This paper by Yan et al. investigated the characteristics of winter haze episodes in Jingzhou of Central China under typical potential synoptic controls (PSCs) during November 2013-February 2014. Furthermore, they examined the contributions of local and transport of pollutants from surrounding regions to $PM_{2.5}$ under different PSCs by applying the GEOS-Chem model with a high resolution. This work also studied the effectiveness of different emission control strategies in Jingzhou, Central China, and other surrounding regions under different PSCs, and highlights the importance of collaborative actions for $PM_{2.5}$ mitigation under server haze pollution. In general, the study is well organized and worthy of publication. However, I have some specific comments that I feel deserve attention.

Major comments

1. The writing should be improved before publication.
2. The configuration of the model is vague. How many nested domains were applied in each simulation? What is the geographic coverage of each domain and the corresponding resolution? What are the emission inventories for each domain? A figure showing each nested domain is also highly recommended.
3. The circulation classification is the basis of all the analysis. Why did you choose the Lamb-Jenkension method? What are the advantages of this method compared to the ones used in other studies such as Chang

and Zhan, 2017, Dai et al., 2021, etc.?

4. The validation of model performances is very weak. The bias of the modeled $PM_{2.5}$ in Jingzhou can be as high as more than 100 μg/m^3, what are the possible reasons? The authors simply claimed the uncertainties in emissions, meteorology, and chemistry might cause this discrepancy without any details. What are the amount of the $PM_{2.5}$ precursors emitted in this study and how are the values compared to the published literature? How about the meteorological parameters used by the model vs. observations? The authors claimed an improvement in sulfate by the increase in primarily emitted sulfate in the model, how is that compared with observations? They also analyzed the changes in the chemical composition of $PM_{2.5}$ under different typical PSCs without examination of the model performances in the base case.

Minor comments:

Line 101-103: There must be many studies targeted the mitigation of $PM_{2.5}$ at a regional scale (Ding et al., 2019; Zhang et al., 2019, Xing et al., 2018, 2019; Fu et al., 2017; etc.). Please rephrase this sentence.

Line 148-150: It is very confusing. The circulation classification is based on the meteorological data from November 2013 to February 2014, which is also the simulation episode. Why did you use the hourly $PM_{2.5}$ data from 2013-2018?

Line 195: Did you do nested runs or just one domain covering China? Please make this clear.

Line 205: The SEEA inventory was developed for the year 2017. Did you use it directly without projection to the simulation episode? If you adjusted this inventory, what are the factors applied for the $PM_{2.5}$ precursors and how did you obtain those data?

Line 215-217: Have you compared the modeled sulfate with observations, at least in Jingzhou? How about the model performances of the other components of $PM_{2.5}$?

Line 305: Again, I am confused about the emissions used in the CON case. You listed too many options for the anthropogenic source in Table S2. What inventories were EXACTLY selected for the CON case? Did you do a global/regional nested run? Please explain the choices of emissions in a separate column in the table.

Line 310: Please compare the meteorological field used in the model with observations to confirm that statement. Also, there are no perfect mechanisms, inventories, or parameterization of the model with no doubt. I suggest using "uncertainties".

Line 323-324: A comparison of the modeled fractions of the inorganic salts to observations, or reported values from other literature if no measurements are available.

Line 324: “As shown in Table 3, ….”

Line 358: How was this calculated？ Please explain it.

Line 415-417: How about the contributions of transported pollutants to the chemical composition of $PM_{2.5}$ under the four PSCs?

Line 424: The base year of emission reduction is 2015 for the 13th Five-year plan, which is quite different from your inventory. How effective is the designed reduction ratio of the anthropogenic emissions in this study?

Line 425 and 428-429: Please explain these abbreviations in the text as well.

Line 437: I think an evaluation of the model performance in ammonium and/or ammonia is desired to confirm that.

Figure 6, 8, 9, 10: I suggest to show the fraction of each inorganic salt to $PM_{2.5}$ rather than their total mass.

Figure 11. It should be “TALL” in NW and C.

References:

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