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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The role of semi-volatile organic compounds in the mesoscale evolution of biomass burning aerosol: a modelling case study of the 2010 mega-fire event in Russia" *by* I. B. Konovalov et al.

Anonymous Referee #1

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This manuscript represents an important scientific research topic about the role of SVOCs in evolution of BBOA. The authors conduct various sensitivity studies using the standard versus VBS approaches to investigate aging of biomass and SOA formation therein. But before the manuscript can be accepted, I recommend major revisions especially related to the descriptions of methodologies. Following are my suggestions:

1. Section 2.4.2 Suggest differentiating between POA in the gas phase and particle phase using different subscripts e.g. POA(g) and POA(a). This is important to clarify

that aging and oxidation in the VBS scheme implemented by the authors is done just in gas-phase.

2. Page 9123: Line 20: The authoirs say that they use the same mass yields as given in Table S3 of Jathar et al. But Table S3 of Jathar et al. has yields for C*=0.1,1,10 and 100 ug/m3. In addition line 10 says authors used a single surrogate species based on Jathar et al. These sentences are confusing and contradictory. Please clarify. Also they say that n-pentadecane represents 10% of NMHC in addition to POA. When I look at their Table 3, none of this is obvious. Suggest re-writing of section 2.4.2 to clarify this. Also C*=10,000 in Table 3 is in the intermediate volatility (IVOC) range. Please use consistent terminologies with previous studies (e.g. Jathar et al. 2014 and references therein).

3. Section 2.7: Line 20: Authors disregarded secondary inorganic aerosol from fire emissions. This is hard to justify given that authors are comparing PM10. WHat fraction of measured PM10 is organic versus inorganic?

4. Authors ran fire emissions without emissions from other sources and zero boundary conditions. Did they do test simulation with just boundary condition turned on to see how much boundary condition contributes to simulated aerosol?

5. Table 2 needs to be more descriptive. Looking at it , the difference between the different VBS scenarios is not obvious. One needs to connect scattered information from various Tables and description in the text to understand these differences. The authors need to make it easier for the readers.

6. Table 4 and Figure 7: How were perturbations of PM and CO calculated? Were they the differences between model run with just fire vs. other aerosol? Also was the mean PM10 or CO varying spatially and temporally?

7. The authors simulate POA and SOA but they compare PM10. They need to make a case from measurements that organic aerosols dominated PM10 concentration.

8. The auhtors have used the Grieshop et al. 2009 scheme for aging and volatility decrease. But previous studies hsowed that this scheme drastically overestimates SOA. See Hodzic et al. 2010. Please comment on the caveats intoruduced by using this aggressive aging scheme

9. The auhtors acknowledged that their method may have compensating errors due to neglecting fragmentation, which is a good point to make. But suggest citing some recent papers which showed the potential importance of fragmentation in 3D models (e.g. Shrivastava et al. 2013, Shrivastava et al. 2015).

Shrivastava M, A Zelenyuk, D Imre, RC Easter, J Beranek, RA Zaveri, and JD Fast. 2013. “Implications of Low Volatility and Gas-phase Fragmentation Reactions on SOA Loadings and their Spatial and Temporal Evolution in the Atmosphere.” Journal of Geophysical Research-Atmospheres, 118(8), 3328-3342, DOI: 10.1002/jgrd.50160, 2013.

Shrivastava M., Easter R., Liu X., Zelenyuk A., Singh B., Zhang K., Ma P-L, Chand D., Ghan S., Jimenez J.L., Zhang Q., Fast J., Rasch P., Tiitta P. “Global transformation and fate of SOA: Implications of low volatility SOA and gas-phase fragmentation reactions,” JGR-Atmospheres, doi:10.1002/2014JD022563, 2015.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 9107, 2015.