Reviewer’s report

Title: EFFECTS OF POLLUTION, LOW TEMPERATURE AND INFLUENZA SYNDROME ON THE EXCESS MORTALITY RISK IN WINTER 2016-2017

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Reviewer: Peter Franklin

Reviewer's report:

This study has investigated effects of various risk factors on mortality. The study design is reasonable, although more detail is required in places. The conclusions are speculative in parts and the manuscript needs some editing.

Abstract:

The Abstract needs editing (as does the entire article). For example, the first sentence does not make sense. Also 'influential syndrome' is not a term - I guess the authors mean 'influenza syndrome' (this also needs to eb changed in the Introduction - lines 77 and 81, and conclusion - lines 285 and 287)

Conclusions: The authors cannot state that excess mortality is linked to individual choices (eg vaccination) when there are no data on vaccination for this study. Also it is not possible to say that these results can be directly related to climate change. This is pure speculation and even if there is some truth that the risk factors are affected by climate change, this study does not confirm that is the case.

Introduction:

Editing for this sections is also required. The third paragraph is particularly poorly organised as it starts with heatwaves and then shifts suddenly to cold temperatures.

I am not sure why heatwaves are mentioned in the Introduction as this study was conducted in the European winter and the effects of cold, not hot, weather was investigated.

The authors state that both temperature and influenza are measured as confounders in air pollution studies. Although this is correct, they are known risk factors for the outcomes of interest and have been studied as primary exposures. Therefore, the implication that they are mostly studied as confounders in air pollution studies is a bit misleading. I think the authors can suggest that they are important confounders for air pollution studies but don't imply that the only interest in these 'exposures' is as confounders.
Methods:

In the statistical analyses the authors state that 'To respond to relevant health prevention issues, we have constructed dummy variables for each exposure'. I'm not sure what 'To respond to relevant health prevention issues' means and why the exposures have to be dichotomised for that reason. Can this be explained. There is a reasonably well accepted linear (or supralinear) no threshold dose-response relationship for PM and mortality.

Results:

I'm not sure why there is a comparison of mortality with the previous year (2nd paragraph). I don't dispute that there were increased deaths in 2016/17 but there are fluctuations in flu seasons over any number of years and a comparison just with the previous year can be meaningless because of these year-to-year fluctuations. I don't think it is necessary to include this.

The measurement units (e.g., 10μg/m³ for PM, 1°C for temp and 1/10³ for ILI cases) need to be included in the first paragraph where relevant results are presented, in this case the final paragraph on p9. It is confusing in that paragraph as that the authors state a 2.02% increase in mortality for PM10 but an OR of 1.002 is actually a 0.2% increase. Therefore I assume the unit of measure for the analyses was 1μg/m³ but the authors are reporting an increase per 10μg/m³.

I think the Figures and tables can be reduced and improved. I don't think Fig 1 is necessary and Table 2 could be submitted as supplementary material. In Table 3 I don't know what a - h stand for. There is no explanation in text or as footnote to the table.

Discussion:

What do the authors consider an inversion phenomenon (second paragraph)? Were temperature inversions measured and included in any analyses (or is it just assumed)?

As with abstract the assumption of an effect of climate change is pure speculation and the results do not 'suggest' this at all. I don't mind the authors mentioning that climate change may make these interactions more likely in the future (probably toward the end of the Discussion) but it is premature to interpret the results through the prism of climate change, and spurious to do so from a single study.

The 7th paragraph (from line 255) gives an explanation that suggests a threshold for PM10 (70 μg/m³) (I think that is what is suggested). However, this is contrary to current understanding of the dose-response relationship between PM and mortality. I reiterate my concern about dichotomising the continuous variables. It would be better to investigate the shape of the dose-response curve using the continuous data.

Last paragraph - it is difficult to make any conclusions about vaccination when the data for individuals are not available for the study and could not be controlled in the analyses.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
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Yes

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