**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.

Anonymous Referee #2

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General comments

This paper presents measurements of the growth rates of activating aerosols in two distinct field locations. Few of these types of measurements have been made in the past and they are relevant to aerosol indirect effects on climate. Additionally, hygroscopicity is quantified using a single-parameter formulation, and while this is not as novel as the growth rate measurements, it is useful information (particularly for organic-rich aerosols), and is more easily compared to other field and laboratory measurements. This study is therefore relevant to ACP and should be interesting to its readers. While this methodology has been performed in the lab previously, this is the first time it has been utilized on ambient particles, and thus it nicely complements Shantz et al (2003).

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Interactive Discussion

Discussion Paper

The conclusions are substantial, particularly regarding the difference between organic aerosols in marine and forested settings. I also found that the methodology was sound, and the presentation clear. I recommend that it be published in ACP after the following points are addressed.

Specific comments

Was the lowered surface tension (as described in Table 1) used when κ 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 (κ) 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 κ.

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).

Pg. 8214, lines 1-4: It seems to me more likely that κ is less than 0.9 because the aerosol is not really 100% H_2SO_4. Any neutralization, even if the aerosol is still mostly acidic, would lower κ.

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.)

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.

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

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.

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