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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of 2000–2050 climate change on fine particulate matter ($PM_{2.5}$) air quality inferred from a multi-model analysis of meteorological modes" *by* A. P. K. Tai et al.

Anonymous Referee #1

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Review of "Impact of 2000-2050 climate change on fine particulate matter (PM2.5) air quality inferred from a multi-model analysis of meteorological modes" by Amos Tai et al., ACP, 2012.

In this paper, the researchers analyze observations of meteorology and particulate matter concentrations across the United States, to derive a sensitivity of annual average PM concentrations to the average synoptic period. This historical sensitivity is combined with an estimate of future-climate synoptic periods (from an ensemble of GCMs) to produce an estimate of how changes in climate will impact PM concentrations in the US. The method builds on earlier work from these authors, relating PM

to meteorological modes and stagnation under present-day climates. This is a logical extension of their previous work. This paper provides a very interesting analysis that suggests that climate change will probably not have a large impact on annual average PM concentrations in the US. The paper is clear and well written; the conclusions mostly seem well supported. In this reviewer's opinion, this paper should be accepted with minor revisions; these revisions should reflect some of the limitations of this study and somewhat more awareness of other groups' related research.

Major comments 1. This analysis assumes that dPM/dT will not change under future conditions. The authors should discuss this assumption and its implications. Given the work showing that particle composition will likely change under future conditions (e.g., Avise et al., 2009; Tagaris et al., 2007, among others), it seems plausible that this will affect dPM/dT. While exploring this topic in detail would be outside the scope of this analysis, this is something that should be mentioned somewhere in the paper (and could be an interesting idea for future work).

2. There appears to be a large amount of disagreement among the GCMs in Figure 7 for the Pacific NW. Given this disagreement and the very large 95% confidence interval for this region, saying "a likely decrease of $\sim$0.3 μg m-3 in the northwestern US" in the abstract may be overstating the degree of confidence. Similarly, in Figure 7, the weighting approaches appear to give a very large amount of weight to a very small number of models in the Interior NW. Given these difficulties, the authors should reconsider including this result in the abstract.

3. In Section 4, the authors consider this work in light of some other studies that have looked at the links between PM and other aspects of meteorology. The authors should consider comparing to findings from other research groups, for example, Avise et al., 2009; Tagaris et al., 2007; Jacobson et al., 2009.

Minor comments 1. Figure 5 appears to be referred to in the paper before Figure 4.

2. In Figure 7, the weighting approaches appear to give a very large amount of weight

to a very small number of models in the Interior NW and Interior SW. For example, only a very small number of models are below the mean in the Interior NW, and only a small number are above the mean in the Interior SW. This suggests that GCMs struggle in these parts of the country; why should the reader put stock in these results? This may merit some discussion.

3. At the end of Section 4, the authors state that "climate change will unlikely represent any significant penalty or benefit for air quality managers toward the achievement of PM2.5 air quality goals" since the combined effects would likely not be any more than 0.5 μg m-3. This reviewer would suggest that the threshold for "significant penalty or benefit" is considerably less than 0.5 μg m-3. For example, 0.5 μg m-3 represents 3.3% of the current annual standard of 15 μg m-3. The standard could conceivably be tightened to as low as 12 μg m-3 in the near future; 0.5 μg m-3 would represent 4.2% of this standard. While 3.3% or 4.2% is small, "insignificant" is probably too strong a word. For example, if this study were looking at ozone and found a maximum climate impact on ozone of 3.3% to 4.2% of the standard, this would mean something like 2.5 to 3.1 ppb, which is small, but probably not insignificant. This reviewer would suggest that this statement be reworded.

4. Very minor point. Are there any thoughts on episodic, rather than annual average, measures of PM? Again, this is clearly outside the scope of this analysis, but the authors should at least think about if these results shed any light on episodic concerns and consider adding something to the conclusions if there are insights they come up with. (This could also be a consideration for future research.)

References Jacobson, M.Z., 2009. On the causal link between carbon dioxide and air pollution mortality, Geophysical Research Letters, 35, L03809, doi:10.1029/2007GL031101.

Tagaris, E., et al., 2007. Impacts of global climate change and emissions on regional ozone and fine particulate matter concentrations over the United States, Journal of

Geophysical Research, 112, D14312, doi:10.1029/2006JD008262.

Avise, J., et al., 2009. Attribution of projected changes in summertime US ozone and PM2.5 concentrations to global changes, Atmospheric Chemistry and Physics, 9, 1111–1124.

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