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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.

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Title: In Situ measurements of tropical cloud properties in the West African monsoon: upper tropospheric ice clouds, mesoscale convective system outflow, and subvisual cirrus.

By Frey, W. et al.

Summary:
This paper reports useful new in situ field measurements of tropical cloud properties, in particular their composition, properties and processes in terms of aerosol number and ice particle composition over West Africa. The data are useful to the atmospheric science community and the results are directly relevant to cloud modelers and potentially to a wider atmospheric science audience. The discussion of the aerosol, nucleation and general cloud properties is well presented in general and the campaign data have been used to their full advantage in this study, despite the issues raised below which are easily resolved. The figures are of good quality. The paper has some easily corrected flaws in the way it is structured and there are some grammatical and spelling issues which need to be addressed (see technical comments below). The content is well within the remit of ACP and the results are an addition to a useful and growing dataset of in situ cloud properties measured around the world.

However, having read this manuscript in detail, there are some issues in the current presentation of some of the results which, in this reviewer's opinion, do need to be rectified before publication. Some important data considerations (i.e. ice particle shattering and counting statistics) have been recognised but rather underplayed and from what I can tell, large volumes of potentially useful data have been selected out from analysis for one reason or another and I'm not sure some of that removal is warranted. This might be expected to lead to very significant biases in some of the crucial results of the paper. It is this reviewer's recommendation that the authors need to be less discounting of these impacts on their results and instead quantify some useful potential uncertainty estimates to include alongside the results. This would reduce the chance of improper use of these potentially quite uncertain results by future users and thus make them much more valuable. I sympathise with the difficulty of representing and correcting for this now well-recognised problem but to explain it away as negligible (even after a valiant attempt at removing affected data) does not do the data justice. I recommend publication if the authors can satisfy the conditions that uncertainty estimates are provided and that better treatment of discarded data is looked into, both of which I believe are achievable. I have included some pointers below as to how to go about this and I have lifted anonymity to allow the authors to get in touch directly if they would like any further help to this end, which I would be happy to provide without recognition.

Major points:

Introduction:
The introduction is long and disjointed and does not succinctly convey the context to this paper. The level of introductory detail might be suited to a review paper but does not set a background to this particular work from which to launch into the presentation of results. Also, the largest proportion of the introduction is devoted to the role of MCS in terms of troposphere-stratosphere exchange (STE) of trace gases and aerosol, where an equally important role of MCS and indeed sub-visible cirrus is their modulation of the radiation budget through contrasting radiative properties as a function of both altitude and microphysical makeup, which is related to both aerosol and thermodynamics. I appreciate the AMMA campaign focussed significantly on STE but the data and results in this paper are equally relevant to those interested in cloud radiative properties. It would be this reviewer's suggestion to reduce the length of the introduction and to balance the context of the roles of convective material transport with radiative transfer modulation.

Summary and conclusions:
The summary is rather long and much of the content belongs in the general discussion. The Summary section should be of similar length to the abstract so as to keep the

casual readers attention. Consider moving some of the summary content into the discussion section where the use of sub-sections may also be useful to break up the content into easily located portions.

Approach to particle shattering:
Firstly, it is well known in the ice measurement field that ice measurements using a forward scattering probe are problematic and require special assumptions and data treatment. Please could the authors include these details courtesy of Borrmann, whose T matrix method is likely to have been used. If it has not been used, it is difficult to see how ice measurements could have been derived from an FSSP.

The authors have recognised the difficult issue of ice particle shattering and are to be commended on their attempt to rule out, remove and quantify the potential errors that ice particle shattering can have on their measurements and their subsequent results. They have devoted an entire sub-section to it, which is more than some other papers. However, despite the attention given to the problem, I think it is still ruled out and explained away too efficiently, especially for the MCS case, which does contain large ice particles. I agree that for the cirrus and subvisual cirrus clouds, the effect is less significant based on the methods and papers the authors have referenced. The authors have decided to class this problem as negligible where they could instead have used their effort to define a good uncertainty estimate. For example, the authors quote the fraction of CIP shattered ice crystals to be 20% (although do not state at what size) and therefore similar to an instrumental uncertainty they have assumed for the CIP. However, the CIP instrument uncertainty is an uncertainty in its sample volume (Baumgardner, 1992) and therefore translates to an uncertainty in number counting statistics, and not to sizing. To say that 20% of particles are shattered is not something which is directly comparable to the stated instrument uncertainty. For example, if the 20% of particles shattered were all very large particles (which they typically are known to be; say 100 micron diameter), then this would dramatically change the observed number concentration at small sizes (say at 50 microns) by much more than 20% of their original number (in fact by a factor 8 assuming sphericity in this example and even more for smaller particles). The potential effect on radiative transfer calculations which could use these "shattered" spectra is highly significant and misleading and of high importance to the climate modeling community. Based on interarrival time, the authors have discarded spectra which are assumed to be subject to shattering but this could lead to implicit biases to smaller sizes in the number size spectrum as spectra with large particles present are discarded from analysis. This needs to be mentioned. In addition, I am not satisfied by the sentence that because ice water contents measured in AMMA are "of the same order of magnitude" as those in SCOUT-O3, that this can be used as an argument to assume shattering is negligible – the earlier introduction rightly points to the inherent differences between bulk properties of deep convective clouds in Australia versus those in West Africa.

As a way forward, I would suggest that the authors do not underplay the likely potential impacts of shattering on their results and that a generous upper bound is estimated for the remaining effects of shattering and the potential biases that could exist by removing "shattered" spectra and place this as an error bar on the later size distributions. The data here are useful and publishable but the authors need to be honest about shattering as a source of error here and to serve the community by highlighting this issue with the older instrumentation. An error estimate on the number size distribution could be calculated by conserving IWC but by recalculating the range of spectral shapes allowed by shattering using the knowledge/estimate of the proportion and likely sizes of the particles which are shattered.

FSSP and CIP spectra:
Shattering aside, how have the data been prepared in Fig 5, 10 and 14? The agreement in the overlap region between the FSSP and CIP is either fortuitously excellent or otherwise smoothed. But perhaps this is because (as stated) those spectra where the overlap was not so good have been discarded. This is bad practice and would mean that spectra where large particles were present have been ignored from this analysis and only those spectra conducive to low shattering incidence (and hence lack large particles) have been included. This biases the reported spectra even further to smaller sizes. Furthermore, by ignoring data that don't overlap nicely, this could bias data that may otherwise may contain instrument measurement artefacts such as electronic gain enhancements at small sizes of the CIP and counting statistic fall off at the large sizes of the FSSP (i.e. in the overlap region). I can't imagine much data was retained at all if the overlaps in Fig 10 are representative of the criteria used to select data based on overlap. Agreements between probes in overlap regions are rarely of this quality because this is exactly the spectral region where the instruments perform least well for the reasons described above, whether flying in water or ice cloud. Please include details of the criteria used to select data based on overlap and be certain you are not throwing away data unnecessarily.

Use of potential temperature:
It is useful to use potential temperature as the authors have for this paper as some of the altitudes of interest are in the LS where potential temperature is a valid and useful measure of dynamical processes and height. However, 10 K bins in the UT are still very wide (i.e. cover many km) and potential temperature changes very slowly in the TTL where dt/dz is close to zero. I am concerned that the use of 10 K bins in the UT is a little too wide. Could the authors consider a change in their binning or otherwise include an altitude range (from the aircraft GPS altitude profiles) alongside the theta ranges they have used so this is clear to the reader to make their own judgement?

Minor points:

Section 4.3.4: Entrainment in relation to the NPF peak: Wouldn't any mixing be isentropic, rather than isobaric?

P. 749. Line 9: The authors state that moist convection in a background dry adiabatic profile is deeper with larger hydrometeors. This is an extrapolation and does not take into account aspects such as wind shear and the role of entrainment – deep convection in moist free air can actually be enhanced by entrainment of moisture rich background air in rapid updrafts in the lower regions of the cloud. This sentence is intended to place importance and context on the role of West African MCSs so perhaps the authors could do this by removing (or completing) this thermodynamic discussion whilst retaining the references to TRMM measurements which show that West African MSCs are observed to be deeper with larger hydrometeors than others. Also, large hydrometeors (comparable to AMMA) were seen on occasion in ACTIVE and SCOUT-03 so this contrast in the observations is not clear.

Technical points:

Quite importantly, change "In-situ" to "In situ" and italicise the font in all incidences. There is no hyphen between these two latin words. Please check and correct all instances, including the paper title and running title.

P. 747, Line 11: Change "...proportionate..." to "...proportionately..."

P. 747, Line 14: Remove the comma after "O3" and add a comma after "satellite images"

P. 749, line 8: Change "As consequence" to "As a consequence,"

P. 754, line 6: Change "TLL" to "TTL".

I'm not sure quotation marks are required around "Geophysica" – consider changing.

P. 767, line 23: Change "show thus" to "thus show"

Does an acknowledgment need to be made for the provision of MSG images?

P. 773, line 5: May be useful to quickly define what Hector is for unfamiliar readers.

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