

Interactive comment on “Meteorological formation mechanism of regional transport in winter heavy air pollution events in the middle Yangtze River area, China” by Yongqing Bai et al.

Anonymous Referee #2

Received and published: 10 March 2021

Title: Meteorological formation mechanism of regional transport in winter heavy air pollution events in the middle Yangtze River area, China

The authors have carried out MV-EOF and EEOF analysis to select and understand the peak pollution episodes and corresponding pollution pathways and meteorological conditions during these events using data from 2015 to 2019 over the middle Yangtze River area in China. In addition, they have also carried out chemistry-climate model simulations to understand the same issue but for a typical event for corroboration.

Though I would like to appreciate the overall effort and the intention of the authors, I found it hard to follow the manuscript due to the following reasons.

C1

The weakest part of the paper is the data and methodology section where scant information is provided with regard to the data, its curation and analysis including the details about numerical simulations.

The whole paper depends on the analysis of PM_{2.5} pollution based on data from the China's National Ambient Air Quality Monitoring Network. Even basic information about this dataset is missing in the manuscript. For example, it is not clear whether this is a gridded data or station data? What is the temporal resolution? What is the spatial resolution of the chemistry-climate model simulations?

Since the objective of the paper is to study the peak pollution episodes during the five-year period during the winter/January, which mode of the EOF was used finally used to create the several figures in the results section?

Was the climatology of the five-year period removed during the analysis? If not, the first mode will show up the climatological mean as the dominant feature.

Line 184 to 186 states about some synthetic and correlation analysis including anomalies. Was this the base database used for further EOF analysis?

Though the authors use several datasets and tools from the surface, reanalysis, and model simulations, the lack of information above basic aspects does not allow me to be positive. As such, in the current form, this manuscript requires substantial revision in terms of its readability and usefulness for a wide range of audiences of this journal. Hence I recommend rejection of the manuscript.

Minor Comments 1. Line 62 to 66, The sentence may be shortened.

2. Line 73, in in is repeated

3. Line 76, What is meant by excessive anthropogenic emissions? Is there any specific emission relevant only for winter that does not exist during other periods?

4. Most sentences are excessively long to understand. An English correction may help

C2

improve the readability of the manuscript.

5. The authors need to explain where the study region is using a map. The area shown in the map is a huge region over China spread over tens of degrees across. It will be better to name the regions in a map for the reader's benefit.

6. Somehow, the periods of peak pollution are similar over the years (early and late part of January) with a bi/tri-weekly separation between them. Is there any specific reason for this?

7. In section 3, local conditions leading to high pollution are mentioned. Are these not meteorological conditions? Perhaps, it may be mentioned as local and regional or large-scale meteorology.

8. In line 215, there is a mention of the use of data from 31 urban monitoring stations. It will be better if a table is provided with all datasets used in the study with their source, frequency, and time periods.

9. In Figure 2b, how much of the variance is explained by the mode shown?

10. Selection of peak pollution events along with the time coefficient must be shown in figure 2 to identify the events.

11. Since the peak episodes are few in number, is it possible to show each of the episodes for their PM_{2.5} spatial patterns along with the circulation patterns (as sub-panels)? This will allow us to know whether the patterns are similar or dissimilar for each episode.

12. Use similar color bars and arrow lengths (Fig.3) so that comparison becomes easier.

13. Figure 4 corresponds to nationwide station data or reanalysis?

14. If showing from reanalysis, anomalies with respect the climatology will show a better pattern with slowing winds/lower or higher temperature over the large domain. It

C3

appears 4a corresponds to actual winds and 4b corresponds to anomalies in temp or are both anomalies.

15. In Fig. 8, it is seen that the topographic features are avoided to a large extent. However, will the 1000 Mb level correspond to the surface? If possible, the temperature below the surface should be avoided when showing such plots.

16. Figure 9, sounding profiles could be shown along with climatology or the difference with respect to climatology similar to Fig. 10. This will clearly show the features during the pollution episodes. This will also validate/provide confidence in the reanalysis in case of any bias.

17. Section 5 appears to me as an avoidable addition to the overall flow of the manuscript. Even removing this section may not affect the overall discussion of the paper.

18. Section 5.2 details about WRF-Chem could be included in the data section.

19. Figures 8 and 13 could have a similar latitudinal spread so that the simulation could be compared with reanalysis easily. The simulations don't compare with reanalysis according to this figure (perhaps, due to the different time periods, but could be checked with the exact period)

20. I find that the manuscript is most China-centric with no reference to the many important and interesting similar studies carried out elsewhere. This could be included in the future for completeness.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-708>, 2020.

C4