Atmos. Chem. Phys. Discuss., 11, C623–C627, 2011
www.atmos-chem-phys-discuss.net/11/C623/2011/

Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #4

Received and published: 7 March 2011

This paper looks at in-situ measurements of ice crystal size distributions in tropical upper tropospheric/lower stratospheric clouds made during the recent SCOUT-AMMA campaign over West Africa. Using observations from a FSSP-100 and CIP on the Russian Geophysica, the authors determine statistics on the ice water content, effective radius, ice crystal concentrations and maximum crystal dimension for developing MCSs, mature, MCS and subvisual tropical cirrus. Based on their presented observational analysis, the authors give an exponential fit to the size distributions in order to establish a parameterization for modeling. The paper looks at an interesting set

of data collected during the SCOUT-AMMA campaign, and hence should probably be published. However, there are a number of problems with the manuscript (as detailed below) that should be fixed before the paper can be published.

1. The major problem with this paper is the potential impact of crystal shattering on the presented results. The authors do acknowledge the importance of crystal shattering on measured in-situ properties in the paper, and conclude that shattering is not a major source of error in their observations because of agreement of directly measured volume backscatter ratio by the MAS with values derived from the FSSP-100 size distributions, because the CIP size distributions overlap with those of the FSSP, and because they have applied corrections to the CIP data set based on interarrival time algorithms. However, their argument is not thorough enough. 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)âĂŤtherefore, 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. Second, with regards to the agreement between the FSSP and CIP, this might be coincidental agreement between two sets of erroneous values. Not withstanding particle shattering, there are many problems associated with the use of CIP data for diameters less than about 150 micrometers. The probe has a poorly defined sample volume which is highly dependent on the particle size: there is a lot of uncertainty on how to calculate concentrations for such small particle sizes. In addition, the CIP arms were designed to be further apart from the arms of the 2DC in order to increase the sample volume: however, in doing so, 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? 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. 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). This is a very important issue as it is critically important that no more misleading scientific manuscripts on ice crystal size distributions be published until this small crystal shattering issue can be resolved. For instance, it may be that the 2DS probe is required to make observations in the size range that are required to examine size distributions in subvisual cirrus. If this is indeed the case, this paper should not be published. 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.

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.

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.

Specific Comments:

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

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.

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 an 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.

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. . ..

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

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 745, 2011.