

Interactive
Comment

Interactive comment on “Secondary organic aerosol formation during June 2010 in Central Europe: measurements and modelling studies with a mixed thermodynamic-kinetic approach” by B. Langmann et al.

Anonymous Referee #3

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In this manuscript, the authors extended the secondary organic carbon (SOC) aerosol formation scheme used in the regional three-dimensional atmosphere-chemistry-aerosol model REMOTE by considering the aging of organic vapours and kinetic condensation of low volatile organic compound (LV-SOG), following the approach developed by Yu (2011). Effects of new SOC scheme were evaluated by comparing simulated SOC mass concentrations with those observed at Puy-de-Dôme, France during a measurement campaign in June 2010. The authors also used a highly simplified nucleation scheme to study the possible effect of organic vapour nucleation. SOC is

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an important component of atmospheric particles and it has been well recognized to be under-estimated in various models. The effort to incorporate new or recently developed scheme to improve SOC simulation in the on-line climate-chemistry model is important. I recommend the publication of this manuscript in ACP after the following issues are properly addressed or clarified.

1. Based on my reading of this manuscript, the authors have two major objectives: (1) to extend the SOC formation scheme and evaluate the impact on simulated SOC mass concentrations; (2) to explore the effect of organic vapour nucleation. I feel that the manuscript can be improved if the authors focus solely on the first objective, for the following reasons.

(1) As the authors pointed out, nucleation events at Puy-de-Dôme were rare during the chosen period of investigation. There are lots of measurements of nucleation events at a number of sites in Europe (within the domain covered by the REMOTE simulation). To present a convincing case, the authors need to look into periods when or sites where significant and frequent nucleation events occurred.

(2) The nucleation scheme used in the REMOTE (e.g., Vehkamaeki et al., 2002) is for binary homogeneous nucleation of $\text{H}_2\text{SO}_4\text{-H}_2\text{O}$. To treat LV-SOG as H_2SO_4 in binary nucleation parameterization calculation is completely unjustified and does not offer much scientific insights.

(3) REMOTE is a climate-chemistry model. The authors should and can readily extend objective 1 by looking into the impacts of the extended SOC formation scheme on cloud properties and regional climate.

2. H_2SO_4 gas and LV-SOG concentrations are key components of this study. The authors should provide their values. The sensitivity of LV-SOG (and maybe also SV-SOG and MV-SOG) concentrations to major assumptions (e.g., threshold values, VOC emission rate, etc.) should be presented and discussed.

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3. Page 26765, line 26. Please explain in more detail how you update the whole model domain with ECMWF data. Restart the simulation? How did you deal with chemistry fields when you force the model to stay close to the observed weather situation?

4. Page 26768, lines 1-5. Simplification generally compromises accuracy. Could you discuss how your simplification might have affected the results?

5. Page 26772, lines 1-6. Why the second aging step doesn't modify OC mass concentration much? Based on my understanding, the condensation of LV-SOG is critical in the thermodynamic-kinetic approach. Without second aging step (SV-SOG to LV-SOG), where did LV-SOG come from?

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