

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

Anonymous Referee #3

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This is a paper about cloud droplet formation ability of aerosol particles in the high arctic, an area of high interest due to e.g. its climate sensitivity. As pointed out by authors, there is also not much aerosol-cloud data available for this remote region. I find the manuscript well worth publishing after consideration of the following comments.

I have some major issues with the paper:

1) The conclusions in the paper strongly depend on a closure concerning CCN number. It is thus very unsatisfactory that the discrepancy between total number from DMPS and CPC number is not discussed further. According to my experience, CPC numbers are much more reliable than integrating DMPS spectra. It would be interesting to see some comparison between CCN number at the highest supersaturation and DMPS number and CPC number, respectively. The maximum ratio CCN# : DMPS#

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and CCN#: CPC #should be one. This could give a hint for concluding which number to trust. I would also recommend a sensitivity test, were the DMPS concentration is increased to fit the CPC number.

2) The conclusion that the kappa value for the organics is close to one is based on the fact that this parameter setting gives smaller error than all others. But, since this parameter setting, that is really in the low hygroscopicity end, does not give a good closure one will have to consider errors or factors not included (see e.g. comment 1 above) that will change the results. When done, why could they not change enough for some of the other parameter settings to fit?

3) Throughout the paper one is waiting for the results from the H-TDMA data to be used. It is very frustrating to know that these data are available but not used. I understand very well that the group of authors wants to get more than one paper from all the efforts to take the instruments to the high arctic, but I think that it has to be handled in a better way. Ideally, I would have liked the two papers to be presented side by side as part 1 and part 2 on the same subject. A minimum is that it is clearly stated in the introduction, that H-TDMA data are not used.

Minor comments:

Concerning chemical composition: Wouldn't one expect the aerosol in this area to also contain NaCl? I know that the AMS cannot detect NaCl, but do you have any other analysis to support not including NaCl?

P8806 I9: "Closure was achieved. . ." sounds as a conclusion. One could use "Closure was tested. . .".

P8807 I 6-7: Check the language in this sentence.

P8808 I 10: "The RH of the sample flow was assumed to ". Was it just assumed or estimated based on temperature increase?

P8808 I 15: "diamter" should be "diameter"

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P8809 I 12-13: How do you know that there are no smaller droplets?

P 8810 I3: shouldn't the sheath to flow ratio be described as 13:1 and 10:1? Are the CCN numbers compensated for this flow ratio? Describe how the CCN number was calibrated. Could the numbers of CCN at the highest supersaturations be used to verify the CCNC number detection?

P 8810 I 10: replace "aerosol" by "aerosol particles"

P 8810 I 14 Are the diameters given as vacuum aerodynamic diameters? Please specify the diameter measure.

P 8810 I 14: How should we read that the AMS sample these particles with 100 % efficiency? Isn't the AMS sampling efficiency much below 100% and a subject of discussion?

P8811 I 1: This set up is normally not called a tandem DMPS (tandem: two after each other) but rather a Twin DMPS and it has two DMAs in parallel (not in tandem, I 3).

P 8811 I 6-7: How well do the two DMAs agree in the overlapping channels?

P 8814 I 24-26: Couldn't you get the ammonium to sulphate ratio from the AMS?

P 8814 I 28: Table 6 should be Table A1.

P 8815 I 15: The identification and excluding of outliers have to be discussed in more detail. Why are they excluded? Which are the criteria for excluding them? How big a fraction of the data is considered as outliers? Have you tested a regression including the outliers? Would that change the results?

P 8819: On line 12 a mean kappa value for the whole campaign is given and on line 20, a modeled kappa value is presented. How well could these kappa values, taken as constant values over the campaign, explain the CCN#?

Table A1: An impressive table and an impressive work running all these simulations!

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It would be interesting to include also the slope of CCN_{pred} vs CCN_{meas}. One could also consider including the size limits for the different supersaturations.

In several places a kappa value of 0 for the organics is interpreted as the organics being insoluble. Still, there is a difference between parameter settings 13 and 14 where the only difference is that in 13 80 % of the organic is water soluble with kappa 0 and 20% is water insoluble and in 14 all the organics is water soluble with kappa 0. How could this be?

Fig 7. One could look at a closure on the cases dominated by ammonium sulphate (kappa 0.61) (that could not be influenced by organics) and see if the closure is better. This could be a way to check the CCN vs DMPS number calibration.

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