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Interactive comment on "Influence of biomass burning vapor wall loss correction on modeling organic aerosols in Europe by CAMx v6.50" *by* Jianhui Jiang et al.

Anonymous Referee #2

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General Comments

Jiang et al. report a modeling study that: (1) evaluates the impact of vapor wall losses during chamber studies on parameters for SOA formation from residential biomass burning emissions, and (2) simulates the increases in OA and SOA concentrations once a CTM is updated with new SOA parameters that take into account these wall losses. Overall, the manuscript is well-written and addresses an important topic in the field of atmospheric aerosol modeling, since implementing accurate parameters for SOA formation in CTMs is challenging. I support publication in GMD once my comments below have been taken into consideration.

Line-by-line comments

Page 1, Line 27 – 29: What is the difference between the "standard VBS" and the "reference scenario"? Does the reference scenario refer to the traditional two-product approach? Please clarify as this is an interesting finding. Less importantly, I also don't understand why the authors have specifically highlighted the result from Romania.

Page 2, Line 37 – 39: The authors imply that residential biomass burning emission is "the dominant source for. . . secondary organic aerosols in winter". However, this statement is supported exclusively by three European studies that are referenced on line 39. Therefore, the authors need to clarify that this conclusion is specific to the European domain.

Page 2, Line 47: I would kindly suggest that the authors specify that the POA emissions are treated as semi-volatile when this "scaling-up" is performed, since some models still assume that POA is nonvolatile.

Page 2, Line 57: The work of Hayes et al. 2015 concerning vapor wall losses used a box model and not a CTM.

Section 2.1: How does the utilization of beech wood as the only fuel potentially bias the results? Is this a fuel commonly used in residential biomass burning in Europe? Basing the parameterization of the VBS scheme on a single fuel is not necessarily a flaw in the study, but some contextualization is needed here to understand how this limitation might influence the model results.

Line 105: The phrase "gas-phase equilibrium concentrations in particle phase" is not coherent. Please clarify. Furthermore, I don't think equation (3) can be correct. A partitioning coefficient of 1 would indicate complete partitioning to the particle phase, but this would give a Ceq(i,p) value of zero. This comment also applies to equation (4).

Line 201: The reasoning why the OM loading would have an effect on the box model's accuracy is not clear. Please elaborate. Also, there are some runs when the loadings are low when the model accuracy is very reasonable, for example a11, so the OM loading does not seem to explain by itself the poor accuracy of the model observed for experiments 9 and 14.

Figure 1: It would be useful if a table of the experiment conditions was provided.

Line 203 – 204: I think the text contains an error here. If anything there is an underestimation at short times and an overestimation at long times. More generally, it seems like the comparison between the model and the measurement varies a lot between experiments, so it is difficult to make conclusions regarding whether the box model is overestimating or underestimating.

Lines 205 – 207: I think using percentages here to compare the two box model versions (with or without vapor wall losses) overstates the difference between the models. In the end, the differences in the MB and RMSE are only about 6 ug/m3, which is not very much when most of the experiments are run at OM concentrations near or above 100 ug/m3.

Figure 3: I very strongly suggest that these data also be given in a table so that the quantitative results can be used by other researchers.

Lines 284 – 287: This sentence is confusing. Which model cases are specifically being compared? In addition, in Figure 7, only the schemes VBS_WLS and VBS_noWLS are compared, but then in the text the 3POA scheme is mentioned as well.

Figure 8: It would be helpful to specify in the figure caption that these plots are annual averages. In addition, why are annual averages used and discussed rather than wintertime measurements, as is done in the other sections of the manuscript?

Lines 318 – 322: The comparisons summarized here are rather haphazard. First, for OA, the VBS_WLS scheme is compared to the VBS_BASE scheme. Then next, for SOA, the VBS_WLS scheme is compared to the SOAP scheme. The authors should be consistent in what schemes they are comparing to as “base cases”.

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