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Comment on acp-2021-628

Anonymous Referee #1

Referee comment on "Process-based and observation-constrained SOA simulations in China: the role of semivolatile and intermediate-volatility organic compounds and OH levels" by Ruqian Miao et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-628-RC1, 2021

Miao and coauthors applied process-based and observation-constrained schemes to simulate OA in China. They found that the addition of nitrous acid sources and therefore the enhanced OH concentration are critical for the improved performance of model simulation. Overall, it is scientifically valid work and advances the important work of simulating SOA in polluted areas. My primary concern is that the bulk of science here is major in the "Model techniques and comparison" not the "Chemistry and Physics". With its current version, I am not totally convinced that it should be published in this specific journal. Also, I do have some technical concerns noted in my general comments below.

(1) To evaluate the model performance, the authors compared the OA simulations with campaign-average OA observations from 2011 to 2019. Do the authors run model simulations also from 2011 to 2019? It was claimed that the base year of the model simulation was 2014 (Line 101). Then for comparison, how do the authors match their simulations with observations from different periods and different years? It seems that the authors ignored the impact of inter-annual variability in their study. But both the emission inventory and observations has demonstrated significant changes of organic carbon in China due to clean air actions. How do the authors consider this concern?

(2) The authors ignored the difference between submicron and find particles due to the lack of information (Line 86). How would it influence the model-observation comparison and thus conclusions from this study?

(3) The SOA yields for monoterpenes and sesquiterpenes were set to be the same in this study (Line 124). But lots of studies have shown significantly different SOA yields of MT and SQT (Yee et al., 2018, Atmos. Chem. Phys., and references therein). Would this largely vary the SOA simulations, especially in summer?

(4) In addition to the statistical values, please also provide the scatter plots for model-observation comparisons with 1:1 line. To clearly see how the model performance varied for urban, suburban, and remote regions, the authors can set different colors for the data points.

(5) In "Results and discussion", the authors mainly focused on the comparison of different model schemes but not the scientific information on chemistry and physics. I suggest to separate this part into several sections and focus more on the scientific value of their model results.

Other comments and suggestions to the text:

Line 105: While more details can be found elsewhere, please still briefly describe in this study.

Line 125: Explain more here why it makes sense to use a fixed lifetime for all SOA precursors.

Line 136: Why the authors use 2.5×10^{-6} s^{-1}? Reasons or references?

Line 196: How much does the model overestimate O_3 and NO_3 concentrations and underestimate OH concentration? Please provide the specific number (e.g., %) and their potential impacts on SOA simulations.