

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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Main criticisms:

1. *In the conclusions section, the authors need to do more to emphasize the paper’s limitations. The authors looked only at the 10-day mean concentrations over 2 periods in January and July, which may or may not have included pollution episodes. The best approach to evaluating the impact of climate change would be to look not just at mean changes, but also at changes at the extremes, when PM_{2.5} levels are high.*

These limitations are now addressed in the conclusions. Looking at extreme / daily average values would be very interesting (especially with the new daily PM standard), but it is outside the scope of this work.

2. *In the eastern United States, extreme events often correspond to periods of stag-*

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nation. What do the sensitivities calculated here suggest will occur if the frequency of stagnation occurs with climate change?

Though stagnation is not directly addressed in the paper, it could conceivably be thought of as changes in several individual variables, such as increased temperature and decreased wind speed. In the summer, these would likely lead to increased PM_{2.5}, given the results of this study.

3. *Another limitation of the study is that it does not take into account the effect of climate change on biogenic emissions or emissions of ammonia. How much does this limitation matter?*

It was decided not to include changes in climate-sensitive emissions in this analysis, since these are in the gray area between climate effects and emission effects. This limitation is mentioned in Section 2. Effects of changes in biogenics will be examined in future work.

4. *The paper starts out with a strong introduction about the general effects of meteorological variables on PM_{2.5}. But I would like more discussion in the body of the paper as to whether the calculated sensitivities agree with observed sensitivities. I realize that such observations are not exactly abundant, but the paper seems too distant from observations.*

A few comparisons to other studies have been added, though, as the referee concedes, there are few observations with which to compare.

Minor criticisms:

5. *Figures 1 and 2 show the calculated total PM_{2.5} concentrations for the Eastern United States. How do these values compare to observations? If observed values are available, it would be good to include a plot of these as well.*

Comparisons to observations are made in the referenced model evaluation papers, and this is now noted in the text.

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6. *Section 3.1. The section on the temperature effects on PM_{2.5} is muddled. I suggest the authors rewrite this, starting with the separate aerosol types, and then going on to total PM_{2.5}.*

This section has been reorganized for clarity.

7. *Section 4 and elsewhere. Here the authors sum up the changes calculated by perturbing the meteorological variables one-by-one and compare the resulting PM_{2.5} concentrations to those obtained with a simulation performed with all the variables perturbed simultaneously. For both the January and the July cases, summing the changes leads to 25 percent smaller concentrations than those calculated with the combined meteorological perturbations. The authors then go on to suggest that a simple 25 percent adjustment may be all that is necessary to compensate for the differences between the two approaches. I disagree. In these two cases, 25 percent turned out to be the bias. Using a different set of perturbations, the bias may have turned out differently. My point is not that the sensitivities should not be summed, but that pinning down the value of the bias is not so easy.*

The reviewer makes a valid point. This claim has been deleted and we just mention the relatively small difference between the results of the two cases.

8. *Figures 4 and 7. I would just show the changes in total PM_{2.5}, and say that the changes are dominated by changes in nitrate.*

These changes have now been made.

9. *Figure 5. I would also like to see a panel showing the changes in secondary organic aerosol.*

This figure aims to show how the change in total PM_{2.5} is primarily the result of the changes in nitrate and sulfate. We are concerned that adding an SOA panel may distract from this primary message.

10. *Figure 13. The caption should make clear what each point represents.*

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This change has now been made.

11. *A final point. I think some readers may feel uneasy about the value of a sensitivity study such as this one, where the meteorological values are changed independently of each other. But I think that is exactly where the value of such a study lies: it allows us to see the relative importance of the different meteorological variables. As more model studies and more observations become available, we can turn to this paper in an effort to understand the calculated or observed PM_{2.5} sensitivities to meteorology. I think the authors need to emphasize this value of their paper, as a kind of reference work for future studies.*

We do agree with this point. This is now mentioned in the “Model description and methods” section, introduction, and abstract.

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

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