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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Synoptic meteorological modes of variability for fine particulate matter ($PM_{2.5}$) air quality in major metropolitan regions of China" *by* Danny M. Leung et al.

**Anonymous Referee #2**

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The manuscript presents an interesting study on the associations between PM2.5 pollution and meteorology over China, as well as projections of climate-induced impacts on PM2.5 pollution by 2050. This is a well-written manuscript that build on years of work and prior studies. It presents a significant scientific contribution from a methodological perspective and provides insight on the meteorological drivers of PM2.5 pollution over China. I recommend publication after the comments described below are addressed:

- The study mainly consists of 3 analyses: Correlations between PM2.5 and meteorological variables; (2) PCA/PCR for dominant modes for PM2.5 variability; (3) a regression model for climate change impact on PM2.5. The three analyses could be better



connected between them. While conclusions are drawn from each, I see do not see shared findings and limited connections among them. For example, the correlation analysis shows the strongest correlations between PM2.5 and temperature. However, temperature is not one of the predictors considered in the regression model used in Section 5. I assume this may be due to the correlation between temperature and relative humidity, and the prescence of temperature in the PC used, however this is not discussed. I believe an improved description of the motivation for each approach, as well as what unique and shared information can be drawn from them would be beneficial.

- The authors' analysis of climate change impacts on PM2.5 is unconvincing. The multimodel ensemble reflects a very high uncertainty in projections of relative humidity and frequency of springtime among the CMIP5 simulations. A regression model based only 2 predictors with a R2=0.31, which projects a very small change in PM2.5 (-0.13$\pm$2.10) for which even the sign of change is highly uncertain, is used to draw the conclusion that there will be a more likely than not decrease in PM2.5 pollution due to climate change in the region. I also found the description and results interpretation of the Monte Carlo analysis to be incomplete. Given the limitations of the modeling approach and the high uncertainties encountered, I felt the analysis described in section 5 does not truly suggest a climate benefit for PM2.5 in the BTH region, but rather demonstrates that the evidence of a climate-induced impact is inconclusive. This would agree with other global and US studies that have shown how challenging it may be to robustly project a climate-induce change on regional air quality by midcentury under natural variability and the large uncertainties in climate projections.

- Although the manuscript is well-written, the introduction includes some odd wording and grammatical mistakes. I recommend a careful review of this section by a native English speaker.

- Page 2, line 3-4: "attributed" is used incorrectly

- Page 2, line 3-4: “attribute” is used incorrectly

- The 2nd paragraph in the introduction should probably be combined with the first. As is, the second paragraph seems repetitive and oddly placed.

- Page 5, line 2: 1497 monitors seems like a large number of monitors; are all plotted in figure 1?

- Page 5, line 19-22: The description of how the AOD-based PM2.5 concentration fields are derived is unclear (e.g. what model simulation?). Improve this description.

- Section 3 and figure 2. The authors mention that PM2.5 sites in much of southwestern China are relatively sparse and these regions are excluded from the analysis. However, it seems that in correlation analysis in section 3, the entire country is considered. Figure 2 does not indicate a difference between grid cells that were excluded or those that are included but have correlation coefficients near 0. I would recommend clearing indicating cells in excluded regions (e.g. coloring them gray) and not drawing any conclusions from those locations.

- Section 3 and figure 2: Some of the correlations between PM2.5 and meteorological variables reported and mapped are very small, yet the authors still draw conclusions above how some of these may drive PM2.5 concentrations. For example, the correlation coefficients for precipitation, pressure tendency and windspeed tend to be below 0.2. Is it still appropriate to draw about the interactions between these variables and PM2.5 concentrations if the explain <5% of the variance?

- Following the previous comment, for example, why would the correlation between precipitation and PM2.5 be positive over parts of central and western China?

- Figure 2: Remove the statistically insignificant vectors from panel (g).

- Page 6, line 35: I am not sure I clearly see the 2 divergent wind patterns on the map, and I am not sure the author’s conclusion that “wind transports pollutant from source regions to the peripheries” is substantiated. Which are the sources in these 2



locations?

- Section 5: The regression model explains about 30% of the variance in annual PM2.5 in the BTH region. Is this correlation string enough to draw conclusions about the climate “benefit” under the RCP scenario? I recommend discussing how this meteorology-driven climate impact is expected to compare with other drivers of PM2.5 change along this emissions pathway.

- Page 11, line 33: If the correlation with RH is statistically insignificant, do not report the value.

- Page 23, line 24: Change Monta to Monte

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