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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.

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This manuscript represents a careful evaluation of measurements made in different types of clouds over the continent of Africa, a region where very limited in situ measurements have been made. The cloud cases that are presented in this study are of sufficient interest that each of them warrants additional analysis. The information that is presented in this paper, however, helps to highlight their features and will be challenging for cloud modelers to try and accurately simulate the cloud processes. These measurements will also be helpful for those developing algorithms for deriving cloud

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properties from remote sensor data.

Overall the information is organized well and the arguments for interpreting the measurements in the context of hypothesized cloud microphysical processes are sufficiently supported by the published literature.

I found the introduction to be a very nice overview of many of the unanswered questions in cloud formation and evolution. That being said, however, I don't know if such a lengthy review is appropriate for this particular study, especially given that it is never clear, either in the introduction or in the remainder of the text, how this study will address any of the questions that are highlighted. I would recommend that the authors shorten the introduction, include and list only the questions that will be addressed in the paper and then clearly explain throughout the remainder of the paper how the results of the study will help answer these questions.

As with the introduction, I also thought the summary was much too lengthy. The introduction and summary alone make up more than 20% of the manuscript. The summary, in my opinion, should succinctly bring together the most important points of the paper and should circle around to the opening remarks where the scientific questions are introduced. In other words, list the primary results and how they help address the critical questions. Many readers will, as I readily admit to doing, when deciding whether or not to read a paper, read the abstract and the summary then go to the body of the text if the subject seems interesting and new. In the case of the present manuscript, the length of the summary would possibly deter some readers from delving further into what is a very interesting study.

The remainder of my comments and questions are mostly focused on clarifying different aspects of the presentation.

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?

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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.?

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.

Page 756, Line 20. The use of interarrival time, despite its advocacy by some small fraction of the community, has not been universally accepted as an acceptable means to filter measurements for the effects of shattering. If one assumes that cloud particles are spatially distributed in uniformly, random positions in space, this means that the distance between particles will have a probability density function that can be described by Poisson statistics. This means that applying a threshold at some arbitrary interarrival time will remove not only some fraction of spurious particles but a significant fraction of legitimate ones, as well. What fraction of the particles measured by the CIP will have been erroneously removed during this study? This can be easily tested by taking the SVC measurements where no filtering is done and see what fraction of the particles would be removed if all those with arrival times less than 5 μ s are removed and how it would subsequently change the shapes of the size distributions.

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?

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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.

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

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?

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

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?

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?

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?

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

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?

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?

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

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summary.

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.

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

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

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

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