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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #3

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Review of "Sensitivity of PM2.5 to climate in the Eastern U.S.: a modeling case study," by J.P. Dawson et al.

In this paper the authors examine the sensitivity of calculated surface particulate matter (PM2.5) to a range of meteorological variables over the eastern United States. They find that the sensitivity varied by PM2.5 type, with perturbations in temperature, mixing depth, wind speed, absolute humidity, and precipitation having the greatest effects. The work is straightforward, with no big surprises, but makes clear how changing meteorology may play a role in air quality in coming decades. The authors show that summing up the calculated sensitivities yields PM2.5 concentrations that approximate the values obtained when all the meteorological variables are changed simultaneously.

I have rated the paper as "good" and recommend that it be published once the following criticisms are addressed.

Main criticisms:

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 PM2.5 levels are high.

In the eastern United States, extreme events often correspond to periods of stagnation. What do the sensitivities calculated here suggest will occur if the frequency of stagnation occurs with climate change?

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?

The paper starts out with a strong introduction about the general effects of meteorological variables on PM2.5. 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.

Minor criticisms:

Figures 1 and 2 show the calculated total PM2.5 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.

Section 3.1. The section on the temperature effects on PM2.5 is muddled. I suggest the authors rewrite this, starting with the separate aerosol types, and then going on to total PM2.5.

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Section 4 and elsewhere. Here the authors sum up the changes calculated by perturbing the meteorological variables one-by-one and compare the resulting PM2.5 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% smaller concentrations than those calculated with the combined meteorological perturbations. The authors then go on to suggest that a simple 25% adjustment may be all that is necessary to compensate for the differences between the two approaches. I disagree. In these two cases, 25% 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.

Figures 4 and 7. I would just show the changes in total PM2.5, and say that the changes are dominated by changes in nitrate.

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

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

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 PM2.5 sensitivities to meteorology. I think the authors need to emphasize this value of their paper, as a kind of reference work for future studies.

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Interactive Comment