

# ***Interactive comment on “Sources of organic aerosols in Europe: A modelling study using CAMx with modified volatility basis set scheme” by Jianhui Jiang et al.***

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Jiang et al. provide a modeling study of organic aerosols in Europe using a volatility basis set approach. This study is important for this region and provides good insights about sources and formation of OA. Results are evaluated with measurements including PMF analysis of AMS/ACSM data.

Below I have several suggestions for improvement and also citation of relevant papers that need to be considered by the authors.

General comments on modified VBS approach: VBS is a framework that represents

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gas-particle partitioning and multigenerational aging of SOA. But depending on SIVOC emissions, reaction rates, functionalization/fragmentation branching etc. different implementations of VBS can produce very different results. Thus, it is important to describe VBS developments in the context of previous studies, specifically acknowledging and documenting differences. The authors describe their VBS as a modified VBS approach. But use of a “modified VBS” terminology has been used in 2 previous papers from M. Shrivastava et al. 2013, 2015. Those papers included both functionalization and fragmentation of organics and compared model results to several field measurements (surface based and aircraft measurements). See:

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/jgrd.50160>

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014JD022563>

The authors use the 1.5D VBS from Koo et al. To avoid confusion between author's version of VBS and previous 2 papers (above), I recommend the authors add a few sentences about how their modified VBS differs from the modified VBS aging parameterizations developed by M. Shrivastava et al. It may be better to refer to their VBS as 1.5D VBS, since this is what they used. It would be also instructive to compare their modified VBS results with those from M. Shrivastava et al. Note that Cholakian et al. 2018 (cited in this paper) used a similar modified VBS as Shrivastava et al. 2013,2015.

Specific comments: Page 3 Line 5-10: In addition to Hallquist et al. 2009, also cite M. Shrivastava et al. 2017 Review paper on SOA published in Reviews of Geophysics: <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016RG000540>

Page 3 Line 15-20: For WRF-Chem please cite 2 of the more recent papers on VBS implementation of SOA in addition to Shrivastava et al. 2011: <https://www.nature.com/articles/s41467-019-08909-4>  
<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/jgrd.50160>

Page 7 Line 5: While several models underpredict OA from biomass burning, some models predict OA from biomass burning could be much more important . See

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<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014JD022563>. This should be acknowledged here in addition to Hodzic et al. 2010 for biomass burning.

Page 11: Aqueous chemistry of organic aerosols (OA) in fog can also increase OA by 4-20% (see Gilardoni et al. 2014 PNAS for Po Valley Italy measurements of aqueous SOA). Since the authors underestimate winter-time OOA, missing aqueous phase SOA in fog would be an important source. Although they are overestimating OA during the autumn due to modeled bias in relative humidity and wet scavenging, the high bias could be due to other reasons like overestimation of SIVOC emissions, biases in aging parameterizations. This needs to be acknowledged as a caveat. The authors could also compare rain rates simulated by their model to measurements in that region to provide further evidence for model underestimation of rain rate/wet scavenging.

Page 12 Above line 5: Instead of “biomass density” the authors could probably just say increasing biogenic emissions here?

Page 12 Line 20: From Figure 6 it seems the authors could have applied a site specific scaling of POA emissions based on PMF HOA+BBOA. This could improve their POA, its diurnal variation and also IVOC emissions for biomass burning that are calculated as  $\sim 4$  times BB-POA. Please comment on use of a site-specific scaling of BB-POA and IVOC emissions based on PMF HOA+BBOA.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-468>, 2019.

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