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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Measurements of the relation between aerosol properties and microphysics and chemistry of low clouds in northern Finland" *by* H. Lihavainen et al.

**H. Lihavainen et al.**

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The paper of Lihavainen et al. presents results from the cloud and aerosol interaction experiment (PaCE) performed in Finland in 2004. This paper complement the previous study published by Komppula et al. (2005) and brings new information on the influence of different air masses, aerosol number size distribution and chemistry on the activation of aerosol particles in cloud. I think that the paper is suitable for publication after answering to the following points:

- p. 14108: My major concern is about the description of the inlet. The authors should bring more information on the separation between the interstitial phase and the cloud hydrometeors. In the present description, the authors are giving detailed description

on the length of the inlet, the flow rate but there is a lack of information on the main interest of the sampling line, which is the removal of the cloud phase. The authors mention a size cut of 7um but without giving any information on the size of the droplet population. Cloud droplets may have sizes smaller than 7um and can thus enter the system and thus contaminate the interstitial phase. The result would be a sampling of the bulk aerosol and not the interstitial phase, which would modify all the analysis.

As mentioned in the manuscipt there is a more detailed description in the paper by Komppula et. al 2005, the main removal of the cloud phase is made after the outside located vertical sampling line; "After this there is a T - connector, in which the sample air makes a 90 degree turn into the station building. The other branch, directing downward and sealed at the end, collects possible condensed water. This is also the point where almost all the particles larger than 10 microm are separated from the sample air by inertia"

The sentence at the end of the paragraph; A more detailed description of the sampling line can be found in Komppula et al. (2005) was moved to the beginning of the paragraph and text "and only brief description is given here" was added to it.

FSSP data was analyzed for the sizes and based on that following sentence was added to end of the paragraph Based on the FSSP data analysis the uncertainty caused by the cut off size is rather small, the median percentage of the cloud droplets below 7 microm was about 2.5 % (average 8.3 %).

- p. 14110: Ionic composition is measured in 2 size classes at the below-cloud site but they are compared to PM7 at the in-cloud site. How was the comparison made and what are the uncertainties?

The comparison is between aerosol and cloud droplets. The cut of diameter of the fog water collector actually means that it collects droplets larger than 7 microm. Aerosol phase is activated to cloud droplets and the cloud droplets are analyzed for ionic content. A sentence; The calculated cut-off diameter of the fog water collector was 7



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microm was rewrote; The calculated minimum collected diameter of the fog water collector was 7 microm.

See below about the uncertainties.

- p. 14110: What was the interest of sampling with the SDI? Why mentioning it if no data are presented?

SDI results are used to confirm the VI results and hence it is mentioned, also in the text it is said "The VI results were compared to the SDI results, and the measured concentrations were in good agreement." The clean conditions lead to long sampling times and more uncertainties and possibility of contamination. The analysis of the SDI results however did not bring any additional information for the context of this manuscript.

- p. 14111: The inlet at the below-cloud station is not heated. What is the influence of contamination by ice crystals breaking in the inlet?

There was a PM10 nozzle at the top if the inlet system and the station was always outside clouds. There were only little were wet snow on the ground. The possible contamination by ice crystals is highly unlikely and was assumed to be negligible.

- p. 14112: How often did you get negative activated fraction? And what was the magnitude? Were the size distribution spectrums negative? If yes these values cannot be removed since they balance the average concentration. This brings some question on the use of two sites, so far in distance from each other, to determine the activated fraction, as well as the influence of hydrometeors in the interstitial sampling at the incloudstation. Please clarify.

This happened at the low end of the size distributions, below 100 nm, since there the size distributions from different stations are usually very close to each other and particularly in the cases with low total number concentration. It should be kept in mind that the DMPS errors are usually already order of 10%. It was tested how this pro-



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cedure effect to DMPS derived cloud droplet concentration results when comparing DMPS derived results to directly FSSP measured concentration. The uncorrected vs. corrected total number of DMPS derived cloud droplets formed a line with a fit corrected=uncorrected*0.96 + 21. In this light of this analysis the effect is greatly within the error limits of DMPS systems. Text was slightly changed and one sentence was added: If the subtraction resulted in a negative value, the value was set to zero to avoid unphysical situation. The effect of this procedure to total number of activated cloud droplets was tested and it falls greatly within uncertainties in the DMPS systems themselves.

The size distribution were not negative.

- p. 14112: Indicate the detection limit for the measured ions and carbon fractions.

The detection limits for ions, OC and EC were added to the text. However, for aerosol samples they were calculated slightly differently from the method mentioned in text. Detection limits for the chemical species were calculated as three times the standard deviation of the blank samples, which is more accurate method than that using only the mean of the blanks. The detection limits for OC, EC, methanesulfonic acid, chloride, nitrate, sulfate, oxalate, sodium, ammonium, potassium, magnesium and calsium were 48, 36, 1.27, 0.27, 0.14, 2.8, 0.17, 0.29, 0.25, 0.13, 0.44 and 2.1 ng/m3, respectively.

- p. 14118, line 13: Why is Reff decreasing with decreasing concentration?? Figure 6 is showing the opposite, as expected, please clarify.

Good point, there is a misprint, the sentence was changed; As one might expect, Reff was larger for higher values of LWC and decreased clearly with the increasing total aerosol number concentration for each LWC category.

- p. 14119: Why mentioning MSA and Oxalate since it is not used anywhere? I would suggest removing this sentence since it does not bring any additional information.

Mentioning them means that they were analyzed and the concentrations were ex-



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tremely low, there is information anyway so we feel that the sentence should stay.

- Table 3: How can Ca2+ be higher for the fine mode than for the coarse mode?

Referee is right that it is very unlikely that calcium concentration is higher in fine mode than in coarse mode. The high concentration of calcium in 19-21 October in fine mode is probably due to the contamination of the sample.

- Table 3 and 4: I would suggest indicating in the caption at which sites the data were collected.

Added

- Table 4: Replace ionic components by nomenclature, Cl-, NO3- and others.

Done

- Figure 1: I would suggest using colors for the graph.

Taken in consideration but there are so few data sets that we felt it is unnecessary.

- Figure 4: I would suggest just explaining the results of Figure 4 and not showing it as a separate graph since Figure 4 and 5 are quite similar.

There is a point of showing both figures. In different studies there has been measurements of either total number concentration or accumulation mode. The number concentration in the accumulation mode correlates better with the number concentration of cloud droplets than total number concentration ($>$10 nm) but in some cases the contribution of particles smaller than accumulation mode particles is not negligible. In this light is good to know also the total number concentration. Visualization of the results of table 1 and 2 serves also as a purpose.

- Figure 7: What are the error bars on this graph? Percentiles and errors are different. Please clarify.

The bars in figure 7 are not really error bars, they are the 10th and 90th percentiles of



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the data. This is now clarified in the figure caption.

- Figure 8: Use color in addition to the patterns. The figure is not clear as it is.

Figure changed to color.

- Figure 9: What are the uncertainties on these measurements particularly for the fog samples considering the huge variability between aerosol and in-cloud samples?

The absolute uncertainty of the instrument is difficult to quantify. In this work, field blanks were taken, and a detection limit, namely 3times the standard deviation of field blanks, was determined. As can be seen the detection limits are in some cases quite high, and the absolute concentrations of this data should be used with great care. This is also stated in the text. The huge variability is in some part also due to the different collection times of the samples. The aerosol samples take an order of magnitude longer collection time, and from that data it is not possible to see temporal variations as from the cloud water data. Furthermore, the water solubility of particles play a role in the variability of the samples.

Technical corrections: - Replace "Air craft" by "Aircraft" throughout the manuscript. - p. 14107: Change "in Northern from" by "in Northern Finland from". - p. 14108: Change "by Finnish Meteorological Institute"; with "by the Finnish Meteorological Institute". - p. 14113: Change "DMSP" by "DMPS". - p. 14115: Change "the overall activated fraction was was quite high" by "the overall activated fraction was quite high".

All done

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 14105, 2008.