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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hygroscopicity and composition of Alaskan Arctic CCN during April 2008" *by* R. H. Moore et al.

Anonymous Referee #2

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General comments:

Moore et al. provide a detailed characterization of CCN measured in the Alaskan Arctic during the ARCPAC study and examine trends in CCN properties. Given the accelerated rate of climate change in the Arctic and need to understand cloud feedback, this study is very important. Recently, Mauritsen et al. (2011, ACP) suggested that "future research should be conducted to quantify the anthropogenic contribution to the natural Arctic background aerosol"..."given the strong observed aerosol indirect effects in the Arctic region." Given the importance of this work, the results of the CCN measurements during the ARCPAC should be discussed in greater context, including discussion of connection to additional previous Arctic research. In addition, increased incorporation of AMS and PALMS data into interpretation of the CCN data could be helpful; for

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example, externally mixed particle populations could be defined based on the PALMS data to examine CCN closure. In addition, the work by Hara et al. (2003, JGR) could be quite useful in considering springtime aerosol mixing state.

Specific comments:

Page 21790, Lines 13-14: Is a comparison to biogenic oxygenated organic aerosol relevant for this location and time of year?

Page 21790, Lines 14-15: Could mixing state, or other factors, be influencing the sensitivity of aerosol hygroscopicity to the O:C ratio?

Introduction: It may be more useful to only summarize springtime CCN measurements in the Arctic since the sources of CCN vary with time of year. Also, the introduction reads as a comprehensive overview of Arctic CCN measurements; however, several studies are missing, e.g. Zhou et al. 2001 (JGR), Leck et al. 2002 (JGR), Wylie and Hudson 2002 (JGR), Lohmann and Leck 2005 (Tellus), Mauritsen et al. 2011 (ACP).

Section 2.2, 1st paragraph: What collection efficiencies were utilized to adjust the AMS data, and how were they derived?

Page 21796, Lines 7-10: If the "unclassified" particles were consistent with the other particle types, why were then not identified and included in the analysis? What fraction of the total particles did the unclassified type comprise?

Section 3.1: It would be useful to the reader to provide sampling altitudes for each of the identified air masses. Also, it should be noted that the particle volume composition reported refers only to non-refractory material measured by the AMS. Was the Arctic boundary layer air mass type defined such that only time periods with depleted ozone were considered?

Page 21800, Lines 9-11: It would be useful to compare the observed average geometric mean diameter for the biomass plume to that observed previously in the literature.

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Page 21800, 3rd paragraph: At what altitudes was the nucleation mode present? To determine whether these particles were statistically significant, couldn't the raw data (rather than just averages) be examined further to determine if this phenomenon was real? How did the particle size distributions vary with altitude? Of particular interest would be a comparison between the particle size distributions in the background and Arctic boundary layer air masses at <500 m amsl. This discussion could be improved overall.

Page 21802, Lines 2-4: Could this observation be due to similar source/chemistry influences?

Page 21802, Lines 15-16: Mineral dust appears to be considered to be non-hygroscopic in this study; however, previous work (e.g. Twohy et al. 2009, GRL) has shown that mineral dust particles can act as CCN. Was the mineral dust observed via the PALMS instrument internally mixed with secondary species? What was the composition of the dust? This chemical information about the dust may give insights into its hygroscopicitiy.

Page 21802, Lines 19-22: The connection to the Arctic is unclear here.

Page 21803, Line 25: The surface tension of water is assumed in this study. However, it should be noted that previous work by Lohmann and Leck (2005, Tellus) found that the presence of surface-active organics was necessary to model CCN concentrations in that study. If a surface tension sensitivity study is not completed, justification of this assumption should be provided.

Page 21806: Why are O:C ratios not reported? Also, the discussion of Figure 8 should be clarified. The f44 discussion on this page and the following page may deserve its own section in the manuscript to improve clarity. In discussing the relationship between O:C ratio and hygroscopicity, it would be useful to refer the reader to laboratory studies of organic acids, for example.

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Page 21812, 2nd paragraph: The construction of a CCN closure scenario based on the PALMS data is a very good direction; however, only two particle types are really considered. Could a more realistic scenario be constructed based on a combination of the PALMS and AMS data? For example, treatment of dust and sea salt could be included. This extension be a great addition to the study here.

Technical comments:

Page 21790, Lines 5-7: It is stated that 4 distinct air mass types were observed, but only 3 are listed here.

Page 21790, Line 10: It does not seem necessary to state in the abstract that m/z 43 and 44 peaks from an aerosol mass spectrometer were used to determine that the organic aerosol was found to be well-oxygenated; the character of the aerosol could simply be stated here.

Page 21795, Line 26: This sentence should be reworded as the C-ToF-AMS does not volatilize refractory material; therefore, it is an overstatement to say that it "gives relatively little information".

Page 21796, Line 3: Is this a reflectron time-of-flight mass spectrometer? If so, this should be stated.

Page 21798, Line 3: Fix spelling of "transfer".

Page 21798, Line 20 & Page 21799, Line 1: "constitute" should be "constituted".

Page 21799, Line 18: Delete extra "the".

Page 21799, Last paragraph: It is suggested that this paragraph be moved to the AMS methods section (section 2.2). Also, provide justification for utilizing an organic density of 1400 kg m-3.

Page 21800, Line 2: "coexist" should be "coexisted".

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Page 21800, Line 12: Fix significant figures in reported uncertainty.

Page 21802, Line 1: Should “around” be “above”?

Page 21802, Lines 26-28: The relevance of this statement to the current project is unclear here.

Page 21803, Lines 3-4: This statement is confusing, and it’s relevance is unclear.

Page 21804, Lines 22-25: Provide references for these assumptions about the organics.

Page 21812, Line 20: Is this referring to AMS mass spectra? Please clarify.

Fig. 5: This is a very informative figure! What are the uncertainties in the PALMS number fractions?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21789, 2011.