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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling the feedback between aerosol and meteorological variables in the atmospheric boundary layer during a severe fog-haze event over the North China Plain" *by* Y. Gao et al.

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We thank the reviewer for helpful and constructive comments.

The Gao et al. article presents results from the WRF-Chem model run for a particularly severe, regional-scale air pollution episode over the North China Plain in January 2013. The authors compare two simulations from the 'on-line' aerosol/chemistry model: one allowing for feedbacks between the aerosols and chemical fields on the meteorological fields and a second simulation where these interactions are not allowed. There is a

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growing body of research investigating the effects of aerosols on the shorter term evolution of the atmosphere and this research adds to that by investigating the influence of aerosol feedbacks on near-surface aerosol concentrations through modifications to the meteorology during this particularly intense pollution episode. While the methodology and conclusions of the study are logical and well-founded, the one facet of the study that does appear to be weak is the assessment of the model predictions of aerosol amounts and related quantities such as aerosol optical depth against observations. Surface concentrations of PM2.5 are compared against observations for four stations, though all of these stations are in the vicinity of Beijing and are removed from the region with the highest aerosol amounts, judging from the spatial distributions presented in Figure 7. Additional comparisons are shown for aerosol optical depth (AOD) from sunphotometer measurements and point measurements of AOD from MODIS for most of these same stations around Beijing. The only observations further away from the region of Beijing are visibility measurements at Shijiazhuang and Baoding. Certainly nothing can be done about sparse observations, but looking at the observations presented in Che et al. (Atmos. Chem. Phys., 14, 2125-2138, 2014) for this same pollution episode there are sunphotometer measurements of AOD at Huimin, about 300 km south of Beijing and within the BTH region used for spatial averaging of quantities presented in several of the figures. While Wang et al. (Atmos. Envir., 89, 807-815, 2014) present maps of MODIS AOD for this same period, which would help extend the spatial coverage of the comparison of model output with observations. Additionally, Wang et al. use CALIPSO cross-sections of extinction to show that much of the aerosol is within 1 km of the surface for this episode. The vertical extent of the aerosols would be an important quantity to assess for the modeling presented here. The modeling results and analysis of the 'positive feedbacks' of aerosols on the aerosol concentrations near the surface for this globally significant region are fascinating, however the lack of a thorough comparison with observations makes it difficult to assess how accurate the estimate of the magnitude of these effects are. I will note that the comparisons for Xianghe and Tianjin, the two stations furthest to the south of Beijing with PM2.5

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observations both show substantial over-estimations of PM2.5 concentration on the order of 30 to 60%. How the overestimations at Xianghe and Tianjin may be related to the model estimations of larger aerosol effects further to the south and west is an open question. A comprehensive (as possible) comparison of model results with different observations would help to strengthen the quantitative estimates of the effects of aerosols on the meteorology during this episode.

Reply:

We thank the reviewer for this constructive comment.

The evaluation of model results is very important. Now we make the revisions as follows according to the reviewer's comments:

First, following the reviewer's comment, now we cite the observation data of AOD at Huimin site in Che et al. (2014) and compare the corresponding model results from the EXP-CTL case with it. We add the comparison in Figure 5 and Table 1. The comparison shows that model can reproduce the variation of AOD at Huimin site with the correlation coefficients of 0.74 but may underestimate it with the mean bias of -0.22. We also add the following revision or discussion to the manuscript.

Page 1102 Line 8: "We also use the AOD data at Huimin site (37.48°N, 117.53°E, 11.7 m.s.l.) from Che et al. (2014) which is collected from the China Aerosol Remote Sensing Network (CARSNET) and were processed using the ASTPwin software offered by Cimel Ltd. Co (Che et al., 2009a). This site is denoted as a cross in Fig.1."

Page 1106 Line 3: "the R values between the simulated and observed AODs from AERONET are 0.59, 0.99, 0.60 and 0.74 for the BJ, XL, XH and Huimin sites, respectively. The AOD from MODIS is much higher than that from AERONET, indicating that the AOD may be overestimated by MODIS. The modeled averaged AOD is higher at XH and lower at XL, consistent with the surface PM2.5 concentration at these two sites (Fig. 3). Although model underestimates AOD at Huimin site with the MB of -0.22, both

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observation and model result show that the AOD at Huimin site is highest among the four sites. Considering the location of Huimin site in Figure 1, this indicates that model can reproduce the more serious pollution to the south of Beijing province."

Secondly, following the reviewer's comment, now we compare the daily distribution of AOD at 550nm at 13:00 from MODIS retrievals and the corresponding model results from the EXP-CTL case from 6∼17 January 2013 over the North China Plain. The figures are presented in the supplement file and we add the following discussion.

"Figure S1 is the daily distribution of AOD at 550nm at 13:00 from MODIS observations and the corresponding model results from the EXP-CTL case from 6∼17 January 2013 over the North China Plain (NCP). In Fig. S1, comparing with MODIS retrievals, the model may underestimate AOD, although there is many missing data from MODIS retrievals. Seen in Fig. 2(a), MODIS tends to overestimates AOD comparing with AERONET. Some previous studies (Ge et al., 2010; Prasad and Singh, 2007, Diner et al., 2005; Li et al., 2009, Wu et al., 2014) also indicated that MODIS retrieved AOD significantly depended on both the aerosol type and the underlying surface type and had large uncertainties. Even though model results could reproduce the more pollutants over the south part of Hebei Province and the evolution of AOD values during the fog-haze period: the event starts from 10 January, when the AOD increases a lot over Beijing, Tianjin and Hebei province. After that, the value of AOD continues to increase during 11 January. Both model results and MODIS retrievals show the decrease of AOD on 12 January and the increase on 13 January. On 14 January, AOD begins to decrease over Beijing but maintains a high value over south of Hebei province."

Thirdly, following the reviewer's comment, now we compare the altitude-orbit cross-section of 532 nm extinction coefficient from CALIPSO and the modeled aerosol extinction coefficient at 532nm from EXP_CTL at 10∼15 January, present the figures in the supplement file and add the following discussion.

"The Cloud-Aerosol Lidar with Orthogonal Polarization (CALIOP) onboard the Cloud-

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Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO) provides aerosol vertically resolved extinction and depolarization ratio (Winker et al., 2007; Huang et al., 2007). CALIPSO 532 nm extinction coefficient (Winker et al., 2009) is used to evaluate model results. The horizontal resolution is 5 km and the vertical resolution is 60 m below and 180 m above 20 km, respectively. Figure S2 is the altitude-orbit cross-section of 532 nm aerosol extinction coefficient from CALIPSO and the modeled aerosol extinction coefficient at 532nm from EXP_CTL at 02:00 11 January, 13:00 11 January, 02:00 13 January, 13:00 13 January, 02:00 15 January and 13:00 15 January (local time). The model results are sampled at the time and location of CALIPSO orbit. The simulated aerosol extinction coefficient at 532nm is calculated by using the default output AOD and Angstrom exponent derived from the output AODs at 400nm and 600nm. As seen in Fig. S2, model can capture the vertical distribution and the evolution of aerosol extinction coefficient during the fog-haze period by comparing with CALIPASO retrievals. Both model results and CALIPSO retrievals show that the high value of extinction coefficient is near the surface below 1 km indicating that the particles mainly concentrate below 1 km. Model also reproduces the shift of pollutant from south (between $\sim$32°N and 34°N) to north (between 37°N and 40°N) from 11 January to 13 January."

Finally, for the overestimations of PM2.5 concentration at Xianghe and Tianjin mentioned by reviewer, now we add the following discussion to the conclusion part of the manuscript.

"There are uncertainties in this study. The model overestimates PM2.5 at Xianghe and Tianjin according to Figure 3. The diurnal variation of PM2.5 at Xianghe and Tianjin from model results and observations shows that the overestimation is basically at night which may relate to the setting of atmospheric boundary layer height at night in the model and also the bias of emission inputted to the model. As the aerosol feedback derived from the aerosol radiative effect mainly has large impacts during daytime, it is noted that the overestimation of PM2.5 may lead to slight overestimation of the aerosol

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feedback during the pollutant period. Emission with higher resolution may be useful for improving the model performance. The aerosol direct and indirect effect is also very sensitive to the mixing state between scattering aerosols and absorbing aerosols. Moreover, the feedback between aerosol and cloud/meteorological parameters also has large uncertainty. Although aerosol indirect forcing is considered in WRF-Chem, it only includes the aerosol effect on resolved stratiform clouds. With a horizontal resolution of 27 km, convective clouds are still parameterized in the model without explicit cloud microphysics in the convective cloud parameterization that links aerosols to cloud condensation or ice nuclei."

Other minor comments are given below.

Page 1096, Line 15: Consider rewording 'a weak weather system'. Perhaps 'weak synoptic-scale winds' or 'a synoptic scale stagnation'?

Reply:

We thank the reviewer for this helpful comment.

Now we revise "a weak weather system" to "a synoptic scale stagnation ".

Page 1098, Lines 10-17: Here it is stated the MADE/SORGAM is used in WRF-Chem and there are several references to the use of a modal aerosol description. On Page 1099, lines 24-28, it is stated that MOSAIC is used and that it is a sectional model with eight size bins. These two statements conflict and it is not clear which aerosol representation was used.

Reply:

We thank the reviewer for this helpful comment.

We use MOSAIC in this study. Now we delete " MADE/SORGAM (Modal Aerosol Dynamics Model for Europe and Secondary Organic Aerosol Model) and" in the manuscript.

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Page 1100, Lines 1-4: Are the MOZART boundary conditions specific to the period being studied or are they climatological? It should be made clear in the article. Note a spelling error on 'MOZART'.

Reply:

We thank the reviewer for this helpful comment.

The MOZART boundary condition are specific to the period being studied. Now we revise the sentence to "Both the initial and boundary chemical conditions are from MOZART's (Model for OZone and Related chemical Tracers) chemical boundary conditions which are specific to the period being studied.".

Page 1103, Line 24: In place of 'nowadays' I would suggest 'present-day'.

Reply:

We thank the reviewer for this helpful comment.

Now we revise "nowadays" to "present-day".

Page 1106, Line 15: Figure 6a presents the difference in the surface energy budget between the two simulations averaged over the BTH region. There is a lot of focus on the Jan 10-15 episode of extreme PM2.5 concentrations, but it is not at all apparent that Jan 10-15 was much different than the rest of the month. Do you have any ideas why the average over the BTH region does not show a significantly larger signal for Jan 10-15? From the magnitude of the differences plotted in Figure 6a, I assume these numbers are averaged over the full 24 hours each day. Does it make any difference if only the 09-18 LT times are used?

Reply:

We thank the reviewer for this helpful comment.

The reason is that the surface energy change is also very large on 19 and 21 January.

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Compared to other periods, e.g. 2∼9 January, 16∼18 January, 20 January and 22∼26 January, the energy change during 10∼15 January is much larger. From Figure 3, it can be seen that the aerosol concentration on 19 and 21 January was also very high and comparable to that during 10∼15 January. We have tried to use the 09∼18 LT for average, it almost did not make any difference which also indicated that it may relate to the higher aerosol concentration on specific date in January (e.g., 19 and 21 January) but not the diurnal change of aerosol concentration.

Page 1106, Line 17: The reference to 'net radiation (LH+LW+SW+SW)' is not quite accurate because not all of the terms included there are radiation. Perhaps 'net energy flux'?

Reply:

We thank the reviewer for this helpful comment.

Now we revise "net radiation" to "net energy flux" in the manuscript.

Page 1110, Lines 8-10: What does the statement 'The higher the surface PM2.5 concentration is, the greater the increase is in the surface PM2.5 concentration...' refer to? Is it the average differences between Beijing, Tianjin and Hebei, or the temporal behaviour of the differences for each station? If it is the temporal behaviour, the fact that at the time of some of the highest peak concentrations in the EXP_NOEF simulation there are lower PM2.5 concentrations in the EXP_CTL should be acknowledged. I believe this is just a result of the internal variability of the meteorology in the simulations and does not impact any of the conclusions of the paper. However the statement should be clarified.

Reply:

We thank the reviewer for this helpful comment.

"The higher the surface PM2.5 concentration is, the greater the increase is in the surface PM2.5 concentration..." refer to the average differences between Beijing,

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Tianjin and Hebei. Now we revise the sentence to " For the average differences between Beijing, Tianjin and Hebei, the higher the surface PM2.5 concentration is, the greater the increase is in the surface PM2.5 concentration produced by unfavorable meteorological conditions.".

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C1021/2015/acpd-15-C1021-2015-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 1093, 2015.