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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Retrospective analysis of 2015–2017 winter-time $PM_{2.5}$ in China: response to emission regulations and the role of meteorology" *by* Dan Chen et al.

Anonymous Referee #1

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This manuscript presents an interesting study that utilizes the GSI-WRF/Chem 3D variational data assimilation system to better simulate the surface PM2.5 concentrations in China for the January months 2015-2017. It shows that WRF-Chem PM2.5 simulations with assimilation of surface measurements significantly reduced the model biases and better captured the inter-annual variability of surface PM2.5 levels in January 2015-2017. The model improvements are independently evaluated with MODIS and AERONET aerosol optical depth (AOD) measurements. Comparisons of model PM2.5 simulations with and without data assimilation indicate the effectiveness of the emission control measures, as well as the unfavorable meteorological conditions in January 2017 that led to PM2.5 increases relative to January 2016.

Overall, I think this is a nice study that illustrates the strength of data assimilation method to constrain PM2.5 changes, and further diagnoses contributions from both emissions and meteorological conditions. The method of this study is solid, and the language is generally appropriate. I recommend publish after the following comments being addressed.

Specific Comments:
(1) Page 7, Line 11:
"The spatial distributions of primary PM2.5 emission are shown in Fig. 1". Here Fig 1 should be Fig 2. Does primary PM2.5 correspond to BC, OC, and OIN in the WRF-Chem model? Since PM2.5 is also produced secondary in the air, it should be useful to show its precursor emissions, such as NOx or SO2.

(2) Page 10, Line 5-8 about the data quality filter:
The study states that PM2.5 observational values larger than 500 μg m$-$3 were deemed unrealistic and observations leading to deviations exceeding 120 μg m$-$3 were also omitted. It is not clear to me how these thresholds would impact the results and the conclusions of this study. What are the fractions of data that were omitted by the filters? In winter, some cases can meet the thresholds and can be realistic. So what would happen if a looser filter was applied. Please add some discussions.

(3) Page 14, Section 3.1:
It shall be valuable to add a table in this section, similar to current Table 4, but summarizing the mean observed vs. simulated PM2.5 concentrations over the 8 regions defined in Figure 3. The readers can then have a more quantitative picture on how effective the data assimilation system is.

(4) Page 15, Section 3.2:
The use of MODIS AOD data was only for support of the AOD decreases over the

Sichuan Basin and Central China after data assimilation. This seems to be insufficient. How about the inter-annual variability of MODIS AOD over January 2015-2017? Are they consistent with surface PM2.5 measurements? Please clarify.

(5) Page 18, Line 23:
In the statement “meteorological conditions might be totally different from 2016 to 2017”, “totally” is a very strong word, however, it is not clear how different 2017 meteorological conditions are different from normal wintertime conditions with Siberian Highs. I have the same comment for Page 20, Line 13, Line 19, the word “totally” is not helpful. I suggest use more quantitative statements, for example, higher temperature by how much?

(6) Page 20, Line 15:
What does “higher RH (thus more reactions)” mean? How higher RH lead to more chemical reactions? Please clarify.

(7) Captions of Figure 7 and Figure 9: Please indicate here that the comparisons are for the January month.

(8) Some technical corrections:
Page 2, Line 8 - “modeled PM2.5 are” should be “modeled PM2.5 concentrations are”
Page13, Line 7 - “reflect combining effects” should be “reflect combined effects“
Page 14, Line 11 - “the 2010 EI” should be “the 2010 emissions“
Page 19, Line 17 - “the emission in . . .” should be “the emissions in . . .”

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