**Atmospheric
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Discussions**

Interactive
Comment

Interactive comment on "In-situ measurements of tropical cloud properties in the West African monsoon: upper tropospheric ice clouds, mesoscale convective system outflow, and subvisual cirrus" *by* W. Frey et al.

W. Frey et al.

freyw@uni-mainz.de

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The authors would like to thank Darrel Baumgardner for his helpful comments and suggestions. The concerns raised by the referee are discussed below and have been incorporated in the revised version of the manuscript. Issues related to shattering and performance of the cloud particle probes are discussed in a separate reply titled "On the issues of instrument performance and shattering artefacts for the FSSP and CIP" in order to combine with the comments by Grant Allen and the other referees. The introduction and summary have been shortened and improved in the revised manuscript

following the referees' suggestions.

Page 753, Line 7, "From this brief discussion it becomes clear that many open questions remain. . ." Yes, but which ones are addressed by this study?

Valid point. The introduction has been shortened considerably and the description of the study's purpose has been more focused on the "delivered data" in the revised manuscript.

Page 755, Line 3. How are the size spectra from the FSSP and CIP combined, i.e. are the concentrations in the overlapping sizes averaged, smoothed, etc.?

The size distributions from the FSSP are simply plotted on top of the CIP size distributions, i.e. the first CIP size bin actually extends down to sizes of $25\,\mu$m but are only displayed down to $29.2\,\mu$m. No smoothing has been applied to the size distributions. The details and an example are supplied in Section 3.2 of our separate reply on the instrumental issues. We apply no smoothing or running because we believe it is the most transparent way for a reader to present the data in an un-altered way including error bars. As a consequence of our treatment few data were rejected and excluded from the analyses as described in Section 3.3. of the separate reply.

Page 755, Line 6. The Baumgardner et al., 1992 paper was for a FSSP-300, not 100, and although many of the uncertainties that are discussed in that paper are true for both models, there are other papers more pertinent to the FSSP-100 as the two have different size ranges and define their sample volumes differently. That being said, nothing is discussed in this paper about corrections for electronic roll-off (this is discussed in the Baumgardner et al, 1992 reference) or for aspherical particles. Given that one of the co-authors of this paper (Borrmann) published a paper on the impact on sizing from OPCs of aspherical particles, I think it might be appropriate to discuss this correction and if it is not being done, what would be the subsequent effect on the size distribution shapes and derived IWC.

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Our statements in the manuscript only pertain to the scattering geometry which is the same for the models 100 and 300. We did use the T-matrix method but further reduced the number of size bins almost by a factor of two to avoid cross sensitivity and ambiguities due to the non-spherical shape. This is described in more detail in our separate reply in Section 3.1.

Page 756, Line 20. The use of interarrival time...

This is addressed fully in the separate reply for shattering (Sections 2.2 and 2.3.).

Page 757, Line 1. "This lies within the instrumental uncertainty of the CIP." Can this be clarified? What is the uncertainty being referred to and where is a reference for this uncertainty?

For the de Reus et al., 2009, paper the uncertainties were thoroughly assessed. There it is said that the uncertainty of the CIP for the measured number concentrations is mainly determined by the error of the sample volume, which was estimated to be 20%.

Page 757, Line 16 and throughout. Only as a suggestion, I think it might be better to use mg/m3 rather than g/m3 given the very small values and the awkwardness of using scientific notation in this case. The same might also be considered when describing the number concentrations from the FSSP and CIP, i.e. number per liter rather than number per cubic centimeter.

If the editor likes us to do this we can change the values accordingly. When reading the many papers from the literature we found ourselves converting units many times back and forth and decided to provide our data in g/m^3 and cm^{-3}, i.e. units we found commonly used, even if a bit cumbersome. This is easy to change though.

Page 757, Line 25. Are the 6, 10 and 15 nm 50% cut points?

Yes, these are the sizes at which the particles are detected with 50% efficiency in a curve of detection efficiency versus particle size. The particle diameter limits were determined by laboratory experiments at the IfT (Leipzig) Condensation Particle Cali-

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bration Facility. The experiments and the characterisation of COPAS are described in detail by Weigel et al. 2009.

Page 758, Line 1. Why was the 250 C temperature chosen? What non-volatile particles would be expected to remain- BC, dust and some organics?

Initially, Jim Rosen published a temperature of near 170°C for evaporation of all H_2SO_4 and water from stratospheric aerosols (Rosen, 1971) from his early balloon borne experiments. We chose a pre-heating temperature of 250°C to additionally volatilise the main component of upper troposphere/lower stratosphere aerosol, namely H_2SO_4-H_2O and possibly HNO_3. At this operation temperature and over the pressure range 70–300 hPa the aerosol pre-heater volatilises more than 98% of the H_2SO_4-H_2O particles. This was determined by Weigel et al., 2009, also at the IfT (Leipzig) CPC calibration facilities. For these tests the real atmospheric low pressures were used together with laboratory generated sulphuric acid droplets. Extra care "was applied" because we knew from experience with the University of Denver CPC on the ER-2 that such measurements are difficult. Possibly a lower temperature closer to 170°C would have sufficed, but we set the wall heating of the COPAS inlet to 250°C because we wanted to be sure that in the centre of the inlet flow temperatures still are high enough. From our Aerosol Mass Spectrometer measurements we know that BC, desert dust, soot, metals etc. do not evaporate even at temperatures above 600°C. The organics, which do not evaporate, are exactly the residuals we are interested in. Dust has not been observed at these altitudes (see e.g. the Murphy et al., 1998, Science paper), that is over South America. Here, over West Africa dust particles may have played a role. Certainly, mineral dust is not volatilised by COPAS. From our measurements we can not tell what kind of particles are left after heating.

Page 759, Line 4. Can more be said about the criteria for identify NPF?

In background conditions the N_6 and N_{15} are almost equal because freshly nucleated particles with sizes below 15nm quickly coagulate. Thus under no-nucleation event

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conditions the COPAS channels with cut-offs of 6 nm, 10 nm, 15 nm all measure the same concentrations within the experimental errors. Considering a measurement uncertainty of 15% for each channel (see the Weigel at al., 2009, COPAS characterisation) an NPF in principle could be identified, if the N_6 concentration multiplied by 0.85 exceeds the N_{15} concentration multiplied by 1.15. The 0.85 and 1.15 are for the most part determined by counting statistics. However, in most NPF cases the difference between N_6 and N_{15} is much more than 100 particles per cm^3 so that NPF is unambiguously identified. The NPF identification is described in more detail in the new paper by Weigel et al., 2011, which currently is on the ACPD discussion page.

Page 759, Line 20. What is the in-cloud accuracy of temperature measurements and how do these uncertainties propagate into the accuracy of the derived RHi?

The temperature is measured with a relatively large uncertainty of 0.5 K. The resulting uncertainties for the RHi measurements are 12-17% (see for details Krämer et al., 2009).

Page 760, Line 8. Can you explain why potential temperatures are being used in the vertical profiles and to describe vertical location rather than pressure or height?

Since thermodynamics determine the height of the outflow region, it is instructive to use the potential temperature as altitude equivalent. Thus, for better comparison from a thermodynamical viewpoint we use the potential temperature. Due to dry adiabatic displacements in the waves near the large cloud systems pressure altitudes may vary while potential temperature is more or less constant. (One might argue that moist adiabats should be used but at the higher altitudes in the UT the low water contents do not strongly alter the air temperature anymore.) For transparency, however, the pressure altitudes of the outflow events are indicated in the vertical profiles as shown now in the revised figures, time series, and size distribution plots, so at least the information is there.

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Page 762, Line 1. When using log normal distributions, it is normally the geometric rather than the modal diameter that is used. Have these distributions been fit to the measurements with good success?

The attempt by a non-native speaker to build the adjective of the word 'mode' has lead to the unclear use of 'modal' and the supervisor did not recognise this. ;-) Meant was the "mean mode diameter". Yes, the size distributions have been fitted with good success. Some examples of such fits are given in Section 1.4 of the separate reply on the instrumental issues.

Page 762, Line 3. "Comprehended" should be change to "summarized" or "listed".

done

Page 766, Line 3. "Or some of the newly formed 6nm particles have already been lost to the surfaces of the preexisting back ground particles." Could you please clarify what is meant here by "lost". If this means scavenged by coagulation, I don't think the concentrations are large enough for frequent collisions at this altitude. Are they evaporating?

The particles are not evaporating (see pre-heating discussion above). The quantification of losses due to coagulation inherently is subject of the manuscript by Weigel et al. 2011. It seems these small particles form in NPF despite the existence of cloud particles which in principle are capable of scavenging them. A result of the Weigel at al., 2011, study is, however, that if cloud particles with sizes above $2\,\mu$m are present at concentrations above $2\,\text{cm}^{-3}$, then NPF are "quenched" as losses – of condensing gas molecules – to existing surfaces are too fast for new particle formation.

Page 771, Line 24. How do you interpret this Chi-square value, i.e. at what level of significance? Why was an exponential function used?

Good point. Several fit functions have been tested (e.g. lognormal). However, the

best fit result was achieved by the use of the exponential fit. For the non-linear least squares fitting the Levenberg-Marquard algorithm was applied to minimise the chi-square. Prompted by the reviewer's comment a more detailed look into the confidence limits and the corresponding chi-square values led us to change the manuscript here because the confidence levels turned out to be unrealistically high. Thus, we replaced the chi-square test with a linear regression on the logarithms of the $n^*(D_p)$, and we report the Pearson correlation coefficient r = -0.89 and r^2 = 0.79, respectively.

Page 775, Line 18. I don't think specific dates, in this context, need to be included in a summary.

Dates are removed

Page 775, Line 21. Are particles that touch end diodes removed from the analysis and if so how is effective array width defined? If not, then the upper size range of the CIP extends beyond 1.6 mm if you are reconstructing some of the crystals.

Partial images are reconstructed (indicated in Table 1 of the manuscript) and effective array width is calculated adopting the Heymsfield and Parrish (1978) method. We changed the formulation in the manuscript to: "(i.e. the maximum size of the CIP images across the diode array)".

Page 775, Line 27. In my opinion, summaries should not refer to previous figures.

The reference is set to a new figure which is shown to summarise the measurements. It is a bit unconventional to provide a new figure in the summary, but here it seems well in place to us.

Page 775, Line 29. "Underpin" should be "highlights" or "underscores".

changed

References for this reply:

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