

## ***Interactive comment on “Cloud droplet activation of mixed organic-sulfate particles produced by the photooxidation of isoprene” by S. M. King et al.***

**Anonymous Referee #2**

Received and published: 5 March 2010

General:

The manuscript deals with the investigation of the activation behaviour of mixed organic-sulfate particle originating from the photooxidation of isoprene. The activation behaviour of such particle is an important issue in quantifying the effects of atmospheric aerosol particles on both, cloud formation and the aerosol indirect effect. Therefore the paper deals with a topic highly relevant to the field of atmospheric research and is consequently suitable for publication in ACP. The manuscript is well-structured and nicely written. Earlier work is adequately recognized and credited and to my knowledge no portions of the manuscript have been previously published. In summary, the manuscript represents a significant contribution to the field of atmospheric science and should be published after minor revisions.

C398

Specific:

page 217, line 17ff: I suggest to give a little more detail concerning the chamber here.

page 217, line 23: I suggest to also indicate the variability of temperature and rh here.

page 218, line 11: It should explained/mentioned which type of particle generator was used here.

page 218, line 15ff: Why is dryer located downstream of the DMA ? At first sight that seems kind of counter intuitive ! A short explanation and discussion of the implications is needed. There is no dryer in Fig. 1 !

page 218, line 22: "The H<sub>2</sub>O<sub>2</sub> radical precursor used in these experiments followed the method of ..." A precursor is not a method! Rewording is needed here.

page 219, line 12: "for an OH-C<sub>5</sub>H<sub>8</sub> bimolecular rate constant of  $1.02 \times 10^{-10}$  cm<sup>3</sup> molecule<sup>-1</sup> s<sup>-1</sup>" I suggest to give a reference here.

page 220, line 22: "... , the larger sheath-to-aerosol flow ratio in SMPS2 compared to that in the DMA2 obviated the need in our application, and the TSI algorithm was used for inversion of the SMPS2 data." This is only correct if distribution widths differ by a factor of ten which is not the case here ! However, if only mean sizes and or integral properties are determined, it should be OK ! Nevertheless, some discussion is needed.

page 222, line 6: "The lines through the data points show the predicted CCN activation curves of the particle population based on the model described in King et al. (2009)." In Fig. 3, it should be indicated which line corresponds to what diameter.

page 222, line 7ff: "In the model, particles within the population have different critical supersaturations because of heterogeneities in particle diameter and organic volume fraction. One underlying source of heterogeneity is the distribution of diameters for the sulfate seed particles; this distribution is affected both by the resolution of DMA1 and by the presence of multiply charged particles. For similar reasons, there is heterogeneity

C399

in the particles that pass through DMA2. These heterogeneities explain the inflection point apparent in the predicted activation curves (as well as in the data) shown in Fig. 3." I have a hard time to understand what the authors are trying to explain here. More explanation and some rewording could be helpful.

page 222, line 21: "A sensitivity analysis showed that the predicted activation curves were reproducible for up to a 25% change in the optimized value of  $V_m,ORG$ , corresponding to  $\pm 0.03$  for  $\kappa_{ORG}$ ." I personally find this sentence pretty confusing. There should be at least a measure for reproducibility here. Maybe something like: A 25% change in  $V_m$  results in only a ??% change in critical supersaturation . . .

page 223, line 6ff: "Therefore, within uncertainty, the CCN activity of secondary organic material produced by the photooxidation of isoprene and that resulting from other investigated precursor gases are equivalent." I personally find the word "equivalent" a little awkward here. May stating that kappas are similar and therefore CCN activities are comparable would be more appropriate here.

page 223, line 11ff: "King et al. (2009) showed that, in the case of the dark ozonolysis of  $\alpha$ -pinene, the CCN activity abruptly increased as the percent contribution of signal intensity at  $m/z$  44 to the total organic signal intensity passed a threshold at 11%, which was concomitant with a decrease in organic particle mass concentration to less than  $1 \mu\text{gm}^{-3}$ ." Maybe I'm missing something here, but why does an increase in  $m/z$  44 come together with a decrease in organic particle mass concentration? At least a short explanation would be useful.

page 225, line 5: Is there any reason why Fig. S2 is not shown in the main text? It's discussed here anyhow, so why not show it?

page 225, line 29ff: I suggest to remove the whole paragraph and just state that adjustment of  $V_m$ ,  $i$ ,  $\sigma$  resulted in unreasonable values.

page 226, line 11ff: Maybe I'm missing something here. I get the impression that you

C400

are trying to explain the changes in activation by oligomerization. Couldn't it simply be explained by loss of soluble mass due to evaporation? A little more discussion would be nice here.

page 228, line 6: "Furthermore, the chamber studies cited earlier also suggest that secondary organic components produced from the oxidation of anthropogenic precursor gases also have  $\kappa_{ORG}=0.1$ ." It should be mentioned that this holds for activation only, hygroscopic growth might be different.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 213, 2010.

C401