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Atmospheric Measurement Techniques Discussions

Interactive comment on "Inversion of droplet aerosol analyzer data for long-term aerosol-cloud interaction measurements" *by* M. I. A. Berghof et al.

Anonymous Referee #1

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Review of the paper " Inversion of Droplet Aerosol Analyzer data for Long-term Aerosol-Cloud Interaction Measurements " by M. I. A. Berghof et al.

This paper presents an improved version of droplet aerosol analyzer (DAA) and an inversion algorithm to process the DAA raw data. Moreover, first field results are presented.

The technical and scientific content and value of the manuscript is a relevant topic for applied fog and cloud research (cloud processing, aerosol cloud interaction) and is therefore in good hands within the scope of AMT. With minor changes (requested in

the general and specific comments below) it should be appropriate for publication.

The manuscript itself is in general well written with respect to language and structure. The instrument operation, the raw data processing and correction, and the uncertainty discussion is properly done.

General comments:

1. Although the presented approach and the presented case study results are convincing, it is a big drawback, that the results, which are achieved by the improved DAA and the new inversion is not validated by other measurements. The dry interstitial, the dry drop residual and drop size distribution and concentration, or at least some of these parameters should have been measured in parallel. This could have been easily accomplished by setting up an interstitial inlet in front of a DMPS (dry interstitial size distribution and number concentration) and an OPC (drop size distribution and concentration). The authors should comment why they passed on these opportunities to confirm their technology and why they think that was not necessary. To the reviewers opinion a good opportunity was passed up.

2. In similar aerosol cloud interaction studies, the term "residual" or "dry residual" particles is limited to the residues of evaporated drops and does not include the dried interstitial particles. In this work it is different and should be clarified in the beginning in order to avoid confusion of the reader.

3. With regard to 1. it should be emphasized that all uncertainties given in the text are not due to independent reference measurements of drops and aerosol particles, but only derived from the single instrument components and inversion uncertainties.

4. Since it is claimed that the DAA is improved for long-term measurements in the title and in the text, it would be nice to have a small text passage in the introduction or the conclusion which particular long-term experiments (including the specific objectives) are planned or already conducted.

Specific remarks

P. 1, L. 69: When discussing the cut-off diameters of counterflow virtual impactors, the reference Schwarzenboeck and Heintzenberg, 2000 (Journal of Aerosol Science, Vol. 31, 477-489) needs to be mentioned.

P. 2, L. 70-72: It is not correct that a counterflow virtual impactor collects interstitial particles. These particles are only pre-segregated by a CVI. Indeed a so-called interstitial inlet (pres-segregating the drops) is needed to collect interstitial particles for analysis. This text passage needs to be reworded.

P. 2, L. 136-137, 154-155: It is not completely clear why two DMA's (1a and 1b) are used. Do they scan simultaneously (and are "only" needed to provide sufficient sample flow for the other six DMPS systems (2a – 2f)) or do they scan at different electrical mobilities in order to improve the time resolution? Please comment.

P. 7, L. 425-440: The authors give a number of total particle number concentration of 515 cm-3 for sizes up to 563 nm. Even though larger particles are present too, it is doubtful if the given particle number can be consistent with a PM2.5 mass of 10 to 20 μm/m3 given by the authors. This should be commented. As already mentioned above a comparison measurement at least for drop concentration would have been meaningful, like for example in Mertes et al., 2005 (Atmospheric Environment, 39, 4233-4245). As an example much higher drop concentration of several hundred drops per cm-3 were found in this study comparable to many other investigations in similar kind of clouds. The same is true for the total particle number concentrations which are typical 2000 to 3000 cm-3 in these earlier studies. This differences in drop and particle concentration should be discussed or at least mentioned.

P. 7, L. 441 - 454: The scavenging ratio or at least the 50% activation diameter is consistent with other studies besides Martinsson et al. 1999. S oit would be of advantage to mention them here, like Hallberg et al. 1994 (Journal of Atmospheric Chemistry, 19, 107-127), Mertes et al. 2005 (Atmospheric Environment, 39, 4247-4256), Svenningson

et al. 1997 (Atmospheric Enviroment, 31, 2463-2475).

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