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Geoscientific Model Development Discussions

Interactive comment on "Modeling Regional Air Quality and Climate: Improving Organic Aerosol and Aerosol Activation Processes in WRF/Chem version 3.7.1" *by* Khairunnisa Yahya et al.

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Reply to Reviewer 1

Regional models exhibit large uncertainties in the simulation of secondary organic aerosol (SOA) which have substantial impacts on climate due to aerosol-cloud interactions. This paper reviewed the current Volatility Basis Set (VBS) treatments and investigated the model performances in SOA simulation with a series of scenarios by changing the model configuration in chemical mechanisms and aerosol activation parameterization. Results suggest that simulations with VBS treatments present better agreement with observations compared to the traditional OA method, however, parameters such as the enthalpy of vaporization, percentage of fragmentation and functional-

ization, and POA emissions can largely influence the result. The paper is well written. I would recommend it to be published after minor revisions.

Reply: We thank the Reviewer for the comments to improve the presentation of the manuscript. Where applicable, suggestions have been taken into consideration and added to the manuscript. Please see below our point0-by-point replies.

Apparently, the POA emissions play an important role in the simulation of SOA. Better performance is suggested in scenarios with increased POA emission. Does that imply that POA emission is underestimated in current NEI emissions? I would suggest the authors to provide some discussion about that.

Reply: Yes, POA emissions are underestimated in current NEI emissions as POA is assumed to be nonvolatile. In the text, this sentence describes the underprediction in POA emissions: "With the semivolatile POA and FF cases in this study, additional IVOC and SVOC emissions are added as three times of the traditional POA emissions from NEI, to account for missing IVOC and SVOC species in the traditional POA emission inventory."

Page 47: "SSummary" should be "Summary"

Reply: This has been modified.

Page 50: Table 4. Note of "The simulations without the sufiňĄx "POA" indicate the cases with nonvolatile default POA emissions" need to be clariiňĄed, it should be "The simulations without the sufiňĄx "POA" or "FF"".

Reply: This has been modified.

Page 52: Table 6. Poor correlation is suggested in most of cases, implying that some important SOA source is missing, biogenic SOA?

Reply: Yes, as mentioned in the text: "The SOA data from the CalNex campaign only consider contributions from a small number of precursors including biogenic precursors

(i.e., isoprene, ïĄą-pinene, and ïĄć-caryophyllene), and the anthropogenic precursors (i.e., toluene, polycyclic aromatic hydrocarbons (PAHs) and methyl butenol (MBO))". This discussion has been made clearer to include that this reason also likely contributes to the poor correlation.

Page 53: for CASTNET, the simulated Max 8h O3 is very close to the simulated Max 1h O3, especially in CB6 (41.9 vs 41.8), but the observation doesn't (51.8 vs 47.4). Does that mean the model underestimate the peak value of O3?

Reply: Yes, this is likely to be true. In addition, NMBs and NMEs for Max 1h O3 are higher compared to Max 8h O3, which means that the model is not predicting well the transient peak O3 concentrations.

Page 54: "CB05-25%FF-EM3" present different values in Table 8 and 9, while observation is the same. Please double check.

Reply: Table 8 cases use the Grell-Freitas cumulus parameterization scheme, while Table 9 use the MSKF scheme. This has been made clear in the table headers.

Page 55: Figure 1, "A/OC ratios" should be "OA/OC rations"

Reply: This has been modified.

Page 64: Figure 10, it is very interesting that low CDNC shows at the edge of simulation domain, any explanation about that?

Reply: This is likely due to the fact that there are no boundary conditions for CDNC.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/gmd-2016-288/gmd-2016-288-AC1-supplement.pdf

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