Atmos. Chem. Phys. Discuss., 8, S5399–S5401, 2008
www.atmos-chem-phys-discuss.net/8/S5399/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effect of organic compounds on the growth rate of clouddroplets in marine and forest settings" *by* N. C. Shantz et al.

N. C. Shantz et al.

Received and published: 29 July 2008

First of all, we would like to thank Referee #2 for the detailed review of our manuscript, with constructive comments and suggestions. Below are our replies to the specific comments provided by the Referee.

Referee specific comment: Was the lowered surface tension (as described in Table 1) used when kappa was determined? This was not clear from the text. My preference would be that it not be used, because the utility of the single-parameter (kappa) model is that all chemical effects (including surface tension) are encompassed in this one variable. If a surface tension lower that that of pure water was used in determining κ, I think the authors need to comment on how this might affect the reported values of kappa.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Response: Thanks very much for pointing this out. No, the surface tension was not lowered when using the kappa-CCN code. Surface tension of water was used and this has been clarified in the text.

Referee specific comment: I think the paper would be strengthened if the measured hygroscopicity of the marine and forest organic matter was compared to previous studies of these aerosol classes. Previous field studies are well-cited in the introductory discussion on aerosol-CCN closure (pgs. 8195-6), but I would add some citations to the discussion of hygroscopicity (pgs. 8213-4). My impression is that this work reinforces previous field studies, which have generally shown that continental (including forest) organic aerosols are more hygroscopic that marine aerosols. The authors might, for example, site some of the papers referenced in the review by Kanakidou et al. (2005).

Response: In Section 5, we have added references to hygroscopicity measurements of laboratory generated diesel emissions (to compare to the organic during July 18th) and we have added references to smog chamber experiments of biogenics. We have also included references to field measurements in Section 5.

Referee specific comment: Pg. 8214, lines 1-4: It seems to me more likely that kappa is less than 0.9 because the aerosol is not really 100% H2SO4. Any neutralization, even if the aerosol is still mostly acidic, would lower kappa.

Response: You are correct: some of the aerosol may indeed be neutralized which would drive the κ down. We were, however, comparing results from 2 models, the kappa-CCN model compared with the detailed microphysical CCNc model using pure H2SO4. Through discussion with Markus Petters directly, it turns out that this lower kappa is more accurate to describe H2SO4. There was a mistake in the Petters and Kreidenweis (2007) paper. This new information has been added to the text.

Referee technical corrections: Pg. 8195, line 12: Conant et al. (2004) assumed that the submicron aerosol mass was ammonium bisulphate, and the supermicron was sodium chloride. (I realize this is an extremely minor point.)

ACPD

8, S5399–S5401, 2008

Interactive Comment

Response: Clarification has been added to lines 10-11.

Referee technical corrections: Pg. 8195, line 14: Kaku et al. (2006) did not assume a 100% sulfate aerosol; rather, they used compositional data obtained from filter samples in their closure calculations.

Response: Thanks for pointing this out. This reference was a mistake and should not have been there. The reference has been removed.

Referee technical corrections: Pg. 8195, line 28: I think you mean that "this *decreases* (not increases) the diameter at which an organic particle will activate...".

Response: We have changed the wording in this section for clarification.

Referee technical corrections: Pg. 8200, lines 19-20: This sentence confused me - at first I thought you were describing how S is calculated. I think it would be clarified if you said "The ACP model and the CCNc model differ in how the supersaturation (S) is calculated" or something along those lines.

Response: Thanks for the suggestion. The text has been changed.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 8193, 2008.

ACPD

8, S5399–S5401, 2008

Interactive Comment