Reply to:
Interactive comment on “Tropical deep convective life cycle: Cb-anvil cloud microphysics from high altitude aircraft observations” by W. Frey et al.

by Anonymous Referee #3

The authors would like to thank the anonymous referee #3 for his/her helpful comments and suggestions.
All issues raised by the referee are discussed below and have been incorporated in the final version of the paper. The referee’s comments are typeset in italic, our replies in normal font.

General Comments

This paper provides reports on high-altitude measurements throughout the lifecycle of a convective Hector storm. A convincing argument is made for the importance of measurements in this region, and the categorization of the measurements into different stages of the same storm is interesting and valuable. The cloud system, measurements & instrumentation are well introduced and explained, and the paper is generally well written.

General Comment #1:
Aerosol measurements in cloud are not well explained; for example, what “aerosol backscatter” & “aerosol depolarization” from the MAS actually represents. The discussion seems to focus on ice crystal properties, but the terminology all references aerosol.
General Comment #2:
In addition, the section on aerosol/cloud particle number ratios, besides being long and unfocussed, was not convincing since there are likely problems with aerosol measurements being used in-cloud. Without further discussion/analysis of the sampling inlet, results shown could be due to shattering artifacts increasing aerosol number concentration, rather than actual changes in the aerosol/cloud ratios.
General Comment #3:
As the first reviewer stated, more use of microphysical images to back up hypothesized changes in ice crystal size & habit at different lifecycle stages would be useful. And as the second reviewer states, there are a number of assumptions made related to cloud freezing history and lifecycle that may not be valid.
I would encourage the authors to give as much care and attention to the last half of the paper as to the first half–and then the paper should be an acceptable and useful addition to the cloud physics literature.

Reply general comment #1:
MAS measurements:
Valid point of the reviewer. Out of cloud MAS measurements indeed represent aerosol backscatter and depolarisation. However, data presented here are all in cloud and thus data are dominated by the cloud particle contribution. Thus, the nomenclature should be “cloud particle backscatter” and “cloud particle depolarisation”. We changed this accordingly.

Reply general comment #2:
Aerosol sampling inlet:
The COPAS sharp-edged diffuser type aerosol inlet is described and its performance is discussed in Weigel et al., AMT, 2009 in terms of aerosol aspiration, transmission and transport efficiency. Some of the salient features are summarized as follows: Actually the inlet consists of TWO sharp-edge diffusors inside each other. The outer inlet acts as a shroud and decelerates the flow from ca. 200 m/s to 20 m/s. Further downstream and inside this shroud there is a second diffusor as isokinetic sampling nozzle which extracts the final sample air flow from the decelerated flow and guides it through a 90 degree bend into the instrument. Cloud ice particles can collide with the outer inlet and produce fragments which indeed could enter the inlet and fly towards the isokinetic sampling nozzle.
(A.) The outer inlets inner entry diameter (7.3 mm) and wall thickness (0.2 mm) provides surface area of about 4.7 mm^2 in total on which cloud particles principally could impact. The likelihood for this to occur is very low due to the small surface area of the sharp edge. Also the diffusor has a conical shape somewhat similar to the recently developed “anti-shattering tips” such that the deflections of fragments into a sampling volume (like for some optical probes) are minimized. Thus only direct hits onto the outer sharp edge can produce fragments capable of reaching the sampling nozzle inside.

(B.) The inner isokinetic sampling nozzle again exposes a very small cross section to the flow. Thus the low probability of fragments produced by shattering at the outer inlet has to be multiplied by another low probability for these fragments actually also entering the inner sampling nozzle.
(C.) Shattered fragments with sizes in the micron range and above, which end up inside the inner sampling nozzle, will impact on the wall of the 90 degree bend with high likelihood and are removed this way.
(D.) Fragments smaller than 1 µm and especially with sizes down to 15 nm are very rare because of the enormous energy required to produce such small pieces.
(E.) IF such fragments exist, their next obstacle would be the heating involved in the deceleration and the COPAS detection units, which operates at 30°C. Ice fragments with sizes up to several hundred nanometers would evaporate fast under such conditions.
(F.) The ambient number densities of the cloud particles (i.e. the available potential shatterers) in the submitted manuscript's Figure 8 are a factor of roughly 100 lower than those of the submicron aerosol particles. In order to appreciably enhance the number of aerosol particle counts a lot of shattered fragments would have to reach the CPC counting unit.
(G.) Finally, such inlet hitting events would cause burst-like increases of the particles number concentration detected by COPAS. Individual – possibly several consecutive - 1 second measurement periods would have significant and identifiable increases of the detected particle number concentration over very short time periods in the 1 Hz data set. In general, if the shattering produced a significant increase of the nanometer sized aerosol particle concentration, COPAS would report measurably higher particle number densities after entering a cloud followed by a return back to clear-sky values after exiting the cloud. Such events in connection with in-cloud measurements were never observed.

Based on these arguments (A) through (G) we are convinced that measurement artefacts due to shattering of cloud elements onto the inlet entry surface are of negligible impact on the measured concentrations by COPAS. Furthermore, the cloud particle observations show that the measurements were not performed under heavy shattering conditions (see Fig. 1 in the revised manuscript).

Reply general comment #3:
The discussion about the ice particle shape analysis below is the same as in the reply to reviewer 1 (Darrel Baumgardner). The questions related to cloud freezing history are discussed in detail in the reply to the second reviewer, where we tried to clarify the unclear points raised by the reviewer. The revision of the manuscript lead to substantial changes particularly in Section 5, thus we would like to refer this reviewer to the revised manuscript.

<u>Ice particle shapes</u>
Unfortunately, the authors do not have a sophisticated computer code to analyse particle shapes. Therefore, we can only provide a more detailed analysis regarding the area ratio of the particle images. The area ratio is defined as the shaded area in the particle images divided by the area of a circumscribing circle with the maximum dimension as diameter. Particles with a maximum dimension smaller than 5 pixels (i.e. 125µm) were excluded from this analysis, because they are too small to give reasonable shape information. This limits the number of available particles for the area ratio analysis, as shown in Table 1. From the remaining data, histograms have been derived for each potential temperature bin.

	developing stage	mature stage	dissipating stage
350-355	0	12118	1361
355-360	21	16612	7
360-365	0	2026	43
365-370	0	31	0
370-375	0	4	0

Table 1: Numbers of particles in each bin for area ratio analysis.

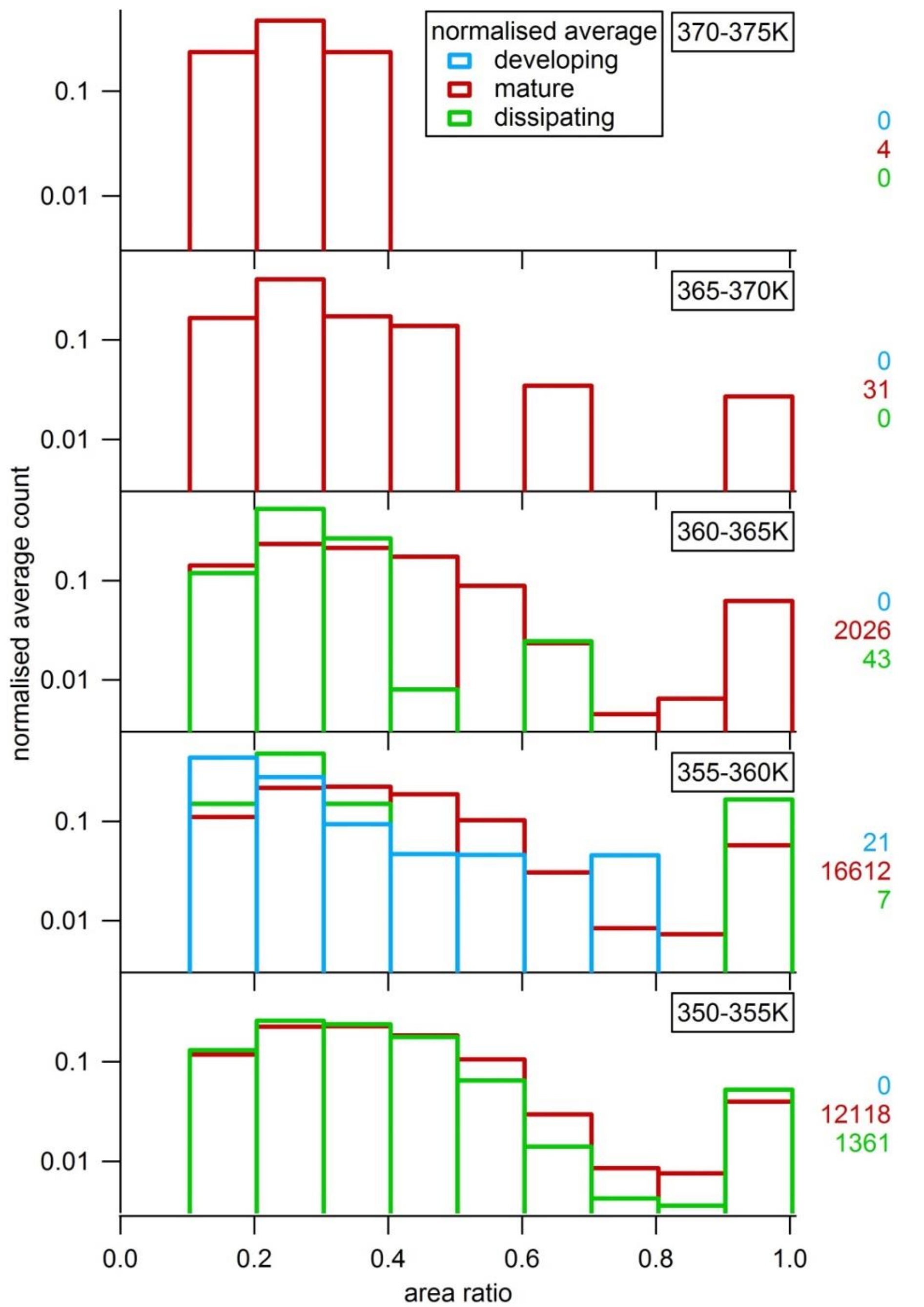

Figure 1: Normalised histograms (to a total value of 1) of area ratio occurrence. The number of analysed particles is given by the coloured numbers on the right.

Particles with area ratios smaller than 0.1 were excluded as a general correction to remove artefacts like streakers. Only few particles are left for evaluation in the developing stage, all in the 355-360K bin. The histogram indicates an increasing contribution of particles with small area ratios, i.e. more elongated particles. However, the meaningfulness of this can be questioned due to the small sample size. The mature stages show a bimodality, peaking at 0.2-0.4 (probably chain aggregates or column particles) and a second peak at 0.9-1 (possibly aggregates or rimed particles). The dissipating stage looks similar to the mature stage, though the bimodality disappears higher up. So if the second peak was due to aggregates and rimes crystals these would certainly have been big and already sedimented. The depolarisation curves of the mature and dissipating stage decrease with altitude which suggests that particles with higher area ratio contribute more to the depolarisation. It has to be noted that even though the normalised area ratio histograms for mature and dissipating Hector look similar, the number of particles in those two classes is different. Also the small particles (<125µm) are not analysed in the area ratio framework, but may contribute significantly to the value of depolarisation. Hence, there might be a difference in the shapes of the small particles causing the different values of depolarisation. Possibly the second reviewer has a good point here in speculating that some of the ice crystals in the dissipating stage might actually have formed in situ – and those newly frozen small particles could lead to the higher depolarisation, while the aged Hector particles are found among the bigger ones that show similarity to the mature crystals.

The discussion about area ratio is included in the revised manuscript at the end of Section 4.1.

Remark from the authors: Please note that the language related specific comments were at first implemented in the originally submitted manuscript. However, since the two other reviewers asked to restructure the text, as well as for a general shortening of some sections, the corresponding sentence (i.e. the "target" of the specific comment) may have disappeared altogether as we overhauled the entire manuscript in a second step.

Specific Comments

Specific Comment #1: p. 11817, lines 24-25: *I would argue that there have been a number of microphysical studies of high-altitude cirrus, particularly anvil cirrus, in the tropics. In the next paragraph you discuss what really hasn't been done much–examination of the TTL layer and the dissipating stages of storms. Thus, just remove this sentence and let the next paragraph speak for itself.*

Reply: The sentence was removed.

Specific Comment #2: p. 11818, lines 18: *"How" should be "what".*

Reply: corrected

Specific Comment #3: p. 11821, line 1*: "perform" should be "performing".*

Reply: corrected

Specific Comment #4: p. 11822: *The aerosol sampling inlet is not described, and may affect your results, particularly in cloud. See also later comments.*

Reply: We provided a more detailed reply to this in connection with our reply to General Comment #2. For more details we would like to refer to Weigel et al. (2009) who provide many details in Sections 2.2, 3.1, 3.2, and 3.3 regarding the inlet system and the sampling.

Specific Comment #5: p. 11827, line 10: *How is IWC measured?*

Reply: We added an explanation about the IWC in the Instrument section:

"An ice density of 0.917g/cm3 was used to calculate the ice water content (IWC), assuming sphericity in the FSSP size range and using an image to mass relationship as introduced in Baker and Lawson (2006) for the larger particles."

Here we also refer to the publication by de Reus et al. (2009), where the ice water content derived from the size distribution measurements is directly compared to the IWC obtained from the two hygrometer instruments. By means of the two hygrometers (one for total water content and one for gas phase (only) water content) the IWC was determined based on the difference between these two gas phase water vapour measurements. This direct intercomparison was performed on the same Hector data set from November 30, 2005, plus the data from the flight on the previous day and resulted in very satisfactory correspondence for IWCs between 10^{-5} to 10^{-2} g/m^3. This fully includes the range of IWCs covered in Table 1 of our paper as well.

Specific Comment #6: p. 11828, line 13: *"the AMMA clouds"–what development stage were these in, for comparison? How many clouds are represented in the median? If it is the median of many cloud systems/stages, then I'm not sure how meaningful the comparison with one Hector case would be. Perhaps these issues/limitations should be discussed upfront, rather than as an aside at the end of the discussion.*

Reply: "The AMMA clouds" were in different development stages and in different distances to the convective core. The numbers of size distributions are: 45, 9, 8, 10 for the 350-355K, 355-360K, 360-365K, and 365-375K potential temperature bins respectively. We agree that this reduces the generality of the conclusions in this subsection, but we think a juxtaposition of the Hector type MCS and the West African Monsoon type MCS still remains useful (not only because in situ data from both with similar instrumentation are scarce, especially from campaigns less than 9 months apart). Indeed, the AMMA MCS clouds are very different from Hector when considering (a) the meteorological mechanisms behind their respective formation, (b) the different surface conditions, (c) their largely different sizes, extents, and temporal evolutions, (d) the much longer life times of the AMMA clouds, and many other aspects connected with their propagation/movement and precipitation. However, when it comes to their impact on the TTL, it seems that the effects – as seen from the local in situ measurements - of the two cloud types are quite similar. The size distributions in the respective potential temperature bins are not very different, and also at the lower altitudes in the anvil region the number concentrations are alike over the covered size range. For these reasons we would like to keep the curves for the AMMA clouds in Figure 7 of the originally submitted manuscript. After all both cloud types are tropical MCS, deep convective, high reaching, and (potentially) penetrating the stratosphere. However we rewrote the small section on the interpretation and removed for example the comparison of the convection strengths. Instead the relevance for the TTL is emphasised.

Specific Comment #7: p. 11829, line 19: *"this illustration shows"–what illustration? It seems to me the subsequent discussion is hypothetical. Cannot the actual dissipation measurements be used to say something specific about the fate of this storm?*
Reply: The illustration = the comparison of the "general" SVC size distribution to the dissipating Hector size distribution. Since the discussion in this subsection indicates the potential but not the necessity for SVC formation, because we did not actually measure SVC, we named this section "Potential for SVC generation". So, yes, indeed there is speculation involved here.
From the measurements of the airborne lidar we included an optical thickness estimate, which resulted in $\tau = 0.88$. This estimate, however, considers the whole cloud layer (up to 6km) and therefore it is not surprising, that optical thicknesses are larger than expected for SVC. Nevertheless, they are much thinner than optical depths of deep convective anvil clouds (e.g. 20-40, Heymsfield, 2003).
We also added an optical thickness estimated based on the in situ cloud particle measurements, following Garrett et al. (2003). We assumed a layer thickness of 1km, which could be the thickness of a SVC layer sheared of from the anvil cloud, or of the cloud part remaining after further dissipation of the lower cloud layers. The calculation reveals that all clouds in the dissipating stage could (already) be classified as thin or subvisible with optical thicknesses between $6*10^{-5}$ and 0.2.

Specific Comment #8: p. 11830: *What particle size is the MAS sensitive to? Are we really discussing "aerosol" backscatter & depolarization here, since the measurements are in cloud?*
Reply: As most lidar-based instruments, MAS is sensitive to particles with radius >0.1µm and larger. As we presented in-cloud data, with high backscatter ratio, data are dominated by cloud particle contribution. As stated in the reply to general comment #1, we changed the nomenclature to "cloud particle" backscatter and depolarisation.

Specific Comment #9: Line 18: *"it's" should be "its".*
Reply: Has been corrected.

Specific Comment #10: p. 11831, lines 11-15: *Aerosol inlets are also subject to crystal shatter at high speed, unless specially designed for interstitial measurements. Characteristics of the inlet should be specified in the instrumentation section and its behavior in cloud should be examined and discussed.*
Reply: We tried to summarise our argumentation on this in our answer to General Comment #2 above.

Specific Comment #11: p. 11831-11832: *This section should be broken up into shorter, more digestible segments with specific foci.*
Reply: This has been implemented as part of the general overhaul of the manuscript as indicated above.

Specific Comment #12: p. 11832, lines 13-16*: The temperature range is seemingly too cold for the traditional Hallett-Mossop process to be important.*
Reply: What we actually meant was that the Hallett-Mossop process occurred at much lower altitudes (and warmer temperatures) inside the turret region of the deep convective cloud producing splinters there, which subsequently are carried aloft and reach the anvil/outflow region. This was just one example for ice multiplication processes and we agree that other examples, though less known, would be more appropriate. Therefore, we rewrote this paragraph as follows:
"Ice multiplication processes might be the reason for higher cloud particle concentrations while aerosol concentrations stay fairly similar to those of the developing Hector cases. Collisions of ice crystals involving rimed crystals can lead to mechanical breakup of the particles, leading to significantly higher number concentrations also at temperatures lower than during the Hallett-Mossop process (Vardiman, 1978; Yano and Phillips, 2011). These multiplication processes could as well have happened in the

lower parts of the cloud and secondary ice crystals subsequently carried upwards into the measurement region."

Specific Comment #13: p. 11833, lines 3-5: *Shatter of larger crystals producing aerosol artifacts could also produce these results.*
Reply: In principle the reviewer of course is right. However, under the specific measurement circumstances of our flights we are far away from high concentrations of large hydrometeors (like in mixed phase clouds) and the "warm" temperatures, both of which typically cause serious shattering artefacts. With the arguments (A) through (G) provided above as reply to General Comment #2 we are convinced that this effect is very small and probably within the counting statistics of the aerosol measurements for our sampling arrangement. The most extensive discussion of shattering artefacts in our measurements within tropical high altitude anvil clouds is contained in the supplement to the paper by Frey et al., ACP, 2011.

Specific Comment #14: p. 11833, line 20: *Be consistent in using aerosol to cloud ratio or cloud to aerosol ratio, not both.*
Reply: Valid point of the reviewer; we changed to 'cloud to aerosol ratio' accordingly.

Specific Comment #15: p. 11834, line 7: *"microphysical" is misspelled.*
Reply: corrected

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