that this is the first study examining the influence of seasonal changes and climactic variations on young suicide attempters isn't correct - see for instance https://www.karger.com/Article/Abstract/98265. There are also many studies that include analyses of data from young people.
14. The claim on page 3 that suicide is most common between ages 15 and 25 is wrong - most studies find it peaks much later in life (e.g., https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1446422/).

15. There's no pre-registration. In the absence of that, it would be good to see how the results hold up under alternative choices of statistical analyses (for the same research questions) - the lagged analyses mentioned earlier might be one way of approaching this issue.

16. There are no open data - I can understand that the authors might have an agreement with the data provider that the data can't be openly posted, but we need more than a statement that "The datasets used are not publicly available". At the least there should be some indication of a willingness to share the data with other researchers who contact them.

17. Although the limitations section notes that social and psychological factors are not considered in the paper it's not clear why this is the case. Given the fact that the study does not test the BAT or serotonin models of the temperature-suicide relationship these don't need to be covered in much detail. Space could be made to make discuss possible explanatory non-biological factors.

18. Some of the written expression needs tidying up - there are problems with tense, plural agreements, and a number of other minor typos that need attention.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

No

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Unable to assess
Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review? If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

Quality of written English
Please indicate the quality of language in the manuscript:

Needs some language corrections before being published

Declaration of competing interests
Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare that I have no competing interests

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments
which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal