**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 Darrel Baumgardner (Referee)**

The authors would like to thank Darrel Baumgardner for his helpful comments and suggestions that helped to improve the manuscript.
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:***

***General comment #1:***
*This paper has a lot of unrealized potential to provide a valuable set of data to what is still a relatively sparse database of measurements in the TTL.*
*The manuscript starts off with a nice overview of what is known of clouds in the TTL and what measurements have been made, particularly in the Hector system. What is missing is a clear road map of what the current study will provide that has been missing and the abstract and summary also lack that explanation.*
*The supplementary material contains an annotated PDF with all my comments and questions. In general, however, I feel that a fairly major modification is needed to the text and associated analysis in order to clarify and provide more detail. There are many speculative statements made in the interpretation of the measurements with not enough discussion to support them. In particular I was very confused by the discussion that tries to link the lidar data with the in situ measurements and the CN measurements with the cloud probe and lidar measurements. Part of the problem is that there are large paragraphs containing the various speculations that need to be broken into distinct sections that provide more detail explaining how these speculations about cloud processes were arrived at. I have commented on all of these in the annotated file.*

***General comment #2:***
*The other critical measurements that are under-analyzed are the images from the CIP. More detailed shape analysis is needed to back up all the discussions about ice crystal types. If nothing else, categorization by degree of asphericity to compare in the various cloud stages would provide a lot more clarity in the discussion.*

***General comment #3:***
*It would help me a lot to follow the somewhat convoluted train of events if the authors provided a conceptual diagram of how they perceive the cloud system in the four stages, including the potential temperature levels so that I can better understand where the measurements are being taken and what are the cloud properties in each of these levels.*
*At the moment the discussion of the microphysical processes come across as too much "hand waving" and not enough concrete description that can support the hypothesized processes.*
*In its present form, the paper doesn't provide the necessary detail to merit its publication.*

**Reply general comment #1:**
In the cause of overhauling the manuscript we hope to have clarified and modified the manuscript in a way satisfactorily to the reviewer. This includes a description about the interpretation of MAS backscatter and depolarisation measurements, which seem to have caused some confusion, also among the other reviewers. We reduced the text and removed many of the statements which were criticised as too speculative. However, due to comments by all three reviewers, text and figures have been added at other places. As mentioned also in our reply to Reviewer #3 the language related specific comments as well as many of the technical comments were at first implemented in the originally submitted manuscript. However, since also the 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/technical comment) may have disappeared altogether as we overhauled the entire manuscript in a second step.

**Reply general comment #2:**
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.

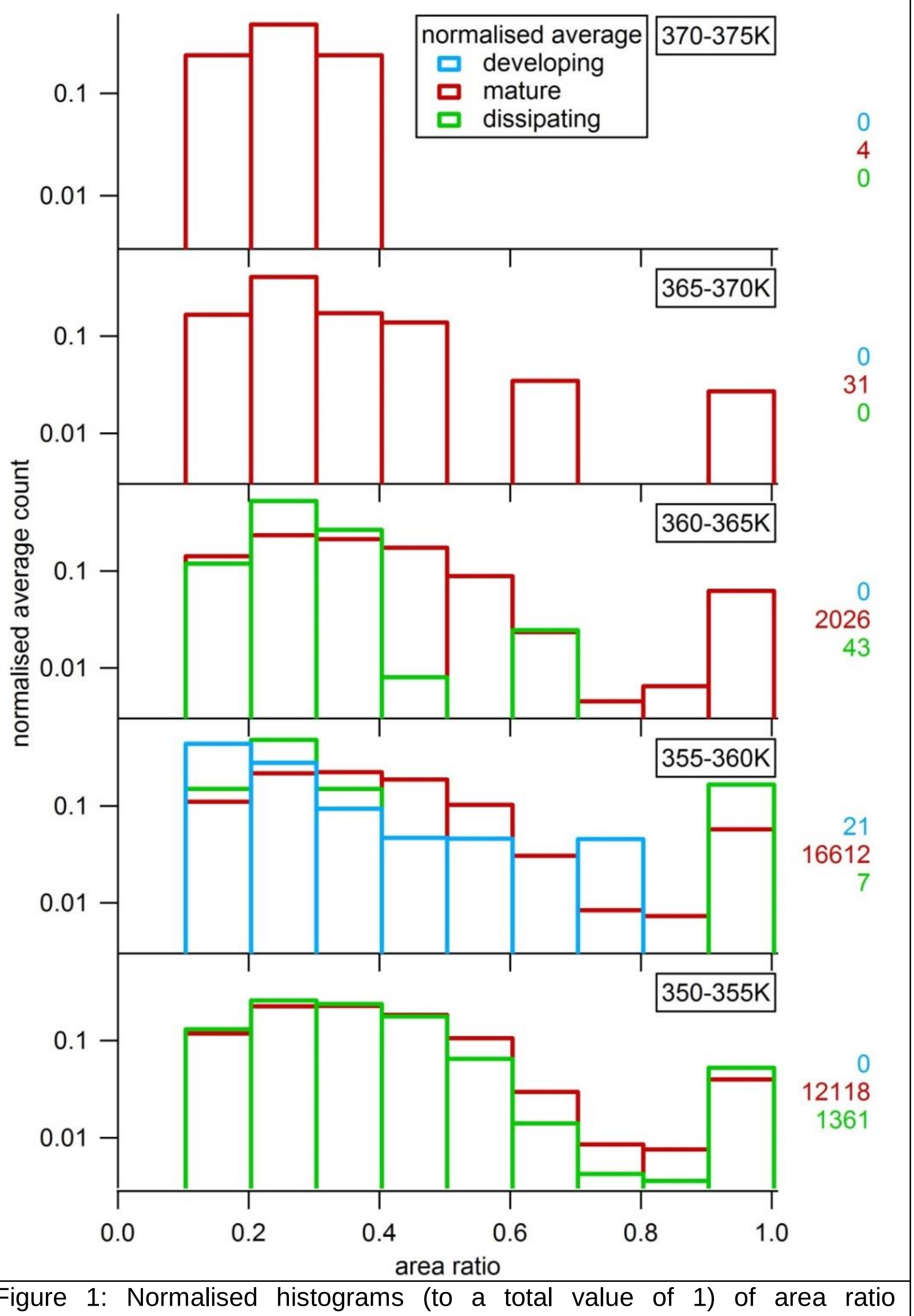


Figure 1: Normalised histograms (to a total value of 1) of area ratio occurrence. The number of analysed particles is given by the coloured numbers on the right hand side.

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 rimed 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 Sect. 4.1 - Observations of size distributions in Hector development stages.

**Reply general comment #3:**
We tried to improve the text such that hopefully an additional diagram is not needed. We added two further figures in the revised version (interarrival times, and shape distributions) which already lengthened the not so short manuscript.

***<u>Specific comments:</u>***
**Authors remark**: We copied here verbatim all the annotated comments of the Reviewer (which were contained inside the document with the submitted manuscript) and replied to each comment individually.

***<u>Introduction:</u>***
***Comment 1:*** *You have already said this above. No need to repeat here.*
**Reply:** This and the next sentence removed.

***Technical corrections***
**Reply:** We implemented the technical corrections as suggested.

***<u>Section 2:</u>***
***Comment 2:*** *Nothing is said about the uncertainties in size measurements and the derived LWC. It needs to be emphasized that the equivalent optical diameter is being measured assuming spherical particles. What ice density is being used?*
**Reply:** In order to derive the diameter of a particle from the CIP images, the maximum dimension (Heymsfield et al., 2002) has been used.
For the calculation of IWC (all measurements were performed below -40 °C) from the CIP images, we used the image-mass relationship as introduced by Baker and Lawson (2006). The smaller particles in the FSSP size range are assumed to be spherical. Furthermore, Mie curves and T-Matrix curves have been used to adapt the FSSP size bins to ice particles, as in Frey et al., 2011 (and in particular in the supplement). An ice density of 0.917g/cm$^3$ is used. As also included in our reply to Reviewer #3 we furthermore 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 lyman-α 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. The measurement errors, as given in de Reus et al. (2009) are for the lyman-α instruments a factor of 2 and for the cloud particle probes 20%.
We added the following text to the revised manuscript:
"Particle diameters are derived from the CIP images using the maximum dimension (Heymsfield et al., 2002). Sizing of FSSP particles has been performed assuming the particles to be spherical. Considering the scattering cross sections from T-matrix and Mie curves with a refractive index of ice, the original 40 size bins have been redefined into 7 size bins, to account for ambiguities. An ice density of 0.917g/cm$^3$ 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."

***Comment 3:*** *I see no need for a separate numbered section*
**Reply:** First, we joined the sections but after expanding on the instrumentation section in general following your and the other the reviewers requests (also in connection with Comment 7 on Section 3 below), we decided to introduce headlines to increase readability and reduce the large textblock, as we did for the remainder of the manuscript.

***Technical corrections***
**Reply:** We did the technical corrections as suggested.

***Section 3:***
***Comment 4 and 5:*** *This is very confusing. Either use local time or UTC, not both. I prefer local time since the development depends on the solar heating cycle that is more understandable in local time units. Also, emphasize that there was only a 4 hour time gap between flights. No small feat! Change in flight crews? - Please only use LT or UTC throughout, but not both. Just state at the beginning the difference in LT and UTC.*
**Reply:** We think so too, but often editors prefer UTC specifications. Now we changed the time specifications to local times, with a note of the 9:30 hours difference to UTC.
Geophysica is a single seated aircraft like the ER-2. All that was necessary, in principle, between the two flights was a change of the pilot. However extensive checking on the aircraft also had to take place. In Darwin a hangar was available which allowed us to service the instruments as well between the two flights. But – deviating from our usual routine - we could only do calibrations (like with PSLs on the FSSP) on the cloud instruments before the first and after the second flight. Fortunately, the calibration runs showed no changes of the critical instrument settings.
We added the following text to the revised manuscript:
"Thus, there was only a four hour gap between the two flights, which was just enough to service the aircraft and the instruments. Since the Geophysica is solely flown by one pilot and no additional crew, the instruments run fully automatically and just the pilot changed on the second flight."

***Comment 6:*** *Suggest not using the word dissolve in this context. Decay or dissipate is the more commonly used expression.*
**Reply:** We removed dissolved and used dissipate instead.

***Comment 7:*** *This should be described in the instrumentation section, not here.*
**Reply:** Moved to instrumentation section.

***Comment 8:*** *Need to explain why this is. The lidar can't penetrate clouds with optical thickness >3? Discuss this in the instrumentation section.*
**Reply:** The observation of the reviewer is fully justified and points to a sentence that was missed to be rectified during the final revision of the manuscript. In fact, the penetration ability of a lidar not only depends on the optical thickness of clouds and aerosol layers, but also on the instrument (characteristics) itself, for instance on its pulse energy level. However, the detection of the surface shows the correct operation of the instrument. The sentence is changed to:
"... as evident from Fig. 3. In those lidar measurements a return from the surface has always been detected, demonstrating the stability of the performances of MAL instrument. "

***Comment 9:*** *Expand this discussion as it is confusing. I assume that you are referring to a previous comment about the satellites not seeing cloud but the message about the relationship between lidar return, satellite optical depth and clouds gets lost in this discussion.*
**Reply:** Rephrased to:
"Apparently after the Hector encounter during the first flight, clouds remained in place at around 10–16km altitude, even though they are not visible in the satellite IR images. Considering the airborne lidar, backscatter, and in situ observations we believe these clouds were not optically thick enough anymore to give sufficient signal for the satellite sensor."

***Comment 10:*** *I don't think that you really mean that the same air masses are being probed. Aren't you referring to the source of the air masses?*
**Reply:** The reviewer is right, we were not clear about this point, we rephrased as follows:
"...leads to the assumption, that there was no significant horizontal advection of air during and between the two flights, which could have transported non-Hector clouds or cloud parts into the measurement region."

***Comment 11:*** *is there a reference to this method?*
**Reply:** we added the reference:
"…analogously to Law et al., 2010."

***Comment 12:*** *I don't follow this argument. Ozone is being used as the means to determine if Flights 1 & 2 were sampling the same air masses so even though horizontal winds are low, suggesting no horizontal advection of new air, the ozone change suggests that is still vertical transport and possible mixing, so these are not exactly the same air masses. What am I missing?*
**Reply:** The main point here is to make clear that in flight 1 and 2 the same clouds have been probed and no

other clouds were advected into the sampling region. Horizontal advection could have easily replaced the air mass column, or parts of it, initially probed during the first flight. Based on our analyses we are convinced that the same clouds have been probed twice and that no other clouds were advected into the sampling region. However, in the column diabatic processes like cloud dissipation and also vertical transport with trace gas redistribution occurred. This indicates that the convective cloud is still evolving but it nevertheless is the same cloud that was sampled.
For clarification we added the statement:
"It also shows that horizontal air mass advection was negligible. Additionally considering the satellite images and trajectories for the two flights on 30 November, it seems reasonable to assume that the same clouds have been probed at different times and that there was no recent horizontal advection of other clouds into the sampling region."

***Comment 13:*** *I don't reach that conclusion.*
**Reply:** Please see our reply to Comment 12 above.

***Technical corrections***
**Reply:** We included the technical corrections as suggested.

***<u>Section 4:</u>***
***Sec. 4.1***
***Comment 14:*** *We know the cloud system is Hector, why refer to the groups using the addition of Hector? Why not just developing , overshooting, mature and dissipating stages?*
**Reply:** Changed according to the reviewer's suggestion.

***Comment 15:*** *class? You mean potential temperature category?*
**Reply:** Rephrased to:
"The vertical profiles, in terms of potential temperature, of the averages of every Hector stage are shown in Fig. 7." (previously Fig. 6)

***Comment 16:*** *Why are all the blue lines solid except for the 350-355 K category?*
**Reply:** The dashed lines denote that only one size distribution has been measured in the respective theta bin for the respective Hector stage, as also mentioned in the figure caption. We added a statement regarding this to the text as well:
"In three classes only one size distribution has been measured. This is indicated by dashed instead of solid lines in Fig. 7." (previously Fig. 6)

***Sec. 4.2***
***Comment 17:*** *I find this section rather weak without further discussion of the processes that lead to larger ice crystals and how these might differ between Hector and West African Systems. Not only CAPE is important and the strength of the updrafts, but the aerosol characteristics can also play a role in the rate of glaciation and formation of larger ice crystals through diffusional growth, riming and aggregation. What is the objective here of making this comparison?*
**Reply:** As also the third reviewer pointed out, the African clouds could not be classified in the same way as here and thus, the comparison as shown here might be misleading. After some discussion among the co-authors we decided to shift the focus of this section. Addressing a similar comment by Reviewer #3 we replied to her/him: 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 and the aerosol reservoirs 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.

***Sec. 4.3***

***Comment 18:*** *This is an interesting section but needs expanding and clarifying each of the points rather that merging them all together in a single paragraph. What information do you get from the lidar about the optical depth of these clouds wrt to SVC?*

**Reply:** We revised this section and also included an optical thickness estimate from MAL measurements, 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 typical 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. Also, we don't want to make the assumption that in the lower cloud layers the microphysical parameters have the same values as in the upper cloud layer. The calculation reveals that all clouds (uppermost 1km) in the dissipating stage could (already) be classified as thin or subvisible with optical thicknesses between $6*10^{-5}$ and 0.2.

***Technical corrections***

**Reply:** We integrated the technical corrections as suggested.

***<u>Section 5</u>***

***Comment 19:*** *I am confused by the terminology here. Are you referring to cloud particles as "aerosol particles" as well as the aerosol particles outside the cloud?*

**Reply:** 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. Therefore, the nomenclature should be "cloud particle backscatter" and "cloud particle depolarisation". We changed this accordingly.

***Comment 20:*** *altitude variation of what?*

**Reply:** Meant is the altitude variation of depolarisation ratio. However, the complete section has been restructured. A similar sentence now reads as:

"…which might be a tentative explanation for the absence of altitude variation of depolarisation in the developing stage."

***Comment 21:*** *I don't follow this. How does a change in morphology relate to gravitational settling, riming and growth by accretion? There are contradictory statements made here. Decreasing depolarization would imply a greater fraction of spherical or quasi spherical particles but riming would remove these particles and you also say that larger particles are rising in altitudes and these larger particles are sure to have higher depolarizations.*

**Reply:** The reviews made us aware that we should add a paragraph explaining the interpretation of these particular depolarisation measurements. This has been included in Section 5.1 under the subsection "Depolarisation ratio". In general, for a given shape the depolarisation increases with the dimension of the particle (i.e. within the range of dimensions not far from the wavelength, here 532nm) up to an asymptotic value, which depends only on shape (Liu and Mishchenko, 2001). Given the cloud particle dimensions, we are in the asymptotic range here! Thus, depolarisation will not increase with increasing cloud particle size. The asymptotic value depends on the particle shape, but in what manner is hardly predictable. For example, it can be shown that spheroids with an aspect ratio close to unity have higher depolarisation than prolated or oblated spheroids. Plates and spheroids produce similar depolarisation ratios, while columns have higher depolarisation ratios (Noel et al., 2004). It is, however, safe to say that when the depolarisation ratio of the probed cloud particle population changes, the average morphology of the cloud particles changes as well.

***Comment 22:*** *First of all, I don't understand what the results in Figure 8 have to do with what is seen in Figure 7. Figure 8 shows that the interstitial aerosol remains constant regardless of the cloud particle number but Fig. 7 shows a very range of depolarizations at the one potential temperature level.*

**Reply:** The interstitial aerosol number densities vary from 70 particles per $cm^3$ to 500 per $cm^3$ over the different sampled cloud parcels from Hector. For the general concentration levels in the TTL region this does constitute a significant variation in submicron interstitial aerosol, which cannot really be considered as "constant" here. However, the submicron aerosol only has a minor contribution to depolarisation which is dominated by the cloud particles even if these are present in comparatively low number densities. Some averaging is implied in Figure 8 as each point represents a size distribution. In Figure 7 by contrast, there is one point per data point and no averaging was performed here. (Please note: in the revised manuscript, the Figure numbers changed: former Figure 7 refers now to Figure 9 and former Figure 8 refers to Figure 11.)

***Comment 23:*** *I don't know how you can arrive at this conclusion based on Figs. 7 and 8. A great deal more explanation is needed here to link all these pieces together and convince at least this reviewer that the same freezing mechanism is at work.*
**Reply:** In general, this section has been restructured. In this overhaul we hope to have clarified this point. Also, we pointed out speculative aspects of the interpretation.

***Comment 24:*** *Aggregation for sure but I don't think that you can distinguish riming from aggregation.*
**Reply:** We believe that in the particle images you can see not only aggregates (often having more elongated shapes, thus being chain aggregates) but also rimed crystals that are more spherical in shape with small attachments. See also the replies to the following two comments.

***Comment 25:*** *Aggregation should decrease not increase number concentration.*
**Reply:** That is correct; however, riming and aggregation provide the necessary large crystals for ice-ice collision that may lead to ice multiplication. See also reply to next comment. We added the following sentence to the revised manuscript:
"These larger particles are important for efficient ice multiplication by ice-ice collision, as detailed below. The change in ice particle numbers is reflected in the size distributions…"

***Comment 26:*** *The Hallet Mossop mechanism occurs at very specific conditions of temperature (-8C) and droplet size. Are these present? If not then you can't use this mechanism as a possible cause for the larger concentrations.*
**Reply:** Yes, of course. 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."
Essentially, this is speculative. All we can do -in absence of measurements from the cloud core region- is mention the possible mechanisms, hoping that at least future model simulations could shed a light on this. Probably the value of our measurements at this point and in general is to provide constraints (as well as motivation) for model simulations.

***Comment 27:*** *I am again puzzled by this analysis. If you have more aerosol particles doesn't this increase the aerosol to cloud ratio, not decrease?*
**Reply:** The sentence before should have said cloud to aerosol ratio (but mistakenly said aerosol to cloud ratio). See also reply to next comment.

***Comment 28:*** *You switch between aerosol to cloud ratios then cloud to aerosol ratios. This is very confusing.*
**Reply:** This clearly is our mistake and the reviewer rightfully complains here; we changed to "cloud to aerosol ratio" in all according places.

***Comment 29:*** *Remove this sentence as it is obvious. No aerosols no clouds.*
**Reply:** This sentence was removed.

***Comment 30:*** *And this leads me back to my comment about differences in Hector and African clouds. Discuss these simulations in that section.*
**Reply:** We hope to have addressed this point sufficiently with our reply to this reviewer's Comment 17 above and in the rewritten section about the AMMA clouds.

***Technical corrections***
**Reply:** We did the corrections as suggested.

***Section 6:***
***Comment 31:*** *What is the purpose of this sentence?*
**Reply:** For clarification we added:
"…, which is not negligible in terms of the conditions regarding TTL humidity and ultimately stratospheric humidity."

***Comment 32:*** *References to figures and tables should not be in a summary.*
**Reply:** This is correct. However, since it would interrupt the text flow for the reader we thought it would be better to repeat the reference to this table instead of writing out its content here. We could change it to a short reference in the sentence before this "… dissipating Hector cases (cf. Table 1)". We would leave the decision to the reviewer/editor whether to leave/change the reference or to remove it completely.

***Comment 33:*** *This statement has no basis, at least no within the context of what has been described in the text.*
**Reply:** We removed: "…, as homogeneous freezing."

***Comment 34:*** *Not in the images in Fig. 9*
**Reply:** Please see our reply to Comment 24. Based on previous analyses of data from several campaigns, which have shown a variety of shapes, from pristine stellar crystals, needles, rosettes, capped columns to aggregates, plates, graupel and rimed particles (examples shown in Frey, 2011, PhD thesis), we do believe that we can also see rimed particles in these images here.

***Comment 35:*** *Pure speculation with no evidence as described in the text.*
**Reply:** We removed: "…contact freezing…"

***Comment 36:*** *Again, not enough or clearly explained enough evidence presented.*
**Reply:** We rephrased to:
"In the dissipating stage Hector shows a wide variety of cloud to aerosol particle ratios, which might be an effect of ageing. Furthermore, according to the area ratio analysis the cloud particles have similar shapes as the particles in the mature stage, also indicating ageing."

***Comment 37:*** *Non-quantitative.*
**Reply:** We rephrased this paragraph. In general we agree that it is not possible, without detailed model simulations, to make a quantitative statement here. However, we believe it is an important point to raise, also to motivate such model studies.

***Comment 38:*** *No useful information in this comment.*
**Reply:** We removed this sentence.

***Comment 39:*** *I don't know what this means.*
**Reply:** We rephrased the sentence as follows:
"The data presented in this study provide a contribution to the very sparse in situ data set of TTL convective cirrus, including a classification of the cloud system's development stage."

***Technical corrections***
**Reply:** Correction done as suggested.

References:
Baker, B. and Lawson, R. P.: Improvement in Determination of Ice Water Content from Two-Dimensional Particle Imagery. Part I: Image-to-Mass Relationships, *J. Appl. Meteorol. Clim.*, **2006**, 45, 1282-1290

de Reus, M.; Borrmann, S.; Bansemer, A.; Heymsfield, A. J.; Weigel, R.; Schiller, C.; Mitev, V.; Frey, W.; Kunkel, D.; Kürten, A.; Curtius, J.; Sitnikov, N. M.; Ulanovsky, A., and Ravegnani, F.: Evidence for ice particles in the tropical stratosphere from in-situ measurements, *Atmos. Chem. Phys.*, **2009**, 9, 6775-6792

Frey, W.; Borrmann, S.; Kunkel, D.; Weigel, R.; de Reus, M.; Schlager, H.; Roiger, A.; Voigt, C.; Hoor, P.; Curtius, J.; Krämer, M.; Schiller, C.; Volk, C. M.; Homan, C. D.; Fierli, F.; Di Donfrancesco, G.; Ulanovsky, A.; Ravegnani, F.; Sitnikov, N. M.; Viciani, S.; D'Amato, F.; Shur, G. N.; Belyaev, G. V.; Law, K. S., and Cairo, F.: In situ measurements of tropical cloud properties in the West African Monsoon: upper tropospheric ice clouds, Mesoscale Convective System outflow, and subvisual cirrus, *Atmos. Chem. Phys.*, **2011**, 11, 5569-5590

Frey, W.: Airborne in situ measurements of ice particles in the tropical tropopause layer, PhD thesis, Johannes Gutenberg University Mainz, Germany, **2011**

Garrett, T. J.; Gerber, H.; Baumgardner, D. G.; Twohy, C. H., and Weinstock, E. M.: Small, highly reflective ice crystals in low-latitude cirrus, *Geophys. Res. Lett.*, **2003**, 30, 2132

Heymsfield, A. J.; Lewis, S.; Bansemer, A.; Iaquinta, J.; Miloshevich, L. M.; Kajikawa, M.; Twohy, C., and Poellot, M. R.: A General Approach for Deriving the Properties of Cirrus and Stratiform Ice Cloud Particles, *J. Atmos. Sci.*, **2002**, 59, 3-29

Heymsfield, A. J.: Properties of tropical and midlatitude ice cloud particle ensembles. Part I: Median mass diameters and terminal velocities, *J. Atmos. Sci.*, **2003**, 60, 2573-2591

Law, K. S.; Fierli, F.; Cairo, F.; Schlager, H.; Borrmann, S.; Streibel, M.; Real, E.; Kunkel, D.; Schiller, C.; Ravegnani, F.; Ulanovsky, A.; D'Amato, F.; Viciani, S., and Volk, C. M.: Air mass origins influencing TTL chemical composition over West Africa during 2006 summer monsoon, *Atmos. Chem. Phys.*, **2010**, 10, 10753-10770

Liu, L. and Mishchenko, M. I.: Constraints on PSC particle microphysics derived from lidar observations, *J. Quant. Spectrosc. Ra.*, **2001**, 70, 817-831

Noel, V.; Winker, D. M.; McGill, M., and Lawson, P.: Classification of particle shapes from lidar depolarization ratio in convective ice clouds compared to in situ observations during CRYSTAL-FACE, *J. Geophys. Res.*, **2004**, 109, D24213-

Vardiman, L.: The Generation of Secondary Ice Particles in Clouds by Crystal-Crystal Collision, *J. Atmos. Sci.*, **1978**, 35, 2168-2180

Yano, J.-I. and Phillips, V. T. J.: Ice-Ice Collisions: An Ice Multiplication Process in Atmospheric Clouds, *J. Atmos. Sci.*, **2011**, 68, 322-333

**Reply to:**
***Review of manuscript acp-2014-332***
***Tropical deep convective life cycle: Cb-anvil cloud microphysics from high altitude aircraft observations***
***by W. Frey et al.***

**by Anonymous Referee #2**

The authors would like to thank the anonymous referee #2 for his/her helpful comments and suggestions.
All issues raised by the referee are discussed below and have been incorporated in the revised version of the paper. The referee's comments are typeset in italic, our replies in normal font.

***General comment:***
*Observations of microphysical and optical properties of the tropical deep convection system Hector (Australia) at different stages of development (developing, mature, dissolving) and different altitudes are presented in the manuscript. In addition, the ratio of cloud to aerosol particle numbers are investigated for the various stages. The aim of the study is to analyse the microphysical evolution of Hector and the freezing mechanisms of the ice crystals.*
*The observational part of the paper is convincing and it can be seen from the interesting data set that the data analysis is performed quite thoroughly. However, the interpretation of the observations and the conclusions drawn with respect to the freezing mechanisms and cloud to aerosol ratio are confusing and seems to be not very well though out. This will be further decribed in the specific comments.*
*Thus, I am sorry that I must say that I find the paper not suitable for publication in it's present form. Even so, I like to encourage the authors to revise the manuscript since the topic of the paper is very timely and the unique high quality measurements at high altitudes in a deep convective system merit to be published. I hope that my comments will be helpful.*

**Reply to general comment:**
We have revised and changed the manuscript with particular focus on improving the explanations regarding the interpretation related to the freezing mechanisms. In addition we removed some of the statements which were also viewed as too speculative by the other reviewers. We hope that we were able to meet the reviewer's expectations thereby.

***Specific comments:***
***Abstract***
1. ***P2, line 2:*** *'...life cycle of clouds in a tropical deep convective system.' Would be better 'life cycle of the anvil region of clouds in a tropical deep convective system.'*

**Reply:** Maybe better 'life cycle of clouds in the anvil region of a tropical deep convective system'? We are focusing on the life cycle of the clouds, not on the life cycle of the anvil region.

2. ***P2, line 16:*** *'... indicating a change in freezing mechanisms.*
   *This cannot be understood here... and I think this formulation in general should be better 'indicating different freezing mechanisms'.*

**Reply:** Changed, however, it's not only a different freezing mechanism but also a change from one mechanism to another.

3. ***P2, line 18****: 'This is indicative for rapid glaciation during Hector's development.'*
   *Can you really derive this statement from your measurements? I would guess that the ice particles in developing phase are from ice nucleation at temperatures colder than -38C and not from frozen drops at warmer temperatures. More detailed comments are given later.*

**Reply:** Please see our replies to your later comments (reply to comments 21-28).

4. ***P2, line 18:*** *'The backscatter properties and particle images show a change from frozen droplets in the developing phase to rimed and aggregated particles.' ... in the mature phase ?*
   *See previous comment....*

**Reply:** We rephrased this sentence to:
"The backscatter properties and particle images show a change in ice crystal shape from the developing phase to rimed and aggregated particles in the mature and dissipating stages."

***1 Introduction***

5. ***General a:*** *I would shorten the introduction and discuss only points which are related to the work presented here. For example, heterogeneous chemical reactions on ice surfaces that lead to ozone destruction -or other chemical processes- don't need to be discussed, I think it is well known that those processes does not play an important role in the tropics. Further, also the argument that the observations can serve to evaluate models is not needed to make the study interesting.*
   *It would be enough to concentrate on the radiative impact of the anvil cirrus and also the water transport to the stratosphere.*
6. ***General b:*** *I recommend to give a short overview of the processes that could be responsible for the presence of ice crystals in the anvil, e.g. uplift of mixed phase clouds to higher regions ( (i) ice crystals could have formed by heterogeneous drop freezing or by freeezing of supercooled pure droplets at -38C -though I think the latter process is of lesser importance since in most cases the droplets evaporate by the Bergeron-Findeisen process at higher temperatures; (ii) formation of ice crystals at temperatures lower than -38C by homogeneous freezing of supercooled liquid solutions or heterogeneous deposition freezing).*
   *Without introducing the mechanisms that produce anvil ice crystals it is hard to understand the explanations that are given later in the paper to explain the observations.*

**Reply to 5. and 6.:** We included a paragraph about freezing mechanisms and shortened the remaining introduction.

7. ***P. 3, lines 13-15:*** *'In what manner clouds impact climate and chemistry critically depends on their microphysics, i.e. sizes and numbers of cloud particles, as as well as ice crystal shapes...'*
   *I would say: In what manner ice clouds impact climate critically depends on their microphysics, i.e. sizes and numbers as well as shapes of the ice particles.*

**Reply:** We rephrased as suggested.

8. ***P. 4, lines 12-14:*** *'Satellite and ground based remote sensing on the other hand are not able to obtain observations of microphysical properties.'*
   *This is a repetition of P.3, lines 22-23: Despite the amount of cloud observations from satellite or ground based instruments, those observations are unable to resolve the microphysical structures.*

**Reply:** We removed this sentence.

9. ***P. 4-5:*** *The paragraph about the modelling efforts and problems should be shortened.*
   *On the other hand, the statement on P.5, lines 11-13: 'However, the decay of a deep convective system may have major implications for the formation of subvisible cirrus (SVC), by affecting the background conditions e.g. regarding humidity.' could be explained in more detail, since this is a topic of the study.*

**Reply:** We think that the model problems should be mentioned since it shows that the processes behind the dissipation are not fully understood and thus, is one motivation for our study. Therefore, we only found little shortening potential. We hope that we were able to explain the implications of the dissipating stage for SVC formation in the according part of the introduction.

10. ***P. 5, line 24:*** *' ... convectively formed SVC.'*
    *How does convection produces SVC?*

**Reply:** Rephrased as follows:
"… and simulated the formation of SVC from remnants of deep convective clouds."

11. ***P. 5, lines 26-29:*** *'Thus, gaining more insight of the dissipating stage of deep convective systems, will also be helpful for understanding SVC formation, either from remnants of convection or facilitated through the changed background conditions.*
    *What are the 'changed background conditions'?*

**Reply:** For clarification we added:
"…changed background conditions regarding humidity and processed aerosols (after cloud dissipation)."

12. ***P. 6, lines 6-7:*** *'... is important from the perspective of the radiative budget and also of the satellite data retrieval and analyses.*
    *What is the importance for satellite data retrieval and analyses?*

**Reply:** We added the following before this sentence:

“Satellite observations may find cloud-free pixels next to cloudy pixels that are actually the twilight zone, i.e. containing undetectable clouds and aerosol. These areas show elevated reflectance and have been found to be not reliable for aerosol retrievals (Koren et al., 2007, Wen et al., 2006).”

***2 Experiment and instrumentation***

***13. P. 7, lines 19-21:*** *’The cloud particle data have been thoroughly filtered for shattering artefacts, following the interarrival time approach ...’*
*It would be convincing to show a figure displaying the PDFs of the interarrival times.*

**Reply:** A figure showing the interarrival times and the following text have been added:

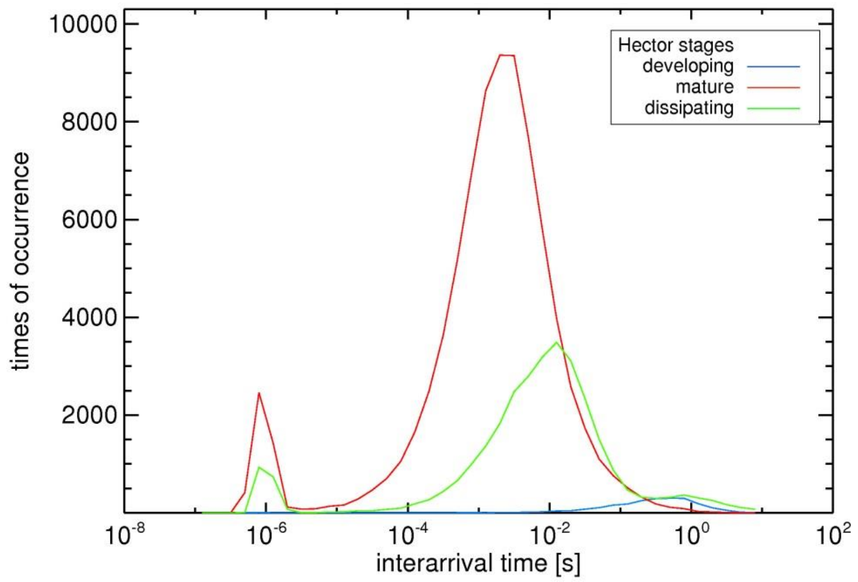


Fig. 1: Frequency distributions of interarrival times used to identify shattering artefacts in the CIP image data. The data are grouped into Hector classification stages as described in Sect. 4.1.

“Frequency distributions of interarrival times for the different Hector development stages (as outlined in Sect. 4.1) are shown in Fig. 1. Shattered particles can be clearly identified by the secondary peak around $10^{-6}$ s.”

***14. P. 7, lines 23-25:*** *’ ... comparisons of the cloud particle data from CIP and FSSP to lyman-alpha hygrometers ..., shattering was not a problem for these particular samplings of Hector clouds.*
*Agreement between IWCs from cloud particle probes and lyman-alpha hygrometers is not an argument versus shattering.*

**Reply:** As discussed in de Reus et al. (2009), we believe that the agreement between the lyman-alpha hygrometers and CIP and FSSP does prove that shattering is not an issue for these particular measurements. De Reus et al. used the data from exactly these two flights presented here plus an additional flight from the SCOUT-O3 campaign to compose their Figure 4, which demonstrates “closure” between the hygrometers and the water vapour instruments. We agree that this kind correlation would not necessarily -in general- prove no-shattering conditions. In clouds that contain higher number concentrations and larger particles (particularly if these have more complex shapes than observed here) shattering does introduce serious artefacts. By such artefacts the number concentrations of particles in the FSSP size range may be strongly affected, while at the same time the IWC is rather insensitive to errors in the FSSP range, because the IWC is mostly “generated” from the larger CIP-sized hydrometeors. However, in the figure by de Reus et al. (2009) the IWCs vary from $10^{-5}$ to $10^{-2}$ g/m³ and the colour-coding shows that for the lowest IWCs the values are sensitive to the FSSP counts as there were little or no large particles present. Significant shattering here would have resulted in a discrepancy between the particle and gas phase instruments. Thus, we believe this is a valid argument here and in a sense we were lucky with the encountered experimental conditions.

***3 30 November 2005 – description of the case***

***15. P. 9, line 21:*** *Please explain what the WRF and UM models are.*

**Reply:** The models have been introduced in the introduction; therefore, we would refrain from repeating this. However, we added the relevant references here in parenthesis.

***4 Microphysical evolution of Hector***

16. ***General comment:*** *I find the representation of the observations nicely descriptive, but explanations are missing.*

**Reply:** We accommodated explanations in the revised manuscript.

17. ***P. 13, line 10:*** *' ... mean values for ice water content (IWC), ...'*
    *Where does the IWC comes from?*

**Reply:** We added an explanation about the IWC in the Instrument section:
"An ice density of 0.917g/cm$^3$ 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."

18. ***P. 14, line 1:*** *' ... - The ambient temperature became warmer with increasing age of Hector.'*
    *It can be seen in Table 1 that not only the temperature became warmer but also RHice is above 100% in all levels except at 350-355K.*
    *I was really wondering how the Hector can develop from mature to dissipating in a warmer and supersaturated environment ???*
    *Vice versa at 350-355K, how can Hecture mature at RHice = 83% ???*

**Reply:** The RHi is generally close to saturation, especially when considering the measurement uncertainty. Therefore, we would count any RHi in about +/- 10% of saturation as saturation rather than sub- or supersaturated. Furthermore, it is known that supersaturation in cirrus clouds will not be removed immediately, but that RHi of up to and more than 200% have been found in cirrus (Krämer et al., 2009, Spichtinger and Krämer, 2013). In the dissipating stage ice particles sediment out of the cloud, which does not affect RHi in first instance. Warmer temperatures in the dissipating cloud decreases RHi at first but when reaching subsaturation ice crystals will evaporate and thus, a RHi around saturation would be expected.
The rather low RHi in the 350-355K level of mature stage could possibly be explained by entrainment of dry air from the side of the cloud.
We added an item clarifying this to the list in the manuscript.

19. ***P.14, 23-24:*** *' ... continental convection is generally thought to produce stronger updrafts and with this larger hydrometeors..'*
    *Why is that? Intuitively I would think that stronger updrafts produce more and smaller cloud drops in the mixed-phase temperature range as well as ice particles in the cirrus temperature range.*

**Reply:** This comparison was removed from the revised manuscript.

***5 Backscatter and aerosol measurements and their implication for freezing history***

20. ***General:*** *This section contains very long paragraphs without any break. The information about the freezing history is hidden in this long text segments. I recommend to introduce subsections and point out clearly the freezing histories of the different Hector stages.*
    *As you will see below, I don't agree with some of the hypothesis about the freezing mechanisms. Please consider these comments and discuss the possible explanations in more detail in the revised version of the manuscript.*

**Reply:** We revised this section and gave more detailed explanations. We also introduced subsections.

21. ***P.16, lines 16-18 :*** *'Thus, glaciation had already taken place before the observations in the developing Hector (all cases at T < 200 K), which judging from the satellite pictures was in it's first hour of development.*
    *'Glaciation' implies that the ice particles originate from the mixed-phase temperature region (about 4-5 km -or more- below) and stem from frozen drops.*
    *How can you know that? Couldn't they just as well have formed directly as ice in the cirrus temperature range starting about 3 km below?*

**Reply:** In the very high updraft speeds (some tens of m/s) that occur in the deep convective turrets, it would take only a few minutes to lift frozen drops by 5km. In case of pure heterogeneous freezing at somewhat higher altitudes we would expect a larger spread in the data. Certainly the ice particles have further grown by deposition (maybe also a minor contribution from riming) causing the broadening of the size distributions. Regarding the number concentrations, not all particles necessarily need to be transported into the upper cloud parts, some may have "escaped" in the turbulent environment before reaching higher altitudes. Photographs and visual observations from the ground throughout the day confirmed that the Hector established itself as a fully developed Cb. (See also Reply to Comment 28 below.) That is why we believe that the particles indeed stem from the mixed-phase part of the cloud and are not formed in situ.

**22. P.16, lines 19-22:** *'On the other side, the decreasing levels of depolarization with altitude for the mature and dissipating Hector case, that reflects a change in the average morphology of the particles, suggests an increasing role for the gravitational settling, riming and growth by accretion.*
*The latter part of the sentence is very speculative. Gravitational settling: couldn't it just be that the largest particles are not transported up to the highest levels by convective uplift? And I don't understand why riming and growth by accretion - which you assume- should be a reason for the decreasing levels of depolarisation.*

**Reply:** Here, what was meant was that a change in depolarisation reflects a change in the average morphology, which is licit to assume because for a change in depolarisation of the whole particle population at least some must have undergone microphysical processes. The sentence "riming and growth by accretion - - should be a reason for the decreasing level of depolarisation." is stronger and it is not what it is said in the text.

Riming and accretion lead to large particles which will first be removed from the upper cloud parts and later also from the lower cloud parts by sedimentation and precipitation. Additionally, less large particle might be transported into the higher cloud layers, as you point out correctly. Generally, the reviews made us aware –as we also mention in our reply to Reviewer Darrel Baumgardner- that we should add a paragraph explaining the interpretation of these particular depolarisation measurements. In general, for a given shape the depolarisation increases with the dimension of the particle (i.e. within the range of dimension not far from the wavelength, here 532nm) up to an asymptotic value (Liu and Mishchenko, 2001), which depends only on shape. Given the cloud particle dimensions, we are in the asymptotic range here! Thus, depolarisation will not increase with increasing cloud particle size. The asymptotic value depends on the particle shape, but in what manner is hardly predictable. For example in case of spheroids with an aspect ratio close to unity it can be shown that they have higher depolarisation than prolated or oblated spheroids. Plates and spheroids give similar depolarisation ratios, while columns have higher depolarisation ratios (Noel et al., 2004). It is, however, safe to say that when the depolarisation ratio of the cloud particle population changes, the average morphology of the cloud particle changes as well. Thus, coming back to the last part of your question, riming and growth by accretion will not decrease the depolarisation because of particle sizes, but if such particles are present in one cloud layer but not in another, this will certainly change the depolarisation. Clearly, in absence of detailed (detailed if not at all impossible) measurements of what happens inside the mixed phase region of the developing cloud such statements remain speculative. We added a sentence like this:
"In absence of good methods for the in situ detection of accretion and riming in the turbulent parts of Cb clouds the statements in this subsection remain speculative. Detailed numerical simulations of the cloud processes are needed for clarification. The same applies to the influence of rimed particles of various sizes on the detectable depolarisation, which could be simulated in a sensitivity study."

**23. P.16, lines 24-26:** *'Heymsfield et al. (2005) and Heymsfield et al. (2009) showed that in convective cells with strong updrafts supercooled cloud droplets reach the homogeneous nucleation level (at about -38C) and rapidly freeze there.*
*I understand Heymsfield et al. (2005) differently: in the mixed-phase temperature range mainly ice crystals from heterogeneous freezing exist at the lowest temperatures (the drops have evaporated due to the Bergeron-Findeisen process in most cases, see above). When the glaciated cloud is lifted to temperatures colder than -38C in weak updrafts, water vapour is depleted at the ice crystals so that RHice never reaches the freezing threshold for new homogeneous ice nucleation of supercooled solution particles (not activated droplets !). In strong updrafts, the water depletion can not compensate the increase of RHice up to the homogeneous freezing threshold and thus new ice crystals form.*
*A remark from my side: I think that the heterogeneous freezing threshold for deposition freezing in the cirrus temperature range -which is lower than the homogeneous freezing threshold- could be reached in both weak and strong updrafts.*
*By the way: the size distribution of frozen drops would look different than your observations, liquid cloud drops have a number concentration of around 100 cm$^3$ or more and sizes between 5 and less then 100 µm. The cloud particle number concentrations and size distributions of the developing Hector points more to ice nucleation (heterogeneous or homogeneous) at temperatures colder than -38C.*

**Reply:** Rephrased: "…supercooled cloud droplets MAY reach…"
You say "*In strong updrafts, the water depletion cannot compensate the increase of RHice up to the homogeneous freezing threshold and thus new ice crystals form."*

That means that liquid water droplets can reach this level, where they freeze homogeneously, which is how we understood the Heymsfield references.
We agree to your remark – about reaching heterogeneous freezing thresholds. Therefore, we add a sentence stating that as well the droplets could freeze at lower altitudes.
Since our measurements were performed well above the freezing level (at -70°C or lower) we think that on the way there not all ice crystals stay in the updrafts, reducing the number. Also, due to further cooling, inducing the increase of RHi, the crystals have the chance to grow via vapour deposition, thus broadening the size distributions.

***24. P. 17, line 12:*** *What do you mean with 'proper efficiency' ? I guess this means that the efficiency decreases with increasing size? Please specify.*
**Reply:** Yes, the sampling efficiency decreases with increasing size for particles larger 1 µm. We changed the text as follows:
"Since the sampling efficiency η of the COPAS inlet sharply decreases for particles larger than 1µm (i.e. η is about 100% for $D_p \leq$ 1µm but ranges below 30% for $D_p >$ 3µm) and aerosol number concentrations are much larger than the cloud number concentrations, the contribution of possibly counted cloud particles in the COPAS system are negligible."

***25. P. 17, line 17:*** *What do you mean with 'some effects of further processing'? Please specify.*
**Reply:** Outside the convective core clouds may have undergone some ageing. Thus, aerosol particles might be captured onto the ice surfaces or are released when ice particles sublimate. We changed the text as follows:
"In the anvil region (i.e. outside the main up- and downdrafts), where the measurements were obtained, some cloud ageing effects as the release of submicron aerosol by particle sublimation or losses of aerosol particles onto ice surfaces might apply to this estimate."

***26. P. 18, line 2-3:*** *'Both findings suggests that these cloud parts were formed under very similar conditions and underwent the same freezing mechanism within a short time.'*
*It is not clear which freezing mechanism you mean? But if you mean homogeneous freezing of supercooled droplets at -38C, I do not agree (see also the previous comment to P.16, lines 24-26).*
**Reply:** We cannot say what exact freezing mechanism took place just that it led to cloud particles with same morphology. We changed the text to:
"Together these findings suggest that the developing cloud parts were formed under very similar conditions and with a similar history of freezing within a short time."

***27. P. 18, line 9-10:*** *'The cloud particles in this stage have undergone some riming and aggregation, thus larger ice crystals were formed.'*
*Couldn't the large ice crystals have grown also by diffusional growth?*
**Reply:** Generally, the ice crystals could also have grown by diffusional growth but it is less effective in growing to large sizes (when the initial ice crystals are larger than 10µm, and here we are talking about particles with sizes even exceeding 1000µm). However, in the convective environment it is likely that aggregation and riming are playing the major role in particle growth. Furthermore, it would take much longer time to grow the ice crystals to observed sizes by diffusional growth compared to aggregation and riming. Also the particle images indicate rimed and aggregated particles, while diffusional growth would lead to other shapes, e.g. dendrites or columns, depending on the environmental conditions.

***28. P. 18, line 9-10:*** *'Ice multiplication processes as rime splintering (Hallett and Mossop, 1974) during the riming might be the reason for higher cloud particle concentrations ...'*
*The Hallett and Mossop ice multiplication process is large for temperatures between -12 and -16C, a maximum occurs at -5C (enhancement of particles by a factor of $10^4$ to $10^5$ ), but the enhancement reaches unity at a cloud temperature of -20C. So I cannot imagine that this is the reason for the observations of higher ice crystal numbers in the mature Hector stage.*
**Reply:** Our idea was that the strong updrafts would carry the particles formed in the lower cloud parts into the upper cloud parts. The reviewer is right in questioning whether under such strong updrafts the conditions for Hallett-Mossop process would be met. However, other ice multiplication processes can occur at higher altitudes (Vardiman 1978, Yano and Phillips, 2011), when ice particles hit other ice particles, which is well possible in the turbulent conditions. We changed the statement accordingly in the revised manuscript.

*What about the speculation that the developing and dissipating stages are cirrus that formed in-situ, while the mature Hector represents the lifted mixed cloud from below that reached the high altitudes during the time of maximum updraft?*
*Only an idea ...*

**Reply:** We could visually observe the development of the Hector cloud from different ground locations in and around Darwin throughout the whole day of November 30, 2005. The cloud started to form at lower altitudes but grew quickly into the vertical direction. At that stage no cloud layer was present prior to the convective turret. Also, the satellite IR and optical depth pictures do not indicate the a-priori presence of cirrus. The pilot of that flight did not report clouds in those altitudes prior to Hector in his flight debrief. (The pilots were requested for this campaign to take notes of such observations; nowadays one would have a GoPro camera in the cockpit.) Photographs of the clear sky from the ground sites were not taken before Hector developed. But pictures from the developing and mature Hector exist as well as from the early dissipation phase (that is before darkness set in).

Photographs of Hector during its development. Taken by Stephan Borrmann in Darwin. The time steps are from top to bottom:
12:48LT
12:58LT
17:19LT
18:29LT
Thus, the first two images were taken before measurements are available. Here, you can see that still some water remains present at the cloud top.
The third image was taken towards the end of Hector's mature stage, where the anvil is radially flowing out.
The last images was taken before take-off for the second flight, during the measurements of the dissipating stage it was already dark, so no images are available from that stage.

As the analysis of area ratio (please see the reply to General comment #1 of Reviewer 1) shows, there is a great similarity between the mature and dissipating cloud. Thus, we think that these crystals (larger 125µm) are aging crystals from the mature stage. However, the small particles are not included

in this analysis and the depolarisation of the dissipating stage is quite different to that of the mature clouds. This might indeed be a hint for new particle formation. In that case, the smaller particles, or a subset of them, would be newly formed while the larger are leftovers from the mature stage. (As pointed out in Comment 18, the measurements here show a more or less saturated environment, which would not support nucleation unless some nucleation had occurred prior to the measurement in a then supersaturated environment.)
The last point here has been added to the revised manuscript at the end of Section 5.1.

***29. P. 19, line 4 and other places:*** *'aerosol to cloud particle ratio'*
*Isn't it cloud to aerosol particle ratio ? Otherwise the numbers must be much larger ....*
**Reply:** The reviewer is correct; we changed to 'cloud to aerosol ratio' accordingly.

***Conclusions***

***30. P.21, 4-14:*** *'Furthermore it gives indications for a change in freezing mechanisms with increasing time of Hector: the developing Hector shows very similar aerosol to cloud particle ratios and cloud particle morphology, indicating a rapid freezing under similar conditions, as homogeneous freezing.'*
*Which homogeneous freezing do you mean (see my earlier comments) ?*
**Reply:** We removed "homogeneous freezing"
***b)*** *'The mature Hector cases show rimed ice crystals and some chain aggregates, higher aerosol to cloud particle ratios, thus, a change to riming, contact freezing, and aggregation.'*
*With respect to riming and aggregation see my earlier comments - it should be very clear here that this is speculative. Contact freezing, where is that mentioned?*
**Reply:** Contact freezing was mentioned on page 19, line 12, however we removed it here also due to the wishes of Reviewer 1.
***c)*** *Maybe I have overseen it in the long section 5. ... so please restructure this section and line out how contact freezing can explain the many large ice crystals of the mature Hector stage.*
**Reply:** Contact freezing does not explain the many large ice crystals, but may explain the change in cloud to aerosol particle ratio (as stated in Section 5, which now has been restructured).
***d)*** *'In the decaying stage Hector shows a wide variety of aerosol to cloud particle ratios, and the cloud particles have a simpler morphology than the particles in the mature stage, which might be an effect of ageing. Due to the varying aerosol to cloud particle number ratio, these results show that the development stage of the convective cloud system has an impact on the activation ratio and thus has to be taken into account. '*
*I am not convinced about the discussion of the activation ratio (cloud to aerosol particle ratios). What does that mean - the cloud system has an impact on the activation ratio?*
**Reply:** We rephrased to:
"In the dissipating stage Hector shows a wide variety of cloud to aerosol particle ratios, which might be an effect of ageing. Furthermore, according to the area ratio analysis the cloud particles have a similar shape as the particles in the mature stage, also indicating ageing. However, the depolarisation ratios of the dissipating and mature stages differ. Thus, it is valid to speculate that small ice crystals may have nucleated in situ in the ageing cloud."
Furthermore:
"These results show that the cloud to aerosol particle ratio varies with the development stage of the convective cloud system and thus the cloud's development stage has to be taken into account in aerosol-cloud interaction studies."

<u>References:</u>

Baker, B. and Lawson, R. P.: Improvement in Determination of Ice Water Content from Two-Dimensional Particle Imagery. Part I: Image-to-Mass Relationships, *J. Appl. Meteorol. Clim.*, **2006**, 45, 1282-1290

de Reus, M.; Borrmann, S.; Bansemer, A.; Heymsfield, A. J.; Weigel, R.; Schiller, C.; Mitev, V.; Frey, W.; Kunkel, D.; Kürten, A.; Curtius, J.; Sitnikov, N. M.; Ulanovsky, A., and Ravegnani, F.: Evidence for ice particles in the tropical stratosphere from in-situ measurements, *Atmos. Chem. Phys.*, **2009**, 9, 6775-6792

Koren, I.; Remer, L. A.; Kaufman, Y. J.; Rudich, Y. and Martins, J. V.: On the twilight zone between clouds and aerosols, *Geophys. Res. Lett.,* **2007**, 34, L08805

Krämer, M.; Schiller, C.; Afchine, A.; Bauer, R.; Gensch, I.; Mangold, A.; Schlicht, S.; Spelten, N.; Sitnikov, N.; Borrmann, S.; de Reus, M. and Spichtinger, P.: Ice supersaturations and cirrus cloud crystal numbers*, Atmos. Chem. Phys.*, **2009**, 9, 3505-3522

Liu, L. and Mishchenko, M. I.: Constraints on PSC particle microphysics derived from lidar observations, *J. Quant. Spectrosc. Ra.*, **2001**, 70, 817-831

Noel, V.; Winker, D. M.; McGill, M., and Lawson, P.: Classification of particle shapes from lidar depolarization ratio in convective ice clouds compared to in situ observations during CRYSTAL-FACE, *J. Geophys. Res.*, **2004**, 109, D24213-

Spichtinger, P. and Krämer, M.: Tropical tropopause ice clouds: a dynamic approach to the mystery of low crystal numbers, *Atmos. Chem. Phys.*, **2013**, 13, 9801-9818

Vardiman, L.: The Generation of Secondary Ice Particles in Clouds by Crystal-Crystal Collision, *J. Atmos. Sci.*, **1978**, 35, 2168-2180

Wen, G. Y.; Marshak, A. and Cahalan, R. F.: Impact of 3-D clouds on clear-sky reflectance and aerosol retrieval in a biomass burning region of Brazil, *IEEE Geosci. Remote S.*, **2006**, 3, 169-172

Yano, J.-I. and Phillips, V. T. J.: Ice-Ice Collisions: An Ice Multiplication Process in Atmospheric Clouds, *J. Atmos. Sci.*, **2011**, 68, 322-333

**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.

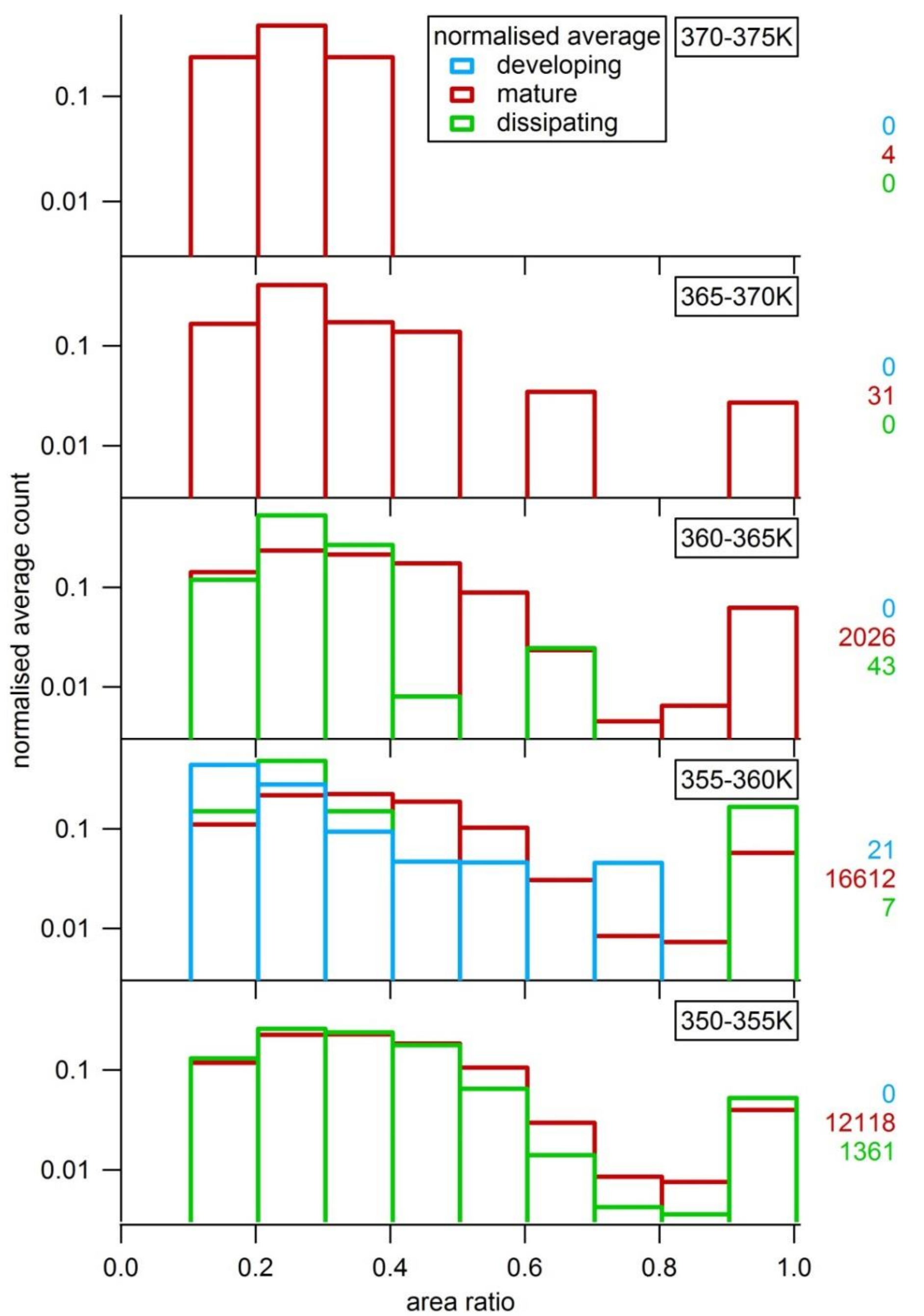


Figure 1: Normalised histograms (to a total value of 1) of area ratio occurrence. The number of analysed particles is given by the coloured numbers on the right.

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


References:

Baker, B. and Lawson, R. P.: Improvement in Determination of Ice Water Content from Two-Dimensional Particle Imagery. Part I: Image-to-Mass Relationships, *J. Appl. Meteorol. Clim.*, **2006**, 45, 1282-1290

de Reus, M.; Borrmann, S.; Bansemer, A.; Heymsfield, A. J.; Weigel, R.; Schiller, C.; Mitev, V.; Frey, W.; Kunkel, D.; Kürten, A.; Curtius, J.; Sitnikov, N. M.; Ulanovsky, A., and Ravegnani, F.: Evidence for ice particles in the tropical stratosphere from in-situ measurements, *Atmos. Chem. Phys.*, **2009**, 9, 6775-6792

Frey, W.; Borrmann, S.; Kunkel, D.; Weigel, R.; de Reus, M.; Schlager, H.; Roiger, A.; Voigt, C.; Hoor, P.; Curtius, J.; Krämer, M.; Schiller, C.; Volk, C. M.; Homan, C. D.; Fierli, F.; Di Donfrancesco, G.; Ulanovsky, A.; Ravegnani, F.; Sitnikov, N. M.; Viciani, S.; D'Amato, F.; Shur, G. N.; Belyaev, G. V.; Law, K. S., and Cairo, F.: In situ measurements of tropical cloud properties in the West African Monsoon: upper tropospheric ice clouds, Mesoscale Convective System outflow, and subvisual cirrus, *Atmos. Chem. Phys.*, **2011**, 11, 5569-5590

Garrett, T. J.; Gerber, H.; Baumgardner, D. G.; Twohy, C. H., and Weinstock, E. M.: Small, highly reflective ice crystals in low-latitude cirrus, *Geophys. Res. Lett.*, **2003**, 30, 2132

Heymsfield, A. J.: Properties of tropical and midlatitude ice cloud particle ensembles. Part I: Median mass diameters and terminal velocities, *J. Atmos. Sci.*, **2003**, 60, 2573-2591

Vardiman, L.: The Generation of Secondary Ice Particles in Clouds by Crystal-Crystal Collision, *J. Atmos. Sci.*, **1978**, 35, 2168-2180

Weigel, R.; Hermann, M.; Curtius, J.; Voigt, C.; Walter, S.; Böttger, T.; Lepukhov, B.; Belyaev, G., and Borrmann, S.: Experimental characterization of the COndensation PArticle counting System for high altitude aircraft-borne application, *Atmos. Meas. Tech.*, **2009**, 2, 243-258

Yano, J.-I. and Phillips, V. T. J.: Ice-Ice Collisions: An Ice Multiplication Process in Atmospheric Clouds, *J. Atmos. Sci.*, **2011**, 68, 322-333