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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global-regional nested simulation of particle number concentration by combing microphysical processes with an evolving organic aerosol module" *by* Xueshun Chen et al.

Anonymous Referee #2

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This manuscript presented a significant effort to couple OA formation pathways and microphysical processes to a global and regional chemical transport model, with the goal of simulating the impacts of OA physics/chemistry to particle number size distribution and mixed particle composition. The work coupled a 1.5-D VBS module and the APM microphysics module to the IAP-AACM chemical transport module. The authors also presented some preliminary comparisons with the observations, and overall the model appeared able to capture the global OA concentrations and the CN10 concentrations. The amount of work done was impressive, and the methods were mostly valid and

up-to-date.

Overall, I think the paper may be published after clarifying some missing details and improving the figure representation.

Section 2.1 Host model: What meteorological data is the IAP-AACM driven by? I see in lines 342 to 350 that the model was driven by meteorological parameters from WRF, but maybe that information can be moved here. Also, how were global meteorological fields obtained from WRF? Did the authors run Global WRF? If so, then additional references for Global WRF should be included, e.g., Zhang et al. (2012). What was the spatial resolution of the meteorological fields, and was interpolation used? What was the temporal resolution of meteorological fields? I.e., how often were the meteorological fields updated. How was the nudging performed in the three nested domains?

Section 2.3 VBS module: I would like to see the model's representation of the relationship between oxidation state and volatility expressed more clearly. Did the authors simply move the oxidation products of POA and IVOC into a volatility bin that is one magnitude lower? What about the fragmented products during the oxidation, i.e., the smaller molecular weight products?

Section 2.3 VBS module: Also, a recent paper (Jo et al., 2019) indicated that the VBS representation of SOA formation from isoprene is incorrect because the reactive uptake pathway dominates SOA formation from isoprene. Please discuss this point, the lack of reactive update pathways in this model, and the implication for the present model results.

Lines 359-360: "In the LV_POA and HV_POA experiment, quartiles of the above-mentioned distribution factors are used". Not sure what this meant. Looking at Table 2, I do not see the use of 'quartiles of the above-mentioned factors'. What different factors were used in the LV_POA and HV_POA experiments, respectively?

Table 2 and related text on the design of the sensitivity experiment: overall, I think the

sensitivity experiment could be explained more clearly. I was not able to understand what was the goals of the sensitivity experiments and how those goals relate to the parameters in Table 2.

Lines 382- 383: "...the China Atmosphere Watch Network... Zhang et al. (2008)": What year(s) were the measurements? The writing of this sentence seemed to suggest that the measurements were from 2010, which cannot be possible.

Lines 437 to 439: "The number concentration of particles from 100 nm to 1000 nm ...correlation coefficient being 0.70.': Figure 2 uses a different unit for particle size (micrometer), and I do not see the diameter extending to 1000 nm. Also, how were the normalized bias and the correlation coefficient calculated? Did the authors calculated only the bias and correlation for the time series of the total number concentration (which is not shown)? Or did they calculated a mean bias and correlation for the entire PNSD spectrum? If the latter, how was this done, and did the statistics entail a preferential weighting of the smaller particles?

Figure 4: The measurements were too small and unreadable in this figure. Please enlarge and circle with a black or white outline.

Figure 4: Also, the OC measurements in China all appeared to be much, much higher than the simulated concentrations. This is inconsistent with what was shown in Figure 1, where the authors indicated that the model was able to represent the observed OA concentrations in the one site in Beijing. Please resolve this inconsistency or provide more discussion in the text. The discrepancy between the measurements and the simulated concentrations in Figure 4c is large enough that, I do not think the difference in the year could explain it. Also, the symbols were too small to read.

Lines 499-500: "Overall, the model explained most of the observations." I really did not see this in Figure 4c. Please revise and provide an estimate of the bias.

Figure 4: Are there also observational constraints on what fraction of the measured

OC was secondary, e.g. using the EC-tracer method? I think this was in Zhang et al. (2008) and can be shown in Figure 4d for comparison

Lines 509-511: "However, our simulations show in Fig. 5 ... large anthropogenic emissions." How does this statement relate to, or can be used to explain the finding in Fig 4, i.e., the model severely underestimated the observed OC, particularly over China?

Lines 518-519: "In the second ... over China": How does this statement relate to, or can be used to explain the finding in Fig 4, i.e., the model severely underestimated the observed OC, particularly over China?

Figure 7: Again, all of the symbols for the observed values were way too small and unreadable. Please revise.

Figure 9 caption, last line: "over the first domain (top panel) and second domain (bottom panel)": should be 'left panel' and 'right panel', respectively.

Reference Jo et al. (2019), Geosci. Model Dev., 12, 2983-3000.

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