Reviewer's report

Title: Apparent temperature and acute myocardial infarction hospital admissions in Copenhagen, Denmark: A case-crossover study

Version: 2 Date: 13 March 2012

Reviewer: Sally Picciotto

Reviewer's report:

The authors have satisfied my concerns about the analysis itself. In particular, they have run analyses adjusting for each of the three pollutants (PM10, NO2 and CO) in both seasons. The results are convincing and consistent. In addition, they have improved the Methods section, by including motivation for and a clear definition of the exposure variable Tapp_max, and the Results section, by expanding the exposition.

They have also made a fair effort that has dramatically improved the flow of the writing. Notwithstanding the lack of critique from previous reviewers of their other, similar papers, I believe they have substantially improved this manuscript by incorporating a number of the suggestions they received about the initial version. The Discussion section is still a little choppy but has been improved by the inclusion of more comparisons with prior studies. Although a few of the authors' stylistic choices are not the ones I would have made, the manuscript is now much more readable and has a clearer structure.

Minor compulsory revisions:

1. Throughout: Replace “lack to” by “lack of”.

2. Page 9: Regarding the problem with measurements of relative humidity, providing supplemental material to explain the issue is an excellent solution. However, I think the supplemental material itself needs a little bit more work.

Supplemental material: Please be more explicit about the error in the measurements. The first sentence states that there is measurement error and attributes it to a probable calibration problem; the second sentence states that the RH values decreased over time. The graph very helpfully shows this decrease in RH measurements over the study period. As the text stands now, it sounds as if the true values of RH decreased over time, which I suspect is the opposite of what the authors intend to state. I imagine that the authors intend the reader to assume that this decrease in the measurements is due to calibration problems (the error mentioned), rather than representing a real decrease in Copenhagen’s relative humidity over the study period. If I am incorrect, then it is even more essential that the error be explained explicitly since the reader is unable to guess at the intended meaning. However, even if I am correct, the authors ought to explain explicitly rather than leaving the reader to make assumptions.
3. Supplemental material figure 2: I had to study this graph for several minutes to see how it supported the authors’ assertion that the small error in relative humidity had only minor impact on Tapp_max, because the graph contradicts this assertion for very low levels of relative humidity. However, there are no measurements below 30% relative humidity, above which value the graph does support the assertion. Therefore, consider displaying only the portion of this graph that represents measurements that actually occurred in Copenhagen during the study period. Alternatively, the accompanying text could make this point.

4. Page 13: “The majority of admissions ... occurred in... highest SES. The admission rate decreased from the lowest to the highest SES group.” These two sentences appear to contradict one another. It appears that the majority of admissions occurred in the lowest two SES categories, even though the effect of temperature (as seen later in the paper) was strongest in the highest SES group. Since this paragraph is about the pattern of admissions, not the effect of temperature, I think that the last sentence is correct and the previous sentence should be corrected.

5. Page 14: The middle two paragraphs on this page begin with “Figures” instead of “Figure”.

6. Page 18-19: The authors have added a sentence to motivate the paragraph on air pollutant results, which does help, but some discussion of the nature of the confounding (which is, after all, the real reason pollutants are included in this paper about the effect of temperature on AMI admissions) is still called for. For example, in comparing figures 1 and 3, adjustment for the air pollutants in cold season does not seem to have much effect on the air pollution effect estimate at the different lags, except perhaps for CA5. The fact that the effect is robust to these adjustments suggests that air pollutants are not solely responsible for the higher risk during colder weather; this would be an appropriate point to make in this section. Furthermore, the CA5 association is strengthened by adjustment for each air pollutant. Is it therefore possible that some previous studies may have missed the relationship between temperature and AMI because they did not adjust for air pollutants?

7. Page 19: First paragraph has an extraneous “of” in the sentence that cites reference 12.


Discretionary revisions:

9. Page 5, end of first paragraph: “air pollution may interact with temperature” seems to imply that the authors plan to study effect modification by air pollution, rather than adjust for air pollution in their analysis. I believe it would be clearer to state that some of the effect of temperature may occur through pathways involving air pollution but that the effect of temperature on health, independent of air pollution, is also of interest.
10. Page 6: Consider changing “Yet, two of the 16…” to “Yet, only two…” since the sentence appears to be criticizing these two studies for considering confounding by air pollution, instead of criticizing the other 14 for not considering it.

11. Page 9: The sentence explaining that missing data were excluded from analysis is now slightly redundant, so consider removing “not imputed and” as well as “automatically”; the sentence will read more smoothly.

12. Page 15: The sentence “The GAM analyses confirmed the insignificant association … with either similar, weaker, or stronger associations than those of the case-crossover analysis.” Not to be too pedantic, but naturally the associations were “similar, weaker, or stronger”—those are all of the possibilities. It might be clearer to state that although some of the associations were weaker or stronger than in the case-crossover analysis, all warm season associations were still insignificant in the GAM analysis.

13. Page 18-19: Perhaps the explanation about PM10 urban background levels in Copenhagen not being traffic-related belongs at the very end of the paragraph, after the discussion about PM2.5, which still feels out of place. This would help tie the paragraph together.

14. Page 19: The limitations of this study should include the inability to adjust for PM2.5, since the authors point out that it is the air pollution measure most strongly associated with AMI.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.