

The manuscript entitled “A Comprehensive Observational Based Multiphase Chemical Model Analysis of the Sulfur Dioxide Oxidations in both Summer and Winter” by Song et al. presents the comprehensive evaluation of the contribution of different sulfate production pathways in both summer and winter by their self-developed multiphase box model (PKU-MARK). The model includes nearly all the established sulfate production pathways, providing valuable insights into the sulfate formation evaluation. Overall, I have some concerns that need the authors to clarify before that I can recommend publication in *Atmospheric Chemistry and Physics*.

1. The concentration of TMIs is vital in this work since the two dominant sulfate production pathways the authors proposed are aqTMI and Mn-surface. An online monitor measured the Fe and Cu concentrations. Due to the lack of Mn data, the authors propose a fixed ratio of Fe/Mn to stimulate the concentration of Mn. So how about the uncertainty of this method? It is better to compare the concentration of Mn with literature results in the same region.
2. The authors state that the average soluble percentage of Fe and Mn in winter polluted conditions was 0.79% and 19.83%. However, the water-soluble fraction of Fe and Mn may change a lot in different regions, as stated in the manuscript. Also, the solubility may change under clean conditions and polluted conditions. It is better to add some discussion about the sensitivity of the solubility of Fe and Mn to the model results.
3. The authors declare that their result is consistent with the result of the WRF-CHEM study. However, in the cited work, the ionic strength inhibition effect was not included. More discussion about the results is needed.
4. It is important that the model has considered the activity coefficient values and reactions about oxalate and Fe. So how about the concentration of oxalate used in the model?

Other minor comments:

Line 11, the wording of sulfate should be better consistent, “sulphate” or “sulfate”.

Line 13, the statement of “observed concentrations of transition metal ions” is not appropriate from my perspective, given the authors only measured the total concentration of Fe and Cu.

Line 22, “...affect the environmental quality and human health”, references to support this conclusion are lacking.

Line 149, “Obvious correlations between alpha-Fe(III) and sulfate...”, The author may better calculate the R^2 of alpha-Fe(III) and sulfate.

Line 380, in Fig. 1(d), the modeled sulfate concentration line is missing.