Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-425-RC2, 2019

Atmospheric Chemistry and Physics
Discussions
Open Access

Interactive comment on "Nonlinear behavior of organic aerosol in biomass burning plumes: a microphysical model analysis" *by* Igor B. Konovalov et al.

Anonymous Referee #1

Received and published: 1 July 2019

Review of "Nonlinear behavior of organic aerosol in biomass burning plumes: a microphysical model analysis"

Summary: This manuscript addresses the different observed effects of atmospheric aging of organic aerosol in biomass burning plumes and their lack of representation in models. The authors used different complexities of volatility basis set (VBS) schemes in a microphysical dynamic model to simulate biomass burning organic aerosol evolution. They analyzed the nonlinear governing processes that drive the evolution of biomass burning organic aerosols. They also considered the biomass burning organic aerosol mass enhancement ratio, examined the behavior of the hygroscopicity param-

Printer-friendly version

Discussion paper

eter, and identified five different regimes of organic aerosol evolution. they come to the conclusion that the evolution depends on initial concentration and plume size of the specific biomass burning smoke plume, and such parameters are not usually captured in models.

This manuscript is very well written, insightful, in the scope of this journal, and will attract community attention for its significance. I appreciate that in the text, the authors not only emphasized the importance of this work but also provided some insights into associated uncertainties (and/or assumptions) and hinted at areas of improvements for future studies. I recommend it for publication in ACP, and I have the following minor suggestions for improvements.

Text comments: The use of chemistry transport models to simulate a single plume seems to be off-scale. Would the authors comment on the uncertainties associated with this disadvantage?

Page 10: Would the authors comment on why they did not include NVSOA formation in MDMOA? Or is it the conventional OA scheme that was mentioned on page 8 line 6?

Page 13 line 24-26: Would the authors specify in the text each of the size bin's range? Also, which 3 size bins were used for T18 and T18f?

Page 14 lines 3-7: "The concentration of OH ....based on the ambient measurements by Akagi et al. (2012), its value was set to 5×106 cm-3 in all our simulations. We also assumed a constant temperature of 298 K... " Is a plume environment equivalent to ambient conditions? e.g. is it valid to assume a constant temperature of 298K and ambient OH concentration in a plume? Would limited photochemistry within a plume reduce OH concentration?

Page 14 lines 8: "Along with aerosol species, MDMOA has been configured to simulate the evolution of an inert tracer. " Would the authors please clarify the concept and use of an inert tracer (it was initially mentioned in the abstract)? What is its composition and properties?

Page 19, Figure 4, perhaps setting both figures with the same y-axis scale would be helpful. Also, in the text, it's unclear why only T18 and T18f schemes are shown, but not other schemes.

Page 24 "This observation indicates that the mass concentration of aged BB OA is likely to be much more strongly affected in the simulations by uncertainties in available representations of the BB OA evolution than the mass concentration of relatively fresh BB OA. One of the reasons is that fragmentation reactions become increasingly important with time when the SOG oxidation level increases, and then the competition between functionalization and fragmentation creates the more complex dependence on the plume parameters." Would the authors please elaborate on or quantify the competition between functionalization and fragmentation? e.g.the branching ratios?

Page 32 lines 28-29: The authors mentioned that such differences among VBS are under the "typical conditions in summer mid-latitudes", it would certainly be intriguing for future studies to examine how these behaviors vary in different environments.

Figures comments: It's unclear what the shaded greys represent in the figures, and they are quite distracting, I suggest that the authors justify them in the caption or remove them.

It may be already sufficient that figures have different colored lines. Adding different symbols are just adding noise to the figures (just as with the grey shades). But it could be a personal preference, just a suggestion.

Again, it would be helpful to compare different schemes if the axis scales are the same when possible (in the same magnitude range).

For consistency among figures, I would suggest that Figure 4 follow other figures (Fig 3 etc) to include legends in the same locations.

Figure 6's (and Figure 7's) caption mentions "dashed lines," although at first read (without looking at the legend's Ctot/C0), it's unclear if it's the dashed lines with dots and dashes, or dashes alone (which should be Ctot/C0). There two types of dashes, it'd be helpful to distinguish the two.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-425, 2019.