

Interactive comment on “Organic condensation – a vital link connecting aerosol formation to climate forcing” by I. Riipinen et al.

Anonymous Referee #1

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Referee General Comment:

The manuscript shows the importance of organic vapor condensation in global climate. The authors also demonstrate the large sensitivity of aerosol number concentrations to the SOA condensational behavior using the global chemical transport model. Overall I found it very nice study combining two field measurements, microphysics models, and a global climate model. The results have important implications and they are certainly interesting for atmospheric science community. The figures are mostly clear and attractive. The paper fits well within the scope of ACP and I look forward to seeing this published in ACP. However some issues are unclear as specified below. The authors should consider and implement the following major comments in the revised manuscript before the manuscript can be accepted for publication in ACP.

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Major Comments:

1. The abstract can be improved better. Please be more specific and bring more points of the results into the abstract. Since this manuscript does not have conclusion section, the authors may extend the abstract so that readers can take important messages of this study more easily.

- The expression “state-of-the-science” is not a common word and ambiguous (At least I could not find this word in the dictionary. Did authors mean state-of-the-art?). Please be more specific. For example, authors can say “We demonstrate that the thermodynamic models fail...”.

- What modeling approach is that which is consistent with measurements?

- “The large sensitivity of climate forcing” is too far to conclude by this study. The authors indeed showed the increase in aerosol number concentration by organic condensation, but did not ‘demonstrate’ any change of climate or radiative forcing. I think ‘discuss’ is a fair word to use here.

2. The description of microphysics models are not enough and it is hard to see how authors did simulations presented in Fig. 5-8 exactly. The authors provided only rough and qualitative information of the models. The authors should specify/clarify at least some key equations, parameters, and assumptions used in both thermodynamic and kinetic models. If the authors do not want to disturb the flow of the discussion, the authors may put them in the appendix or supplemental information.

- For kinetic model results of Fig .5, did the authors assume mass accommodation coefficient of 1? If so, please explicitly mention this. I expect the kinetic model results are very sensitive to this parameter. Did the authors do any sensitivity tests?

- The authors use the sentence “organic mass flux condense kinetically” quite often. But what do you mean exactly? Do you mean that all molecules colliding to surface are taken up by particle? In reality, anyway molecules collide kinetically to the surface,

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whether you treat them thermodynamically or kinetically. Therefore, I am wondering if the way authors present (50% kinetic + 50% thermodynamic) are really appropriate way.

- Although the authors call the model used in Fig. 5(d) very general name of 'kinetic model', it seems that the authors did not resolve any chemical kinetic processes (i.e., chemical reaction on surface and in bulk), but consider only a part of the physical kinetic processes (i.e., collision and condensation, and also gas phase diffusion?, but not bulk diffusion). I would suggest calling this model 'kinetic condensation (collision) model', for example.

- Is the simulation result shown in Fig. 5(b) gas diffusion limited or mass accommodation limited?

- In the thermodynamic model, it seems that the authors consider only absorptive partitioning, but not adsorptive partitioning (is this correct?). If yes, probably it is better that authors call this model 'thermodynamic absorptive model' for clarification, as adsorptive process is not included. If the authors include adsorptive partitioning in their model, do the authors get better results than Fig. 5(b)? Are authors sure that adsorptive partitioning is a minor process?

- P399, L13: Why the authors are sure that these assumptions do not change any conclusions?

3. The authors suggest that the 50%-50% ratio of LV- and SV-OOA corresponds to the ratio of the organic mass to condense on the aerosol. I think LV-OOA can be formed mostly in the condensed phase reactions and it may be difficult to form such high mass and very low volatile organic compounds by gas phase reaction. Since the presented models (both thermodynamic and kinetic models) seem to consider only gas phase oxidation but no surface or condensed phase reactions, how the authors can connect thermodynamic and kinetic partitioning to LV- and SV-OOA?

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4. The authors use the word "climate forcing" both in the title and abstract. However, there is actually not much discussion about climate forcing. Is it possible for authors to provide a figure of for example "increase in radiative forcing ($W m^{-2}$) due to ultrafine aerosol growth" analogous to Fig. 9-11?

Minor Comments:

- P.395, L14: What does EUCAARI stand for? Better to put a reference of EUCAARI project.

- P.399, L11: It should be "the fraction of semi-volatile..."

- P.400, L16: Please give references for large uncertainties in the emissions of SOA precursor gases in all atmospheric models, if you have.

- Nel, 2005 is missing in the reference list.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 387, 2011.

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