Atmos. Chem. Phys. Discuss., 7, S7721–S7723, 2007
www.atmos-chem-phys-discuss.net/7/S7721/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Optical particle counter measurement of marine aerosol hygroscopic growth" *by* J. R. Snider and M. D. Petters

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Received and published: 18 December 2007

We thank reviewer #2 for the review and constructive criticism. Below we include reviewer #2 comments and our reply. In those instances where the text has been revised, we also attach the revised text.

Perhaps the most interesting aspect of the paper is the novel way of determining the view volume of the FSSP-300. However, its description and its rationale are inadequate. The reason for adjusting the active laser area till predicted Poisson frequency of zero count-rate equals the calculated one needs to be explained. Why is this a better way of determining the active area than simply equating concentration of particles in the overlap range?

Please see our response to the related critique from Professor Baumgardner. In the

revision we make the point that these two approaches yield nearly the same project-averaged AF. One final comment: The AF we derive is average=0.034+-0.006 mmˆ2. What we wrote in the original manuscript was average=0.034+-0.060 mmˆ2. We apologize for the typographic error in our representation of the standard deviation of the sample area.

It seems strange that measured true air speed is not used in determining the view volume of the FSSP-300. Why not? How robustly does the 110 m/s speed used represent the actual speed of the C-130?

True air speed variability, expressed as the ratio of standard deviation divided by the average, ranged between 0.021 and 0.004 for the seven above-cloud averaging segments. The variability is small so the segment-averaged true air speed was used in Equation 1. For all other calculations, the 1 Hz values of true air speed were used. Also, the value TAS = 110 m/s is representative of all research portions of the DYCOMS-II flights, to within plus or minus a few percent. Also of relevance to this question, the NetCDF variable name we used for true air speed is TASHC. This is the one recommended by NCAR (http://www.eol.ucar.edu/raf/Projects/DYCOMS-II/QA.html).

The description of the discrepancies between particle concentrations measured inside the aircraft cabin and the wing probes is interesting, but not relevant. Although the cause of the discrepancy is unknown, and in the worst case would mean the analyses were done on data having uncertainty in concentration, the results here would only be affected if sizing of the particles by the wing probes were in error. Unfortunately, discrepancies in the size distributions from the two instruments suggest there may have been some calibration issues.

All discussion of the cabin-mounted OPC (Lasair) is removed from the revised manuscript.

Instead of detailing the discrepancy in concentration between measurements inside and outside the cabin, it seems more space may be dedicated to what effort was ex-

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pended to calibrate the instruments and minimize sizing errors.

Because we were not involved in the calibration of the OPCs, we do not have anything to add other than what we presented in the submitted manuscript. In the submitted manuscript, and in the revision, we make the point that OPC calibration is an area of concern, as we discuss on p. 12386 (top paragraph) and on p. 12401 (both page numbers refer to the original manuscript).

Page 12388, lines 17-18: Typo! ‘‘Wind-mounted” probably meant to be ‘‘wing- mounted”.

Thanks. This sentence is in one of the paragraphs describing the Lasair. It is removed from the revised manuscript.

Page 12390, line 28: ‘‘vide intra”? Can the meaning of this be expressed in English?

We have corrected this. Here is the revision:

The effect of the anticipated refractive index shift, from n=1.59 to n=1.41 (Table 2), is quantified using an optical-to-actual diameter ratio which we estimate to be 0.75 based on measurements reported by Stolzenburg et al. (1998).

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12381, 2007.

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