

Interactive comment on “Modeling study of PM_{2.5} pollutant transport across cities in China’s Jing-Jin-Ji region during a severe haze episode in December 2013” by C. Jiang et al.

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Anonymous Referee #1 Received and published: 26 February 2015 Review comment on “Modeling study of PM_{2.5} pollutant transport across cities in China’s Jing-Jin-Ji region during a severe haze episode in December 2013” by C. Jiang et al. General comments This study using chemistry transport model GRAPES_CUACE to simulate a typical haze episode in December 2013 over Eastern China. The results analysis is mainly focusing on the Jing-Jin-Ji region, the national capital region of the Northern China because of its importance of regional source and sink of particulate matter. The modeled wind fields and PM_{2.5} concentrations are compared against observed ones

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from weather and air quality stations. The model simulations are comparable to observations. The results are interesting and the study is helpful to get an insight into the regional haze/smog problem in China. I recommend its publication in ACPD after considering the following comments.

Major comments 1. The authors only mention the datasets used for meteorological initial and boundary conditions in Section 2.2. However, the chemical initial and boundary conditions are also needed for driving the regional chemistry transport model. It is necessary to clarify datasets for this purpose.

Response: The monthly mean values of all tracers from observation data are used for initialization at the very beginning of the model run. The initial values of all gases in RADM2 and aerosol concentrations are based on the 24 h forecast made by the previous day’s model run. This has been added in the manuscript.

2. Another regional high PM_{2.5} event occurred on 24-25 in the same month according to observational data shown in Fig.4. I am wondering why the authors did not analyze this episode even they already ran the model for the entire month. In my opinion the analysis include the both episodes certainly makes the study stronger.

Response: We have already finished the simulations of the whole 2013-2014 winter and chose one haze as the case study of the PM_{2.5} pollutant transportation in this paper. PM_{2.5} transport study of the whole 2013-2014 winter will be analyzed in next paper, which need more model evaluation including meteorology and PM_{2.5}.

3. In order to identify the transport contribution to PM_{2.5} levels in Beijing (PK), the authors estimate the horizontal advective fluxes of PM_{2.5} with a box covering PK. But it is hard to conclude that “the remaining 1230t could be attributed to local emissions” at Line 11 on Page 3757 because sinks (e.g. dry and wet deposition) and sources (e.g. emission and chemical transformation) are not involved the authors’ calculations. Therefore this method cannot quantitatively decouple the contribution of transport process from final results determined by all processes.

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Response: Sinks (e.g. dry and wet deposition), emissions and chemical transformations were all calculated in the model dynamic, physical and chemical processes in every model step, 1230t is the difference between the total increasing amount of suspended PM2.5 and transport amount from surroundings of BJ, which may be mainly caused by local reasons. "The remaining 1230t could be attributed to local emissions" is not exact and should be changed into "The remaining 1230t suspended in the atmosphere over BJ could be attributed to local effects".

4. Due to the aerodynamic effects of large scale topography on the regional wind field, it is not surprising to me the spatial distribution pattern of PM2.5 is clearly dependent on topography over the Eastern China. The authors may analyze topographic influences on the regional winds (patterns) and population and emission sources distributions due to the topographic features. It makes sense for regional emissions mitigation policies.

Response: The wind speed and wind direction close related with the topography in the North China Plain has important impacts on the distribution of haze and fog. The aim of this paper is to discuss the wind field pattern, the attribution of particles transportation from Hebei Province on the haze pollution level in Beijing during a severe haze episode. Local topographic certainly has important impacts on the meteorology fields including wind, this certainly may influence haze event and pollution level. But this is very complex and it is not possible to be discussed clearly as part content considering the length of this paper. Fu et al (2014) has ever discussed this in detail (Fu. et al., 2014). Anyway, we will pay much more attention on this in the following study on the whole winter of 2013-2014.

Fu, G. Q., Xu, W. Y., Yang, R. F., Li, J. B., and Zhao, C. S.: The distribution and trends of fog and haze in the North China Plain over the past 30 years. *Atmospheric Chemistry and Physics*, 14(21), 11949-11958, doi: 10.5194/acp-14-11949-2014, 2014.

Minor comments

1. Line 8 on Page 3747: "..., and it changes the climate on a regional ..." may be

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changed into ". . ., it also has climate change effect over a regional . . ."

Response: It is revised in the manuscript.

2. Line 15 on Page 3747: "the central-eastern China, is not one of China's . . ." should be "the central-eastern China, is not only one of China's . . ."

Response: It is revised in the manuscript.

3. Line 21-22 on Page 3747: ". . . to inform policy aimed at averting irreversible environmental . . ." might be ". . . to inform policy aimed at averting environmental degradation ..."

Response: It is revised in the manuscript.

4. Line 3-4 on Page 3749: ". . . as a unified chemistry model . . ." could be changed into ". . . as a unified chemistry module . . ."

Response: It is revised in the manuscript.

5. Line 8-9 on Page 3749: You may remove "with diameter ranges of . . . 20.48-40.96 μm " because the size bins have been defined in Gong's paper (Gong, 2003) you cited.

Response: It is revised in the manuscript.

6. Line 21-22 on Page 3750: "Simulated PM2.5 values were similar to the observed PM2.5 values . . ." can be changed into "The simulated PM2.5 concentrations are in good agreement with observations..."

Response: It is revised in the manuscript.

7. What does "weather phenomena" mean in the manuscript? (Line 11 and 18 on Page 3751, Line 19 on Page 3757, and in Figure 2 caption). Weather phenomena can be any weather conditions that may and may not be hazardous to human life and property according to my understanding.

Response: It should be "haze weather phenomena" and this has been revised in the

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manuscript.

8. Line 17-20 on Page 3751: For high simulated PM_{2.5} in the southeastern Shanxi Province, please provide more evidences or detail explanations of “overestimated emissions”.

Response: The simulated PM_{2.5} has been on reasonable level after using the 2010 inventory, this figure is redrawn and the results using 2010 inventory are used in the manuscript.

9. Line 8 on Page 3758: “... 80 hPa ...” should be “ ... 800 hPa ...”

Response: It is revised in the manuscript.

10. It is much better to develop a single figure to show the difference between mean observed and modeled visibility for 6-7 December 2013 instead of Figure 3 (a) and (b).

Response: Figure 3 has been redrawn.

11. Please improve the quality of Figure 4 because it is difficult to read numbers and legends with its normal size.

Response: Figure 4 has been redrawn.

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