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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Pollutant emission reductions deliver decreased $PM_{2.5}$-caused mortality across China during 2015–2017" *by* Ben Silver et al.

Anonymous Referee #2

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The manuscript presents a study on estimating the changes of mortality due to air-pollutants in China in 2015-2017 and explaining the causes of it using WRF-Chem simulations. For this, modeled trends are compared with observed ones to provide reliability in the model estimates. This study represents good contributions to the field and it's within the scope of ACP. I think the paper needs a bit more work before it's ready for publication based on the comments below.

My main comment is the following. Given the issues in the modeled trends of the PM2.5 precursors (SO2, NOx), getting the right trend for PM2.5 could be do to a cancellation of errors, so you might be getting the right trend for the wrong reasons. I would like to

encourage the authors to look into more details on this topic. For instance, analyze the model results by aerosol composition and how are the trends of each specie to assess the role of each of them. I would also encourage the authors to collaborate with other researchers that maintain sites where this speciation is observed and so the speciated comparison can be done as well. An example is the Beijing site from the Spartan network (https://www.spartan-network.org/beijing-china), but I'm sure there are many more. Even if a few sites are included this could provide useful information.

Comments by line:

66-70. Please list some references on the second approach.

79. Please briefly summarize the quality control process

Section 2.2. Any previous work where you have used this or similar configuration with positive results in terms of meteorology, PM2.5 and O3? In this work you are not much model evaluation other than the evaluation of the trends and brief statistics in sections 2.3. Adding evaluation on the ability of this model configuration to capture aerosol speciation would also be desirable.

103 Hodzic and Jimenez, and Knote et al. papers described two very different SOA schemes, please specify which one you are using.

Section 2.4. Can you briefly describe the exposure response functions used for PM2.5 and O3?

129. After reading section 3.1, I don't think the trends compare largely well as stated in this sentence. You could make this point for PM2.5, but for the others, although the sign is generally correct, the magnitudes tends to be off by at least factor of 2. For the case of NO2 the sign of the trend is not even well captured. Please revise to better represent the actual results

129-136. Can you add additional analysis in whether the model captures the regions with more negative (and more positive) trends? I see this info in the plots but it's not

discussed

150. Is clear to me that natural emissions remained equal in the two emission scenarios, but what did the authors do for biomass burning emissions?

153-154. Chen et al. (2019) also found that there were periods where meteorology did play a role, can you compare your results to theirs?

155-156. I would say “little influence” rather than “no influence” as you are basing your analysis in a model that contains uncertainties.

166-167. This statement depends if the region is NOx or VOC limited. Might be good to include these indicators from the model perspective to shows that this is what’s actually happening in the model.

168. The Li et al. (2019a) study blames heterogeneous chemistry happening in declining particles for the negative trend. Is this process included in this WRF-Chem configuration and how this influences your results? An attempt to compute similar metrics as in the Li study might be good to intercompare results.

191-196. Is not clear in this paragraph where you consider the model issues on O3 trends.

Minor Edits

75. I believe ACP policy is to not use links but references, please check.

124. Fix issue with the symbol after 0.05

329. Should this be 2019b?

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