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Comment on acp-2022-304

Anonymous Referee #3

Referee comment on "Contributions of meteorology and anthropogenic emissions to the trends in winter $PM_{2.5}$ in eastern China 2013–2018" by Yanxing Wu et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-304-RC3, 2022

This work proposes a different method for the MLR analysis of PM2.5. Based on the new interpretation and the comparison with previous studies, the MLR results among different studies were found to be more consistent. In addition, the authors also pointed out that the relationship constrained by long-term data is more reliable. Overall, this is an interesting study and it provides some useful information for other researchers when choosing MLR for air quality trend analysis. However, more explanations, especially for the methodology, are still needed. Major revision is suggested and specific comments are listed as follows.

(1) Line 60, the resolution of the PRD emission inventory is three degrees, which is rather coarse.

(2) This work mainly focuses on the PRD, YRD and Jing-Jin-Ji regions. For YRD and Jing-Jin-Ji regions, the authors combined the MEIC and PKU emission inventories to do the analysis. While for PRD, they combined PKU and PRD-EI to do the scaling. MEIC and PRD-EI are different emission inventories and the methods that used to derive these two emission inventories should be not consistent. Based on the literatures, the MEIC emission inventory should have already covered the PRD region, why not also using the MEIC emission inventory to analyze the PRD region?

(3) Please label the scaling factor and Ei equations.

(4) Line 71, please use data or reference to support this assumption.

(5) The PRD scaling factor was calculated by the emission sum from 2006 to 2013, while the scaling factors for the other two regions were calculated by the emission sum from 2010 to 2013. Please explain why using different emission sum to derive the scaling

factors.

(6) The authors applied the nonlinear exponential fitting to retrieve the long-term PM2.5 concentration before 2013, because China began to release the air quality observation data since 2013 and it is unlikely to acquire long-term observation data in this nation before 2013. However, based on the figures in the supplemental material, some of the fittings are not acceptable for further analysis, such as BTH-RH (40, 60) and YRD-RH (90, 100). The authors need to analyze and discuss whether such errors can influence their conclusion.

(7) For the PM2.5 concentration retrieval, I suggest the authors use the data of 2014-2018 for the fitting and the 2013 data for the verification, this can help to verify whether the methods implemented by the authors are reliable or not.

(8) Please introduce about the data source of RH and visibility in section 2.2. Generally the locations of the meteorological stations are not the same with those of the air quality stations. Did the authors use the nearest matching to pair the data? If so, what is the mean distance between the meteorological station and air quality station?

(9) Line 120, combined with other studies and this work, we understand that the emission is the major factor that influences the PM2.5 trend when compared to the meteorological variables. However, in Chen et al. (2019), the meteorological factors can still account for 21% of the contribution, which is much larger than the values reported by the authors in Line 117. I do not think this is an 'agreement'.

(10) Lines 162-164, based on the analysis performed by the authors, if there exists any method that can compensate the shortcomings of the MLR and prognostic model?

(11) Lines 180-185, whether this means that previous studies that focused the ASI harbor relatively large uncertainty?

(12) Lines 166-169, I suggest the authors to provide some theoretical foundations to support this interpretation.