

We believe that we have properly addressed all review comments according to our answers to the reviewers. Below we describe the important changes in the manuscript, which may not been fully explained in the answers to the reviewers.

On P3, L30 to P4, L9 we have added a reference to Zhou et al., (2013) and discuss the difference in the estimated diffusion coefficients from this study compared to Abramson et al. (2013).

On P4, L29 to P5, L5 we have added a reference to Saleh et al., 2013 and discuss possible reasons why they do not observe a substantial evaporation inhibition because of diffusion limitations in the particle phase.

In Sect. 2.2.1, P10, L11-L18 we describe that phase separations between monomers, dimers and organic salts are not modelled explicitly in ADCHAM. We also write that we intend to update ADCHAM with a simple approach to calculate liquid-liquid phase separation (e.g. Topping et al., 2013).

In Sect. 2.2.3, P13, L25 to P14, L6 we have added a section which describe that in ADCHAM the values of the first order loss rate from the near wall gas phase to the walls ( $k_{g,w}$ ) used in the model need to be substantially larger than the (experimentally quantifiable) effective total chamber volume loss rate  $k_{g,w}^*$  (e.g. from Matsunaga and Ziemann, 2010). We also describe how  $k_{g,w}$  can be estimated from  $k_{g,w}^*$  and refer to a new figure in the supplementary material (Fig. S1) with results from several sensitivity tests.

In Sect. 2.3.4, P19, L4 to P20 L20 we have added several more references to studies about different oligomerization mechanism. We also write that from thermodynamic calculations of different organic mixtures, it has been suggested that ester formation (Barsanti and Pankow, 2006) and peroxyhemiacetal formation (DePalma et al., 2013) can be thermodynamically favourable, while hemiacetal formation is not (Barsanti and Pankow, 2004 and DePalma et al., 2013). We have added a discussion concerning realistic values of the dimer formation and degradation rates based on the work by Ziemann and Atkinson (2012).

In Sect. 2.3.4, P21, L13-L22 we have included an updated description of how we in practice handle the degradation of dimers back to monomers in the model and the limitations with this method.

In Sect. 2.4.1, P23, L6-L20 we describe the updated kinetic-multilayer model (with variable layer volumes) for diffusion of organic and inorganic compounds.

In Sect. 2.4.2, P25-26 we have replaced  $O_3$  with  $Z_{ox}$  that represents any kind of oxidation agent in the particle phase.

In the end of Sect. 3, P27 we refer to Table 2 that summarizes the different processes and range of parameter values used for the different simulations in Sect. 3.1-3.4.

We have changed order of Sect. 3.2 and 3.3

In Sect. 3.1 we have not made any important changes.

In Sect. 3.2 evaporation of  $\alpha$ -pinene SOA, Sect. 3.2.1 (previously 3.3.1) , Sect. 3.2.2 (previously 3.3.3) and especially Sect. 3.2.3 (previously Sect. 3.3.2) we have made substantial changes and we thus suggest that the reviewers read this part of the manuscript again. In all these sections we have added results where we test the potential impact of wall deposition losses in the smog chamber where the  $\alpha$ -pinene SOA is formed.

In Sect. 3.2.3 we have changed from modelling the formation of relatively short-lived dimers by reactive uptake and formation of dimer between pinonaldehyde and monomers at the particle surface, to an ordinary particle phase dimerization mechanism (peroxyhemiacetal formation). In this section which has been completely rewritten we examine whether the observed slow evaporation rate can be explained by nearly solid-like SOA in combination with two types of dimers; the first being relatively short-lived (lifetime of a few minutes) and a second long-lived (lifetime of more than a day).

In Sect. 3.3 (previously Sect. 3.2) we have not made many changes in the text except that we have added a sensitivity test where we also tested to model the  $\text{NH}_3$  uptake with a one orders of magnitude lower Henry's law coefficient (P42, L25 to P43, L3 and Fig S14-S15), and added a description and results from a few simulations with atmospherically more relevant and substantially lower  $\text{NH}_3$  and  $\alpha$ -pinene concentration (P45, L8-20 and Fig S16).

In Sect. 3.4 we have not made any substantial changes except that we refer to Fig. S17 where we compare the modelled and measured temporal evolution of the particle number- and particle volume concentration for simulations with different values of  $\bar{E}$  and  $u^*$ .

In Sect. 4 we have made some small changes in the conclusions on P58, L17 to P59 L2 and on P59, L9-L14 where we now write "However, for atmospheric more relevant  $\text{NH}_3(\text{g})$  and  $\alpha$ -pinene concentrations,  $\text{NH}_3$  has only a minor influence on the uptake of carboxylic acids to the particle phase. Thus analogous to Yli-Juuti et al. (2013), our simulations indicate that it is unlikely that  $\text{NH}_3$  and carboxylic acids from  $\alpha$ -pinene oxidation are responsible for the initial growth of nanometre sized particles over the boreal forest."

Because of the changes in the kinetic multilayer model for diffusion of organic and inorganic compounds, the model results in Fig. 5, 6, 7, 9,10 and 11 are all from new model simulations. However, the conclusions concerning these model results generally remain the same because the results are nearly identical with the previous ones (at least for those simulations where no other changes were made e.g. in the dimer mechanisms or wall losses).