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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of gas-to-particle partitioning approaches on the simulated radiative effects of biogenic secondary organic aerosol" *by* C. E. Scott et al.

Anonymous Referee #1

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In this manuscript, the authors use a global aerosol microphysics model and an offline radiative transfer model to study how the manner in which secondary organic aerosols (SOA) is added to the aerosol size distribution affects simulated changes in CCN-sized particles and the radiative effects of biogenic SOA. They show that the first aerosol indirect effect (AIE) due to biogenic SOA is sensitive to the SOA model treatment. This work highlights the importance of better representation of the impact of SOA on particle size distributions. The content of this work is within the scope of ACP. The following comments should be properly addressed before I can recommend it for final publication in ACP.

Major comments:

1. The authors used two different methods to treat the partitioning of biogenic organics into particle phase ("kinetic" and "equilibrium" approaches). In the "kinetic" approach (Eq. 9), all organics from the oxidation of monoterpene were assumed to be non-volatile. This may significantly over-predict the growth rate of freshly nucleated particles as only the extremely low volatile part (a small fraction) of oxidation products can directly condense on the nucleated particles. In the "equilibrium" approach (Eq. 10), all SOA were also assumed to be non-volatile and the re-partition of SOA into gas phase were not allowed. This can't really be considered as "equilibrium" approach because it was not "equilibrium" at all (no reparation, no temperature-dependence). The authors should at least discuss the uncertainties associated with the non-volatile assumption of all oxidation products. A figure showing the global distribution of modeled Sorg based on the two approaches is needed to help the reader better understand the results of the work.

2. Another concern is related to the uncertainties of this study associated with several other simplifications and assumptions:

(1) Offline chemistry ("prescribe six-hourly mean offline oxidant");

(2) No nitrate and ammonium;

(3) "secondary organic material is generated at a fixed molar yield (13 %) from the oxidation of monoterpenes".

The formation of atmospheric particles and their growth to CCN sizes depend on the concentrations of precursors involved. The authors showed that the method to deal with organics lead to large difference in CCN and AIE. The above simplifications and assumptions will surely impact CCN and AIE. The authors need to justify the usage of these simplifications in this study and discuss how these may affect the conclusions of this work.

3. GLOMAP-mode was employed for this study. The mode approach uses several log-normal to describe particle size distributions. Since this work focuses on the effect of particle growth associated with SOA on particle size distributions and CCN concentrations, I was wondering why the authors didn't use their more accurate GLOMAP-bin aerosol model for the work.

4. Page 4150, Equation (2). How [NucOrg] was calculated in the model? Is NucOrg the same as Sorg (Eq. 4)? You only mentioned that NucOrg represents monoterpene oxidation products but monoterpene oxidation should have many products of different volatilities. Some details on the calculation of [NucOrg] and global distribution of [NucOrg] should be given.

5. Page 4150, Equation (2). Riccobono et al. (Science, 2014) gave a more updated formula for the organically mediated nucleation. Why not use the formula given by Riccobono et al.? What is the uncertainty associated with the usage of specific nucleation parameterizations? A figure showing the global distributions of nucleation rates predicted by ACT and ORG schemes should be provided.

6. Figure 1 shows the simulated and observed particle size distributions at a boreal forest site. The conclusion of this work is for global. Therefore, it is necessary to compare simulated CN and CCN number concentrations with those observed at different sites around the globe.

Minor comments:

1. Pages 4147-4148. As I understand, both kinetic approach (for low volatile organics) and equilibrium (for semi-volatile organics) were used in in the work of Yu (2011) cited here. This should be reflected and correctly described in the review of previous work in the Introduction.

2. Page 4149, line 12. Offline radiative transfer model was used. Please add some details on what aerosol (and gas) fields at what temporal resolutions were saved for

the offline RT calculation.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 4145, 2015.