

It is really a hot topic for assessing the relative importance of meteorological parameters and emission reduction measures on the PM_{2.5} reduction from 2013. There is a similar manuscript on ACPD “Dominant role of emission reduction in PM_{2.5} air quality improvement in Beijing during 2013-2017: a model-based decomposition analysis”. When compared with this one, different conclusions were drawn for the contribution of meteorological conditions to PM_{2.5} reduction in Beijing in from 2013 to 2017. However, this manuscript is far from the publishing criterion of ACP.

I suggested rejection of this paper as the following reasons:

To Referee 2:

Thanks so much for your general and detailed comments on our manuscript. We have fully revised this manuscript according to your general and detailed comments mentioned in this and the complementary file. Specifically, we really appreciate you mentioned a relevant paper, which also investigate the relative contribution of meteorological conditions and anthropogenic emissions to PM_{2.5} variations in Beijing from 2013 to 2017. We also noticed this paper and actually very glad to see that this research and our research revealed the same fact that anthropogenic emissions contributed majorly to PM_{2.5} variations in Beijing from 2013 to 2017 from different perspectives. Cheng et al. (2019)’s research employed more fine-scale emission-inventories to specifically quantify a diversity of emission sources to PM_{2.5} concentrations in Beijing from 2013 to 2017. Meanwhile, the major aim of this research is to use the statistical model KZ to filter the influence of meteorological variations and also use CTM model to verify the result from the KZ model. To better present our results, we have fully revised our manuscript to include more discussion of these relevant studies and recently released reports, highlight the unique contribute of Cheng et al. (2019) and our research, and conclude the combined theoretical and practical significance of these studies to air quality improvement in Beijing and other mega cities in China. As a result, we do believe a significantly improved version of our manuscript and Cheng et al. (2019)’s research can jointly contribute a lot to the ACP society.

Meanwhile, although the authors have already published many top journals including Science, The Lancet, PNAS and of course some ACP papers, clearly the figures, text and languages of this manuscript can be improved a lot, especially according to your comments. Thanks again for giving us suggestions to improve the presentation of this manuscript. We have carefully reproduced all these figures and polished the structure and language according to your comments. We are more than willing to conduct further revisions if additional requirements are given.

Thank again for reviewing our manuscript and your valuable comments indeed help us a lot.

(1) There are so many typesetting mistakes that I can not listed all of them. The authors could

find the attached manuscript that I have labeled. Some mistakes indicated that the authors are not serious for the scientific papers, such as the character subscript, the citation form of references. I am really confused why such kind of papers can be on the ACPD for open discussion.

R: Thanks a lot for your comment. Although we have already published several papers in ACP, clearly there is room for us to improve this manuscript. And the forthcoming of more qualified ACP paper do rely on more and more strict requirements on received manuscripts. We are very grateful that the Co-Editor and the reviewers rated high on this manuscript and accepted this manuscript for ACP discussions, and thus we can receive highly valuable comments from qualified and strict experts like you. Thanks again for all your comments in the revised manuscript. We have fully revised this manuscript according to your comments listed in your attached files.

(2) The figures are made by Excel and in so poor quality, especially for Figure 2, 3 and 4. I really have a suspicion that are the authors know the quality of figures for scientific papers, not only say for ACP.

R: Thanks so much for your comment. Actually, some figures are not produced by Excel and we are very sorry that you think these figures are in poor quality and do not like them. As mentioned above, since we have published many high-level papers and previous reviewers, including other three reviewers for this ACPD manuscript, do not question the quality of these figures, therefore we do not have a stricter standard for figure production. Thanks again for pointing this out. We reproduced all these figures and hope the reproduced figures can better fit your requirements. Please feel free to let us know if you have additional requirements for these figures and we are more than willing to reproduce them again fully according to your suggestions.

(3) For the whole manuscript, it is just like a primary data analysis report, no discussion and no verification of the results.

R: Thanks so much for this comment. According to your comments, we have fully revised the manuscript in the introduction, discussion and result part to highlight the practical meaning and the correlation between this research and relevant studies. Actually, model simulation for three sites has already been there in the previous manuscript. According to the suggestions of you and another reviewer, we added the verification of additional three sites. We are sorry that we did not make this clear and have added more explanation of the verification and potential simulation error to the revised manuscript. Thanks again for your valuable suggestions.

(4) I am quite disagree that at the last the authors wanted to assess the emission-reduction measures considering both PM_{2.5} and O₃. They should know even for the assessing PM_{2.5} reduction, there existed large uncertainty especially for emission inventory, for subsector sources and for chemical speciations. More scientific questions should be addressed for improving the simulation. It suggested that the authors may be not quite

sure about the research shortages on the emission inventory and its adoption on air quality modeling.

R: Thanks so much for pointing this out. We are sorry that we did not make it clear. We are not saying that we would like to use CTMs to assess the effects of emission-reduction measures on ozone and PM_{2.5} reduction. We understand a diversity of uncertainties related to CTM simulations. Actually, the negative, positive or inconsistent effects of emission-reduction measures on PM_{2.5} and ozone concentrations can be understood simply based on the observation data. According to our previous studies, we found ozone concentrations in Beijing were even enhanced (based on observed data) while specific emission-reduction measures for PM_{2.5} reduction were conducted. This fact is also proved by some relevant studies based on observation data that proved ozone concentrations were not consistently reduced during specific events (e.g. 2014 APEC meeting) with emission-reduction measures. That's the reason we mentioned that emission-reduction measures for PM_{2.5} concentrations may not effectively reduce ozone concentrations and emission-reduction measures should be balancedly considered for PM_{2.5} and ozone pollution. To avoid unnecessary confusions, we have fully revised the discussion part by including more details on the introduction of relevant studies and removing the discussion of ozone pollution management. Thanks again for your comment.

(5) At last, I strongly suggest the authors carefully read the similar paper on ACPD and find the wide gap between yours and that one. In the future, the manuscripts should be carefully prepared. When you want to submit it to a high quality journal, please write it in a form of paper, not a report. Please also give the research shortages in science, not just say what you do.

R: Thanks again for recommending this manuscript, which is a well-presented work and quantified the contribution of different sources to PM_{2.5} reduction from the model-simulated perspective. As we know, CTMs are affected by a diversity of uncertainties, including the variations in PM_{2.5}-meteorology interactions, emission inventories, incomplete descriptions of reaction mechanisms between precursors under heavy pollution episodes, difficulties in parameter setting for long-term running and so forth. In this case, the statistical model, KZ employed to filter meteorological influences based on observed time series of PM_{2.5} concentrations and meteorological conditions are affected by many less influencing facts to quantify the relative contribution of meteorological conditions and anthropogenic emissions. So, based on your detailed and valuable comments, and a careful study of relevant high-level publications, we do believe that we can properly revise this manuscript and improve its quality significantly. A significantly improved version of our manuscript and Cheng et al. (2019)'s research can jointly contribute a lot to a comprehensive understanding of anthropogenic and meteorological influences on PM_{2.5} reduction from 2013 to 2017. Thanks again for recommending this manuscript and all these valuable comments on our manuscript.