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Interactive comment on “Cloud condensation nuclei closure study on summer arctic aerosol” by M. Martin et al.

Anonymous Referee #1

Received and published: 9 May 2011

The paper presents results of a closure study on CCN concentrations measured during ASCOS. It is shown that, for the most part, CCN concentrations were overpredicted at the two highest supersaturations. If I understood correctly, there was a several day period when the opposite was true, i.e., CCN concentrations were underpredicted at the two highest supersaturations (233.9 – 238.1). These results are explained by invoking either the existence of small particles that contain insoluble organics or small particles that are more hygroscopic than is assumed. The paper would be more complete if it included relevant data that could add insight to the results. For example, were there multi-stage impactor samples that could be used to assess the composition of the smallest particles? Did the HTDMA measurements extend to < 100 nm to reveal hygroscopicity information about the smallest particles? I understand that further analyses are planned (comparison with the HTDMA data, case studies considering meteorology,

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etc.) but I think this paper could benefit itself from these type of analysis. In particular, data that would provide further information on the composition and hygroscopicity of the aerosol as a function of diameter would greatly strengthen the paper.

Abstract, lines 24 – 25: Not sure what is meant here by “. . .this is not unambiguous”

p. 8809, line 11: Do you mean “. . .if they have not grown larger than 1 um by the time they reach the OPC”?

p. 8815, line 5: omit “exemplarily”

Figure 2: What are the red triangles? They don't appear in the legend. Also, what do the dark blue triangles represent? If they are “considered data”, why don't they coincide with a specific supersaturation? This figure needs a more descriptive caption.

p. 8816, last paragraph: The aerosol was assumed to be internally mixed based, in part, on HTDMA measurements. What was the size range of these measurements? Did they extend to particle sizes < 70 nm, i.e., the size range not measured by the AMS (taking mobility vs. vacuum aerodynamic diameters into account)? If so, is there evidence of a separate organic mode at the smaller sizes as has been previously reported (e.g., Zhang et al., Environ. Sci. Tech., 39, 4938–4952, doi:10.1021/es0485681, 2005)? I am wondering if the lack of closure at the highest supersaturations is due to an externally mixed insoluble organic aerosol or if it was truly an internal mixture?

p. 8817, lines 14 – 23: Please add more explanation to the difference between an internal mixture with an insoluble core and the assumption by Lohmann and Leck of an activated Aitken mode with a surface active fraction.

Figure 6: It would be helpful to color the data points by supersaturation to illustrate the outliers at the two highest supersaturations.

p. 8819, first paragraph: It is stated that the period from 233.9 to 238.1 has underpredicted CCN concentrations at the two highest supersaturations which is in conflict with data from other periods. Is there any evidence from the HTDMA data that the smaller

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particles were more soluble during this period? Can impactor data be used to probe the difference, i.e., by looking at the composition of particles on the smallest stage?

p. 8820, line 4 and throughout: data is plural so this should read “. . .data shown here were. . .”

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 8801, 2011.

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