

Interactive
Comment

Interactive comment on “The analysis of size-segregated cloud condensation nuclei counter (CCNC) data and its implications for aerosol-cloud interactions” by M. Paramonov et al.

Anonymous Referee #2

Received and published: 24 June 2013

In the present paper Paramonov et al. have presented the long term measurements of cloud condensation nuclei using size-resolved technique. The measurements were carried out at in northern Finland at SMEAR II station in Hyytiälä over the period for more than two years. The dataset reported appears to be of high quality specially since it is a long term data set and I believe that manuscript adequately meets the standard of ACP. I, however, have following comments, which authors need to consider before manuscript is finally accepted in ACP.

General comments:

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I am not sure if the manuscript adequately addresses the message in the title. I believe it is more so important when no chemical data is presented and direct link (may be through modeling) related to clouds under given environment is presented.

1. I am not quite convinced with what authors have presented about Dc during NPF and non NPF days. I believe it is well expected that NPF would have some effects on Dc; having said that I think if authors could have had the chemical composition data would have been interesting to see during this period. In addition instead of Dc authors might consider showing κ for the relevant diameter during NPF and non-NPF events.
2. As rightly pointed out by Referee#1 the average κ during Feb seems to be too high. Do authors believe that it has something to do with CCN calibration for correct effective supersaturation (too high supersaturation). This is more so important when the area under study is expected to be dominated by organics

Minor comments:

1. Part 2, Theory in the manuscript may be shortened as reader may be a-priory familiar to the theory.
2. There are some previous campaign based measurements from the similar location by other groups. Authors might want to compare the results; especially for that of hygroscopicity parameter.
3. Figure 7 is relatively complicated to follow. I would suggest that authors split it and present it in much simpler way.
4. I am not sure if authors wish to show Fig. 11 as it may be little confusing especially since it is difficult to explain why inactive fraction is negative in Jul. In addition in relation to Fig. 8b the highest kappa is in the month of Feb which is not consistent with lowest 1-MAF, which indicate something to do with supersaturation (calibration).
5. The diurnal plot of kappa and Dc for winter is not clear to me why these variations are like this; again if Feb has the highest kappa value it is not seen in the diurnal plots.

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Do authors have any explanation for it?

6. Some more discussion regarding the comparison between hygroscopicity parameter is required.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 13, 9681, 2013.

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