

## ***Interactive comment on “Simulation of the interannual variations of aerosols in China: role of variations in meteorological parameters” by Q. Mu and H. Liao***

**Anonymous Referee #2**

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This study simulates the surface PM<sub>2.5</sub> (and its major compositions) concentrations in China during 2004–2012, with the goal of understanding the processes affecting its interannual variability (IAV). The authors showed, through model sensitivity experiments, that regional wind and precipitation were the principle factors driving the IAV of surface PM<sub>2.5</sub> concentrations.

In my opinion, this is an important topic that will help us understand the relationship between meteorology/climate and air quality. The topic is of great interest to the community and suitable for publication in ACP.

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However, there are some important flaws in the current manuscript, which would require major revision before it can be published in ACP. The most critical flaw in this manuscript is the lack of discussion of the implications from the results of the sensitivity experiments. This greatly diminishes the value of the paper. Also, many details of the experiments were not clearly described. I would suggest reducing the too-lengthy reporting of numbers and instead focus on the implication of the results.

Specific comments:

P11183 Line 3: ‘... in which anthropogenic emissions ...’: This wording is confusing. Did Park et al. (2004) used the Streets 2006 emission inventory over Asia? Or was the Streets inventory used to overwrite the emission inventory of Park et al. (2004) over Asia in this work? Please clarify.

P11183, Line 3–6: David Streets’ 2006 emission inventory should be cited as Zhang et al. (2009)

P11184, Line 4–7: Were natural emissions turned off completely? Or were the natural emissions kept at levels without interannual variability (this seems like the better approach for the authors’ purpose)? Please clarify.

P11184, Line 14–21: This paragraph is confusing. Please consider revising to make the significance of the experiments clearer.

Section 3.2: The report of MAD and APDM values in this section is somewhat tedious and confusing. I would suggest that, instead of reporting values, it would be more useful to simply refer to Table 2+3 and Fig 3+4 and then point out the implication of these values. Also, please avoid non-meaningful sentences. E.g., (line 22) “.. MAD or APDM of OC was similar to that of BC...” and (lines 25–26) “The APDM values of BC were about the same as those of OC”.

P11188, Lines 10–13: The high correlation between observed and simulated AOD over Beijing is many driven by the model’s ability in reproducing the annual cycle. Looking

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at Fig 5, the high correlation does not indicate the model's ability at reproducing the observed IAV in NC, as the authors postulated.

Section 3.3: Why is Changsha chosen to represent Southern China?

Section 3.3: The authors show that AOD, column PM2.5 burden, and surface PM2.5 concentration in NC (Beijing) are all higher in summer and lower in winter. This is somewhat surprising, as it seems to contradict surface PM2.5 measurements (e.g., Zhang X. Y. et al. (2012)). What is the cause of this discrepancy? Did the authors plot the simulated wet or dry PM2.5 concentrations in Fig. 5?

Section 3.3: What is the cause of the bi-modal feature in the annual AOD cycle over SC (Changsha)? This same feature is seen in the surface PM2.5 many SC sites in Zhang X. Y. et al. (2012). The model was unable to reproduce this feature, which the authors attributed to topographical complexity and cloud contamination of satellite AOD retrieval near Chengdu. However, the fact that several SC sites show the same surface PM2.5 annual cycle is indicative of a more regional mechanism at work.

Section 4.1: Again, instead of reporting values, I would suggest that the authors discuss the implications of the IAV of various meteorological parameters. Also, I do not think it makes any sense to compare with Piao et al. (2003) or Qian and Lin (2005). The IAV of meteorological variables for different periods do not necessarily have to be the same.

Section 4.2 and Figs 9-14: This section is not well explained. How were these budgets constructed? Are the budgets presented in Figs 9-14 based on the standard simulation ANNmet? If so, then this should be clearly indicated in the text and in the caption. Or did the authors conduct multiple sensitivity tests to isolate the contribution of each of the processes shown in Figs 9-14? If the latter, then the authors should better describe the sensitivity experiments conducted in the Methods section.

Throughout: What is the value of showing both the MAD and APDM in all analyses? If

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there is value in showing both, the authors should better explain the implication of the differences between MAD and APDM.

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