

Interactive comment on “Applying the CPCB setup to study the hygroscopicity and composition of freshly-formed 2–9 nm particles in boreal forest” by I. Riipinen et al.

I. Riipinen et al.

Received and published: 1 April 2009

We thank Dr. Birmili for his insightful and constructive comments that we think will help us improve the revised manuscript. Our detailed answers to the comments are as follows:

Specific comments:

1)[...] *I suggest to add a small chapter 2.1.4 that summarizes some of the above mentioned key properties of the aerosol inlets*

We will add short descriptions of the inlet structures of the DMPS, NAIS and CPCB setups in revised version of the manuscript.

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2) *I am missing an explicit statement on the reference instrument regarding the CPC efficiency calibration. Probably an electrometer? So please indicate which type, model, and sampling flow.*

The reference instrument used in the calibrations was a TSI electrometer, model 3068. The flow rate in the calibrations was 3.7 lpm. We will add this information to the revised version of the manuscript.

3) *The CPC efficiency curves seem to change with time, although not to an extreme extent (Figs. 2 and 3) [...] What I am asking you is to try to estimate the propagation of error in the CPC efficiency curve into the D50 that is determined later for the ambient aerosols. [...] A similar sensitivity study would be desirable in relation to the DMPS data.*

We will add an estimate on the uncertainty related to the drifting of the CPC detection efficiencies during the field experiment. It should be noted, however, that the TSI 3010 needed to be changed during the field campaign, so that the detection efficiencies reported for this instrument before and after the campaign refer actually to two different individual instruments. We think, however, that this uncertainty is remarkably smaller than the uncertainty introduced by the highly varying ambient conditions and the assumptions made on the activation phenomena taking place inside the CPCs during the field study. This uncertainty is unfortunately very difficult to quantify. Because of this ambiguous uncertainty we decided to report median values of the ambient cut-off sizes and effective growth factors. We will also add discussion on the uncertainty in the D50 values resulting from the uncertainty in the concentrations measured by the DMPS and the NAIS to the revised manuscript.

4) *Were D50 cut-off sizes determined for completely soluble or organic particles?*

Completely insoluble silver particles were used in the calibrations presented in this study. Petaja et al. (2006) and Mordas et al. (2008) present calibrations with varying particle composition.

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5) [...] Please clarify which observations in the NAIS plots probably correspond to charger ions, and which not

In the sizes close to 2 nm the NAIS charger ions interfere with the signal of the ambient particles. This does indeed introduce a significant uncertainty to the analysis of the data. However, the charger ions typically form a mode which increases towards small sizes, and at least in room air, does not typically extend to sizes above 2 nm (Asmi et al., 2009). In Kulmala et al. (2007) it was actually argued that the "shoulder" in the smallest mode (the most of which is charger ions), was implying the presence of an additional cluster mode around 1.5 to 2 nm. This argument has been - at least qualitatively - verified by the measurements by Sipila et al. (2008) and Lehtipalo et al. (2008). Also, in case the charger ions would affect the particle concentrations detected by the NAIS around 2 nm, we tried to minimize this bias by comparing the ratio of the concentrations shown by the two CPCs instead of looking at absolute numbers. However, it is of course possible that some of the concentrations of 2-4 nm particles as detected by the NAIS contain signal from the charger ions. We will add a sentence clarifying the charger ion contribution to the revised manuscript.

6) *The "Results and Discussions" section would be much more readable if it were divided into subsections using appropriate headers.*

We will clarify the section by, for instance, adding suitable subsections.

7) *Please expand the conclusions section. Currently, this includes only very scarce "conclusions". Here one would like to learn more details on how well you can actually discriminate totally soluble particles from organic particles, ambient particles (mixture of all), and insoluble particles.*

We agree, and will expand the "Conclusions" and add more discussion on the extent of which the data can be used to find implications of particle composition at these small sizes.

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Formal issues:

Title: The term "CPCB" should be spelled out, because it is not an everyday term. The word "setup" is superfluous. "Hygroscopicity and composition": 1) Hygroscopicity measurements are commonly associated with a humidification of particles below supersaturation. Therefore the term "hygroscopicity" may be misleading. An alternative may be "activation property", or "water affinity". 2) How confident are you that you are really looking at (chemical) composition? [...]From a proper study of "chemical composition" I would also expect the presentation and/or discussion of corresponding compound calibrations of the instrumentation, which seems to be not the case. I therefore recommend to rethink the title in view of all these points.

We will modify the title according to these suggestions to the following form: "Applying the Condensation Particle Counter Battery (CPCB) to study the water-affinity of freshly-formed 2-9 nm particles in boreal forest". With respect to CPC calibration with particles of different chemical composition, there are some relevant studies such as Petaja et al. (2008) and Mordas et al. (2006) that should give some indications on the effect of particle composition on the CPC detection efficiencies. Calibrations with small enough organic (or even more so mixed organic-sulphate) particles would naturally be highly desirable but left as a subject of a future study.

p. 14895, l. 23: use simple past instead of present perfect.

We will modify the revised manuscript as suggested.

Same page, l. 25: Use the definite article like "The proposed atmospheric..."

We will modify the revised manuscript as suggested.

p. 14896, l. 7. Maybe the authors can include another relevant citation on the subject (HAFEX), where statistically relevant annual cycles of newly formed particles, their growth rates, and calculated product rates of VOCs are shown (<http://www.atmoschem-phys.net/3/361/2003/>).

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This reference is definitely relevant to this work and we will add it to the revised manuscript.

p. 14897, l. 29: Insert a reference (no citation) to the TSI Inc., with the location of their headquarters.

We will add this reference.

p. 14898, l. 12: Delete the citation Kulmala and Hari (2005) here since it gives the misleading impression that the CPCB data was collected during those authors' work.

We will modify the revised manuscript as suggested.

p. 14898, l. 20: "Material" (uncountable noun) should be singular.

We will rename the Section simply as "Methods".

p. 14898, l. 23: Replace "were made... match" by "were matched".

We assume that this comment refers actually to p. 14902, l. 23, and will modify the revised manuscript as suggested.

p. 14901, l. 14: The NAIS "was" or "has been" developed.

We agree, and will modify the revised manuscript accordingly.

p. 14902, l. 12. Misleading sentence. I guess you mean something like "Because of minor drifts... concentrations ratios were normalised to a ratio of unity, based on night-time measurement values when the smallest particles (<10 nm) were overwhelmingly absent."

We agree, and will modify the revised manuscript accordingly.

p. 14909, l. 9: Clarify what you mean by "role of organics": Mass fraction of the particles, or maybe mass fraction of the condensing material?

To be exact, we actually mean the "contribution of material that is less hygroscopic than

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pure ammonium sulphate". We will modify the revised manuscript to clarify this point.

Figure 1: The sketch is irritating because some lines signify real aerosol sampling flows, while others imply rather associations with labels. Try to keep the real aerosol flows as lines but disconnect the other labels. "Aerosol in" could be replaced by "Ambient aerosol", "x nm" by something like "Dc =x nm".

We will modify the figure as suggested.

Figs. 2 and 3 might be combined as Fig. 2a and b.

We will modify the figure as suggested.

Figure 4 could be made much clearer. Also add tick labels for particle diameter axes.

We will add ticks to the particle diameter axes.

Figure 5: Avoid axis labels that span multiple graphs.

We will add separate axis labels for all the axes.

All Figures: Try to use always the same labels, for instance either "TSI 3785", or "CPC 3785" etc. Currently the labels differ quite a lot.

We will make the labels consistent.

Figure 5: Labels are misleading, use, for example, "DMPS (simulating CPC 3010)".

We will clarify the labels.

Figure 7: Ticks for the diameter axis are sadly missing. Also add tick labels at the bottom and the top of that axis.

We will add the ticks and the tick labels.

References:

Asmi, E. et al. (2009), Atmos. Chem. Phys., 9, 141-154.

Kulmala, M. et al. (2007), *Science*, 318, 89-92.

Lehtipalo, K. et al. (2008), *Atmos. Chem. Phys. Discuss.*, 20661-10685.

Mordas, G. et al. (2008), *Aerosol Sci. Technol.*, 42, 152-158.

Petaja, T. et al. (2006), *Aerosol Sci. Technol.*, 40, 1090-1097.

Sipila, M. et al. (2008), *Atmos. Chem. Phys.*, 8, 4049-4060.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 8, 14893, 2008.

ACPD

8, S11966–S11972, 2009

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