

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.**

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The authors would like to thank Anonymous Referee #4 for her/his helpful comments and suggestions for improving the manuscript. We discuss the issues raised by the referee here, and we have incorporated several changes into the revised version of the manuscript. The issues related to shattering and instrument performance of the cloud particle probes were voiced by several reviewers. For this reason we discuss these in a separate “common” reply in order to avoid redundancies. Since these instrumental issues are at the core of the paper and our discussion includes several new graphs, we

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suggested to the editor to include this separate reply in a suitable format in the online material accompanying the paper, if accepted.

**Comment.** “**First, small ice crystals are typically non-spherical meaning that their sizes are not readily determined by FSSP (the Mie theory algorithm used to derive size from the amount of forward scattering assumes spherical particles) Therefore, one cannot readily derive the volume backscatter ratio as it is dependent on particle size. The FSSP could at best provide an estimate of total small crystal concentration if it was not affected by particle size.**”

Reply: In principle this is true. However, as detailed in Section 3.1 of the separate reply we decreased the size resolution to a much lower number of bins than possible from both, the T-matrix binning or the Mie theory based binning. With the estimates of the backscatter ratios from these coarse size distributions the agreement with the measured backscatter ratios is remarkable. This is shown in the publication by Cairo et al., 2011, who used concurrent measurements of FSSP/CIP and their MAS (backscatter type) instrument.

**Comment:** “**Second, . . . , there are many problems associated with the use of CIP data for diameters less than about 150 micrometers.**”

Reply: This comment is specifically treated in Sections 2.1 (and 1.2) of our separate reply on the instrumental issues.

**Comment:** “**. . . there are many out of focus particles that appear in the CIP imagery as donuts. Have efforts been made to remove and resize these particles?**”

Reply: The out of focus particles are size corrected as described in Korolev et al., 2007 (see Table 1 of the manuscript). The imaged particles were mostly quasi-spherical, and thus, this correction is applicable. In general, a lot of visual inspection of the recorded images by the first author was performed in order to quality-control the treatment of the images by the software.

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**Comment:** “Third, the paper of Korolev et al. (2011, BAMS, in press) suggests that algorithms such as those of Field et al. are not able to effectively remove all shattered particles from standard optical array probes. It may be possible that in some instances, such as the flights through subvisual cirrus, that shattering is not a problem. However, in the developing MCS, with maximum particle sizes of 1.5 mm, shattering most likely would be a problem.”

Reply: This issue is dealt with in Section 1.1 of our separate reply on instrumental problems. One never can be sure to exclude all potential shattering artefacts, even with the newer probes with particular designs. We believe that we performed state of the art analyses on this issue. Furthermore, to draw the reader’s attention to this, we additionally highlighted those size distributions where shattering might potentially introduce artefacts in significant amounts.

**Comment:** “If the authors were able to do some more thorough analysis to show definitely where shattering is a problem and where the data can be trusted, it might be possible to retain some of the data analysis in this paper (e.g., type of analysis found in Jensen et al. 2009 and Heymsfield et al. 2007 looking at how small crystal numbers are impacted by other factors).”

Reply: This is a helpful comment. In preparation for the reply we performed “Jensen-style” analyses. Two of the resulting figures are included in Section 3.4 of the separate reply on instrumental issues. Essentially our data well confirm the results from Eric Jensen’s analyses.

**Comment:** “If it can be demonstrated that the FSSP/CIP do capture all the data in the range from about 5-200 micrometers, then this paper would be acceptable for publication.”

Reply: We addressed this comment in Sections 1.2, 2.1, and 3.1 of our separate reply on instrument issues. With (1.) the decrease of the size resolution of the FSSP we tried to minimise cross sensitivities, with (2.) the rejection of data which show poor

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overlap between the FSSP and the CIP we tried to minimise shattering contamination, and with (3.) thorough error analyses e.g. of the sample volumes we provide conservative estimates. For (3.) we performed new laboratory measurements (of the kind Sara Lance from Boulder did for the CCP) on the FSSP sample area in 2010. The measured sample area agreed well to the one used for the calculations in our manuscript. Furthermore, intercomparisons of preliminary data from the RECONCILE 2010 campaign and test flights on Geophysica of the CCP (greyscale CIP with anti-shattering tips plus CDP) and CIP/FSSP-100 as deployed during SCOUT-AMMA indicate good agreement for sizes larger  $6\ \mu\text{m}$ . Thus, we believe we have done what is possible, and, where uncertainties remain we have appropriately pointed these out in the paper such that a reader can form independent opinions.

**Comment A: “Point 2. The paper analyzes 117 ice particle size distributions in the vicinity of MCSs, and based on these measurements develop a parameterization for modeling. This is not a statistically significant sample upon which a modeling parameterization can be developed. The authors, in fact, demonstrate this by showing that their data differ from some data that were obtained in other locations around the world. While it is acceptable to fit a function to the measured data for ease of comparing with other data sets, this should not be advertised as a parameterization unless a more statistically significant set of data are available.”**

and

**Comment B: Page 25, before Eq. 2. It is very difficult to say that this is a parameterization because of the limited set of data upon which it is based. The differences in this “parameterization” and past measurements suggests that there are insufficient data to capture all the differences in the size distributions that might be expected because of observations made in various locations.**

Reply: We report on the quantity (117 size distributions) to enable any potential user

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of the fits/parameterisations to get a feel of the quality. The other references, which we consulted, do not report their numbers of measurements. In principle, it is not really possible to derive A parameterisation for the size distributions from (such) clouds because of the large inherent variabilities, even if one had thousands of measurements. Maybe we should not speak of “parameterisation for modelling”. However, our data well fit into the framework of the CEPEX measurements and we believe they should be presented in a paper. In the revised version of the manuscript we rephrased the respective parts more carefully and avoided the term “parameterisation”.

**Comment: “Point 3. The basis for determining the temporal averaging for the measured size distributions is unclear. It is also unclear whether the authors have used a statistically significant set of data for each of the analyzed size distributions. Hallett 2003 describes a technique for ensuring that a large enough volume has been sampled to determine the number concentration for particles of each bin size. The authors should refer to this paper to ensure they are using significantly significant samples of data, especially for the small ice water contents where longer averaging periods are required (Hallett, J., 2003: Measurement in the atmosphere. Handbook of Weather, climate and water: dynamics, climate, physical meteorology weather systems and measurements, T.D. Potter and B.R. Colman, Eds., John Wiley and Sons, 711-720.) I am especially concerned with the statement on page 10 where the authors state “two second averages have been calculated for the CIP data.” This is not sufficient time to obtain a statistically significant sample of data. Later on (page 15) the authors state that the “measurements were performed with averaging times of 10-20 s resulting in good counting statistics for the majority of cases.” The authors need to clarify how they choose which averaging time (2, 10 or 20 s) and how they determined that they got good counting statistics for these cases.”**

Reply: We tried but could not get a copy of the quoted handbook, even though the reference thankfully was provided in detail by the reviewer. Concerning the averaging

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we probably gave ambiguous information. In a first step, the 2 second averages have been calculated for the time series only. Basically, the FSSP-100 recorded data to disc in two second intervals and CIP data have been calculated accordingly. The size distributions, however, have always been derived for longer time periods. In case of the MCS clouds 10 to 20 seconds were averaged depending on the number of the detected particles. For the SVC cases even longer averaging times had to be adopted. In the graphs of the size distributions the error bars are calculated for each bin including counting statistics and sample volume error. As can be seen these error bars are small enough for most cases/bins. This was achieved by adapting the averaging time interval lengths (often individually) to the number of particles encountered.

### Specific Comments:

**Comment: Page 6, “The formation of large sheets of SVCs . . . probably is a result of deposition freezing). This is speculation. Recommend removing from the manuscript.**

Reply: removed

**Comment: Page 10, The ratio of the third moment to second moment of a size distribution is not a way that the effective radius is commonly defined. Typically effective radius for ice particles is proportional to mass content divided by projected area or extinction.**

Reply: Yes, several studies define the effective radius as proportional to the ratio between mean particle volume and mean particle projected cross-section area. In our case the former is represented by the third moment the latter by the second moment of the size distribution, in terms of spheres of equivalent cross-section area, as defined by McFarquhar and Heymsfield (1998). They summarised a number of different definitions of effective radius for ice particles and, after testing, suggested that the proposed definitions are useful.

**Comment:** “Page 12, comments on comparing IWCs from in-situ hygrometer against the IWC from the size distribution. I don’t see how such a test can show anything about the role of shattered particles for a couple of reasons. First, there is a lot of ambiguity on how to estimate a three-dimensional volume or mass from a two-dimensional projected image of a particle. Past studies have shown that such uncertainties can cause variations by a factor of up to 5 in estimated mass. There is no information included on how mass is estimated from the size distributions (and there are a number of different techniques in the literature for doing this). Second, the size ranges where shattering is expected to make an impact on the ice crystal size distributions do not typically make large contributions to the total mass (they make much larger contributions to the total number and area). It would be much better to look at a bulk measure of extinction for investigations of the impact of shattering.”

Reply: The IWC for the hygrometer/particle probe comparisons have been calculated using the Baker and Lawson (2006) scheme. Further arguments for this comparison are given in the separate reply on shattering and instrument performance in Section 1.3.

**Comment on Page 16, “CEPEX parameterization clearly underestimates the concentrations for large particle sizes. . .”** It is not really that the parameterization underestimates the concentrations, but rather than the parameterization was designed for data collected under a different set of conditions. It would be better to state the comparison shows that lower concentrations for large particle sizes were found during CEPEX.”

Reply: Yes, of course; agreed. We rephrased this section and the reference to CEPEX more carefully.

**Comment:** “Page 24, What is the basis of stating that the clouds were subvisual? Was there a remote sensor that showed their presence?”

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Reply: From the mean microphysical parameters and the vertical extent of the SVC cases SVC2, SVC3, and SVC4 a rough estimate of  $\tau$  can be obtained following Garrett et al. (2003):

$$\tau = \Delta z \beta = \Delta z \frac{3CWC}{2\rho r_{eff}}, \quad (1)$$

where  $\Delta z$  is the vertical cloud thickness,  $\beta$  the extinction coefficient,  $CWC$  the condensed water content, which is in the SVC cases equal to the IWC,  $\rho$  the density of ice, and  $r_{eff}$  the effective radius. Since SVC1 has been probed on level flight, no estimate of the vertical cloud thickness can be made and thus no estimate about  $\tau$  can be provided. The estimation of  $\tau$  for SVC2, SVC3, and SVC4 results in 0.0055, 0.0102, and 0.0051, respectively. Since the IWC of SVC1 is smaller than the IWC of the other events it can be assumed that the optical thickness also is subvisual. In the revised version of the manuscript we point this out.

### References for this reply:

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Garrett, T. J., Gerber, H., Baumgardner, D. G., Twohy, C. H., and Weinstock, E. M.: Small, highly reflective ice crystals in lowlatitude cirrus, *Geophysical Research Letters*, 30, 2132, 2003.

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