**Author’s response to reviews**

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

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Authors' response to the reviewer’s comments:

The authors wish to thank the reviewers for their careful reviewing and the valuable suggestions that we tried to follow carefully.

All changes in the manuscript were marked in red.

Saxby Pridmore (Reviewer 1): Two minor typographical points - the authors use the acronym BAT, without defining it (as far as I can see), and the words "These towards" could use an additional word.

The authors write this information will be valuable for "developing preventive health-care measure" and "in order to reduce suicidal behavior - which is doubtful (in my opinion) and could be removed."
Thank you for the suggestions, all recommended changes were done in the manuscript.

Stephen R. Hill (Reviewer 2): 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 its current state we do not believe this paper is ready to be published.

We have performed the following changes:

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 of days would provide a more comprehensive picture.

   Thank you for the suggestion.

   This was definitely not a post-hoc decision to track down any effect, to the contrary. Previous work shows that the relationship between meteorological variables and suicides can be on the same day or over certain time spans in the past. In order to limit the number of tests, one representative time span of 10 days was chosen a priori. A lagged analysis would have required a much stricter correction of alpha levels.

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

   Thank you for the question.

   In the present study, temperature was used as daily average temperature of Istanbul.
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?

Thank you for the suggestion.

We do not believe that descriptive statistics would add much information. (Temperature, as an example, is depicted graphically in figure 2.) In particular, it would not reveal non-linearities or outliers that would require Spearman over Pearson correlation. Again, the decision to use Spearman was driven by the goal to keep the number of tests low. It is a general recommendation in statistics to use Spearman correlation if one needs to choose between the two, since it is more general and does not have much less power than Pearson’s.

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).

Thank you for the suggestion. This has been changed accordingly in Figure 2.

We agree that a trigonometric function is more appropriate. We have therefore recalculated the removal of seasonality using a sine function, as well as the subsequent correlations.

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.

Thank you for the suggestion. This has been changed accordingly on page 7, paragraphs 2 –4.
We performed a partial correlation as a post-hoc analysis on the highly significant result involving temperature.

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.

Thank you for the suggestion.

Removing seasonality should remove a large part of autocorrelations. Standard errors are not looked at in the analysis.

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.

Thank you for the suggestion. This has been changed accordingly on page 4, second and fourth paragraph.

The present study was conducted in Istanbul, one of the big metropolitan regions of the world, with approximately 17 million inhabitants and an area of 5,343.02 square kilometres (Türkiye İstatistik Kurumu, 2014).

There were temperature variations across the different areas of Istanbul. As we analyse whole temperature variations of overall Istanbul, we have to take the average temperature.

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

Thank you for the suggestion. This has been changed accordingly in the manuscript.

We added the coefficient for a regression model for the most significant correlation.
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).

Thank you for the suggestion.

Data not available, therefore we cannot control for it.

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

Thank you for the suggestion. This has been changed accordingly in the manuscript.

All important details are given for correlations. We added the Chi Squared value for the respective test.

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.

Thank you for the question.

There are four seasons and two sexes, therefore it is a 4x2 contingency table, for which df=3.

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

Thank you for the question.

Our study is based on the available data from the General Meteorological Department of Ankara (Turkey). On several days, there were no meteorological data available due to technical faults.

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
There are also many studies that include analyses of data from young people.

Thank you for the suggestion.

The study of Lahti et al., 2006 analysed victims in shooting suicides and its correlation with seasonal patterns, among 42 adolescents living in Finland.

Contrarily, we analysed seasonal and meteorological patterns of 2,131 young suicide attempters living in Istanbul. In the present study, we focused on suicide attempts and did not include completed suicides, so the study of Lahti et al., 2006 is quite different to our study.

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/).

Thank you for the suggestion.

The study of Spicer et al., 2000, which was conducted in the U.S., shows a peak among suicide attempters aged between 25 and 44.

Previous studies, Doganay et al., 2003, Breuer et al., 1986 and Chiu et al., 1988, which, were conducted in European and Asian regions, show a peak of suicide attempters aged between 15 and 25.

We assume that the regional differences may play a role at the peak of the age range of the suicide attempters.

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.

Thank you for the suggestion.
Exactly because this study was not pre-registered, we wanted to design the analysis as straightforward as possible, limiting the number of variables and tests to be performed. Now that we added a couple of post-hoc analyses, upon the reviewer’s suggestion, we feel that any additional analysis would indeed be prone to come up with spurious results due to multiple testing (see our above comment on the lagged analysis).

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.

Thank you for the suggestion.

We got the data about the meteorology from the General Meteorological Department of Ankara (Turkey). We got the permission to use the provided data only for the present study. Therefore, we are not able to pass the information, but we can give the contact details to get the information about the meteorology. The same is valid for the information about suicide attempts, which we got from Health Directorate of Istanbul, Turkish Ministry of Health.

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 discuss possible explanatory non-biological factors.

Thank you for the suggestion.

In the present study we focused on the relationship between the suicide attempts and the different climatic/meteorological indicators, therefore we did not analyse social and psychological factors of suicide attempts.

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.
Thank you for the suggestion.

We did proofreading for the entire manuscript.