

Interactive comment on “Increased cloud activation potential of secondary organic aerosol for atmospheric mass loadings” by S. M. King et al.

Anonymous Referee #2

Received and published: 11 March 2009

The authors of the manuscript address an important concern in the prediction of organic aerosol to act as CCN: that the composition of semivolatile organic aerosol changes as the partitioning of the gases and condensed materials with the mass concentration. This is important because the much of our knowledge about organic CCN has been taken from laboratory experiments, which usually require high aerosol concentrations due to instrumental limitations. The authors overcome this issue by generating the aerosol in a constant-flow mode, so that while the total aerosol mass concentration is at an atmospherically-relevant concentration, the aerosol mass concentration is also maintained to provide enough time for signal-to-noise averaging. The authors find that as the aerosol mass concentration decreases, the aerosol becomes

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



more CCN active in comparison to predicted values. The authors suggest that this is due to a decrease in the surface tension of the aerosol at lower mass concentrations.

Overall, the manuscript is written well and concise. The figures are clear and appropriate. The manuscript represents an important step forward for understanding how to connect laboratory measurements with atmospheric observations and the manuscript is a worthy publication.

Regarding the conclusion, that a decrease in surface tension is reasonable mechanism for changes in CCN activation with mass concentration: is it also chemically reasonable that the shift in the composition of CCN at low mass concentrations leads to species with lower surface tension? The authors cite the work of Grieshop et al. and Shilling et al. which are evidence of shifts in the overall chemical composition at low mass concentrations. If the authors do not agree that surface partitioning causes the decrease in surface tension, the authors should postulate which chemical species could this change in surface tension. And are there known chemical species with lower surface tension that are consistent with the increase in %m/z 44? Is it possible to measure the surface tension of the condensed phase at various mass loadings by traditional means as a confirmation? However this is addressed, authors should make an attempt to connect the observation of a decrease in surface tension with a plausible mechanism for differences in the species distribution (either chemical or physical location within the particle) of the condensed phase.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1669, 2009.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

