

Interactive comment on “The 2013 severe haze over the southern Hebei, China: model evaluation, source apportionment, and policy implications” by L. T. Wang et al.

Anonymous Referee #3

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The manuscript performed comprehensive MM5-CMAQ source apportionment simulations (as a total 37 two-month cases) for two extensive most hazy months (Dec 2007 and Jan-Feb 2013) for Northern China Plan. The model performances were evaluated by observations from nearly 500 national sites in northern China. The contributions by each source sector in each source region are analyzed to identify the most influential contributors to the severe haze pollution in three most polluted cities in Hebei province. This work provides scientific information for policymaking on the air pollution control and emission mitigation. I would recommend publication of this work on ACP when the following concerns been considered in the manuscript revision.

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1. In general the manuscript was written in a quite smooth manner in terms of English. However, most of the figures are presented in multiple small panels and their content, captions, legends are hard to read. Moreover, it would be nice if the authors could consider shortening the manuscript to be more concise, which I believe will make the manuscript more interesting and easy to follow. For example, the model evolution section (section 3) can be considerably condensed by putting some of the information that shows the model in general performed well in to supplementary and more efforts focus on those specific features which indicate why or why not the model performs well.

2. The authors summarized 4 major factors that may lead to the underestimation of particulate matters during sever pollution episodes (meteorology, spatial location of emission, lack of dust scheme, weakness in treatment of aqueous/heterogeneous formation of secondary aerosol). Could the authors comment in the manuscript whether these factors will influence the source apportionment results, since Dec 2007 and Jan 2013 are sever haze months?

3. The magnitude differences between Dec 2007 and Jan 2013 are described in the manuscript in terms of the regional/sector source contributions. Could the authors further analyze or comment on the reasons why these two haze months are different? Is it solely attributed to the different metrological conditions and how? Or might the emissions during these two time periods be different regarding both magnitude and spatial distributions? What insight can be provided to the policy-making by comprising the regional/sector contributions of these two haze months?

Technical comments:

1. It might be better to cite *. Wang et al., 2012 and 2013 as “Wang et al., (2012a,b) or (2013a)”, to be consistent with other citations.

2. Some abbreviations appear without first spelling out the full names, e.g., MBs, RMSE, NMB etc.

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