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Interactive comment on "Simulation of the evolution of biomass burning organic aerosol with different volatility basis set schemes in PMCAMx-SRv1.0" *by* Georgia N. Theodoritsi et al.

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(1) *This study tested the performance of a new VBS parameterization in the chemical transport model PMCAMx-SR on simulating biomass burning organic aerosol (bbOA) in the U.S. The results show that the model performs differently depending on the season, indicating further needs to quantify the emissions and reactions of IVOCs from specific biomass burning sources. The paper is generally well written, and can be helpful to improve the bbOA simulation in the U.S. I would recommend it for publication if the following concerns can be well addressed.*

We appreciate the positive assessment, the comments and the suggestions of the ref-

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eree. Our responses (in regular font) and the corresponding changes to the manuscript follow each comment (in italics).

General comments

(2) *The biomass burning emissions in this study include prescribed burning, agricultural burning and wildfire, while the tested VBS parameterization by Ciarelli et al. (2017) is constrained based on biomass burning in residential stove. Since the different biomass fuel types and burning conditions could largely influence the OA formation, is there any explanation about the potential bias? Could it be a reason the new VBS parameterization performs worse in the wildfire dominated season?*

This is a good point also made by the second referee. In most chemical transport models, the biomass burning organic aerosol emitted from all sources is simulated with the same parameterization (volatility distribution, chemical aging scheme). Our work provides some support to the hypothesis that different parameterizations may be needed for residential heating and wildfires. We have added this important point to the revised paper and it is clearly a topic that deserves additional attention.

(3) *The model evaluation section lacks necessary technical details, making it a little difficult to follow. Do the 161 STN sites and 162 IMPROVE sites measure only $PM_{2.5}$ or also OA? In L403 it refers to "daily average $PM_{2.5}$", but the analysis is for OA. Please clarify it. Is there any information about the OA measuring methods? In addition, besides the Table 2 it will be more straightforward to add a map showing the spatial distribution of the mean bias for each site.*

We have provided additional details about the measurements used for the model evaluation in the corresponding section. All the STN and IMPROVE sites measure both the $PM_{2.5}$ concentration and its composition. Therefore, they provide OA measurements (the networks actually measure OC and OA is then estimated). The word OA was missing in L403; we have corrected this typo. We have also added some additional

information about how OA is measured in the two networks. Finally, we tried preparing maps with the spatial distributions of the evaluation metrics, but there was little additional information there so we would prefer not to include them.

(4) *For the structure of the manuscript, it makes more sense to evaluate the model performance first, and then predict the bbOA and discuss where the differences of two VBS schemes come from. I would suggest moving the section 6 before current 4 and 5.*

We have followed the suggestion of the reviewer and changed the order of presentation of the results. We now discuss first the model performance and then discuss then analyze the predictions of the two schemes.

Specific comments

(5) *L28, "were mixed" is not clear, better to specifically refer to the seasonal differences.*

We have rephrased this sentence referring specifically to the seasonal differences.

(6) *L53, the references here could be more updated.*

We believe that it is important to include some of the older work that established something, but we agree with the suggestion that some additional more recent references would be useful. A few more recent references about the important of biomass burning as an important global air pollution source have been added.

(7) *L78, the term "VBS" is already defined in L66.*

We have deleted the second definition of the acronym.

(8) *L92, the "$PM_{2.5}$ OA" needs to be defined. It seems not necessary to add "$PM_{2.5}$".*

We have rephrased this sentence. The discussion of previous work refers to PM_1, $PM_{2.5}$ and PM_{10} OA, so we would prefer to be accurate and specify the corresponding size range.

(9) *L96, is the "overprediction of bbOA" based on comparison with source apportionment of measurements? Since most of the source apportionment studies do not separate the bbSOA, do you mean bbPOA?*

This is a good point. The evaluation was against OA measurements, so the discrepancy could be due to either the overprediction of bbPOA or bbSOA or both. We have rewritten this sentence to avoid confusion.

(10) *L143, the "2.5 times" may need some references.*

We have added both the original reference (the Robinson et al., 2007 study) and a couple more additional references from other applications of this factor.

(11) *L267, the bbPOA level 0.02 μg/m^3 is quite low, even lower than the difference of two models in bbPOA (0.1 μg/m^3) in L265. If they refer to average in different time period or scale, it needs to be clarified.*

They do refer to different quantities. One (the 0.02 μg/m^3) is the average difference over all the modeling domain and the other (0.1 μg/m^3) is the maximum difference in the domain. We have rephrased these sentences clarifying these quantities to avoid confusion.

(12) *L674, the format of Table 1 needs to be updated. Please use the standard three-line table.*

Table 1 will be formatted according to the GMD typesetting requirements.

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