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Comment

Interactive comment on “Sensitivity of PM_{2.5} to climate in the Eastern U.S.: a modeling case study” by J. P. Dawson et al.

J. P. Dawson et al.

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General comments: It is understood that in the “real world” meteorological parameters do not change in isolation of one another. A modeling study that changes meteorological variables individually allows the study of something that cannot directly be tested in the real world, i.e. the response of PM concentrations to changes in individual meteorological parameters. We believe this is an important first step toward determining how PM concentrations may respond to changes in the climate. We do agree with the reviewer that this should be viewed as a sensitivity study to specific meteorological parameters. The results here should be helpful in understanding the response of the PM in realistic climate change scenarios, when all the meteorological parameters change together. This is now explained in the abstract, introduction and conclusions of the paper.

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Specific comments:

1. Page 6488 Line 22: "indicate that changes in climate may have important impacts on PM2.5 concentrations": This study cannot conclude that. The fact that independent meteorological parameters affect PM2.5 concentrations doesn't mean that climate change will affect pollutant concentrations (see general comments)

In this sentence, the word "may" has been changed to "could potentially". We mean to convey that there is a potential for important impacts, not that there will necessarily be important impacts. This new phrasing should make this point more clearly.

2. Page 6489 Line 18: "Emission control policy is currently made assuming that climate will remain constant". Do the authors have any indication on how much emissions will be affected under climate change?

Predicting future levels of emissions is currently an active research area. Future levels of emissions are quite uncertain, especially given the social-science and economic aspects of the problem. Adding to the uncertainty are the changes in biogenic emissions and, indirectly, anthropogenic emissions (due to changing behavior patterns).

3. Page 6491 Line 27: "Ten aerosol size sections were used711; from 40nm to 40um". The authors do not provide any indication how much the climatic parameters examined here will affect aerosol size distribution. I think that it is important to point out the possible change in aerosol size distribution.

We decided to focus on PM2.5 concentration because of its policy relevance in the US. Changes in the predicted mass size distribution are rather small in all of these tests given that diameter is proportional to mass to the 1/3 power. The changes in ultrafine particle number concentration are a different issue. Unfortunately such changes are not well captured by PMCAMx, because it is a model focusing on particle mass not number.

4. Page 6492 Line 10: The authors use 7 days simulation data after a spin-up period of

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3 days. Is the 3 day period enough to wash out possible errors in the initial conditions etc or has this period been arbitrarily chosen? Please elaborate. Is the 7 days period enough to extract representative results? How would the results be affected if the output data used were on a monthly or seasonal basis? Also it would be useful to present daily average values and explain how these differ from the 7 day average results presented here.

The spin of PMCAMx and CMAQ in this domain and for similar periods has been investigated by previous studies and three days were found to be sufficient for the start-up. References to these studies have been added. One could easily use longer simulation periods, focus on individual days, etc., however many of the basic phenomena observed (effect of temperature on sulfate, effect of humidity on nitrate, etc.) generally occur regardless of the choice or length of time periods modeled. This paper aims to elucidate the key processes that are responsible for the links between meteorology and PM2.5; these key processes are not very dependent on the specific time period modeled, even though the calculated sensitivities are. This is now noted in the conclusion.

5. Page 6492 Line 13: “In the vertical direction” It is not clear to me why the authors chose to use different number of vertical layers and different altitude during January and July. How different would the results be if the same number of layers and the same altitude were used for both modeling periods? How the authors have defined the vertical layers?

Different resolutions were necessary for the better meteorological simulation of the two periods. The additional January layers were above the July layers, so there was simply a larger domain in January, rather than a more highly resolved one.

6. Page 6492 Line 16: PM2.5 concentrations are strongly affected by the emissions. Please provide a more detailed description (e.g., tons per day) of the emission inventory used.

Two references have been added that describe this emissions inventory in some detail.

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7. Page 6492 Line 20: “PMCAMx performance has been evaluated711; and area”. I would prefer the authors to give more details for the model evaluation and not a general statement “was found to vary from fair to excellent”. How do the “base case” results compare with observations?

More detail about model performance has been added. Also, it has been added that the periods over which the model was evaluated in the previous studies are the same periods used as the base case in this study.

8. Page 6493 Line 17-22: Please provide references for the fixed concentrations of each PM2.5 species used as boundary conditions. How do the fixed concentrations affect the modeling results presented in this study? How would the sensitivity results differ if a multi-nesting approach was adopted?

A sentence and references have been added explaining that we are assuming that there no changes in the long range transport of pollution into the domain. This topic will be discussed in future work in which PMCAMx is linked to a global climate model / chemical transport model.

9. Pages 6499-6500, Section 3.6: Please explain why changes in cloudy area affect PM2.5 species concentrations.

A sentence describing this has been added.

10. Page 6501 Line 23: “changes in area of precipitation” How do the authors define the precipitation area?

A definition has been added.

11. Page 6503 Line 10: “It may be valid to represent a combined change as individual sensitivities”. We can’t combine changes caused by different meteorological parameters. The results obtained by such combinations have no practical value or scientific basis and could be largely misleading, as it is not valid to present a combined change in many meteorological variables as the sum of individual meteorological changes (see

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general comments).

This has been reworded to be more straightforward and less open to misunderstanding. We are simply suggesting that the two methods may give roughly equivalent answers. In other words, not that there are no interactions, just that the interactions may possibly be minor compared to the direct effects.

12. *Page 6503 Line 18: “The sensitivities were multiplied by the potential meteorological changes” Are the sensitivity results presented here linear to the change of each meteorological parameter to do that?*

Each of the sensitivities was roughly linear, and the linear sensitivities were calculated using the two extremes of the simulations for a meteorological variable.

13. *As Dawson et al (2007) have presented sensitivity of ozone using the same framework, a brief paragraph comparing the outputs of both works would be useful.*

This would be rather challenging, given the differences in key meteorological parameters, precursors, seasonality, and units. We believe that discussing the ozone results may prove confusing and that simply referencing the ozone paper is a better solution.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 7, 6487, 2007.

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