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.

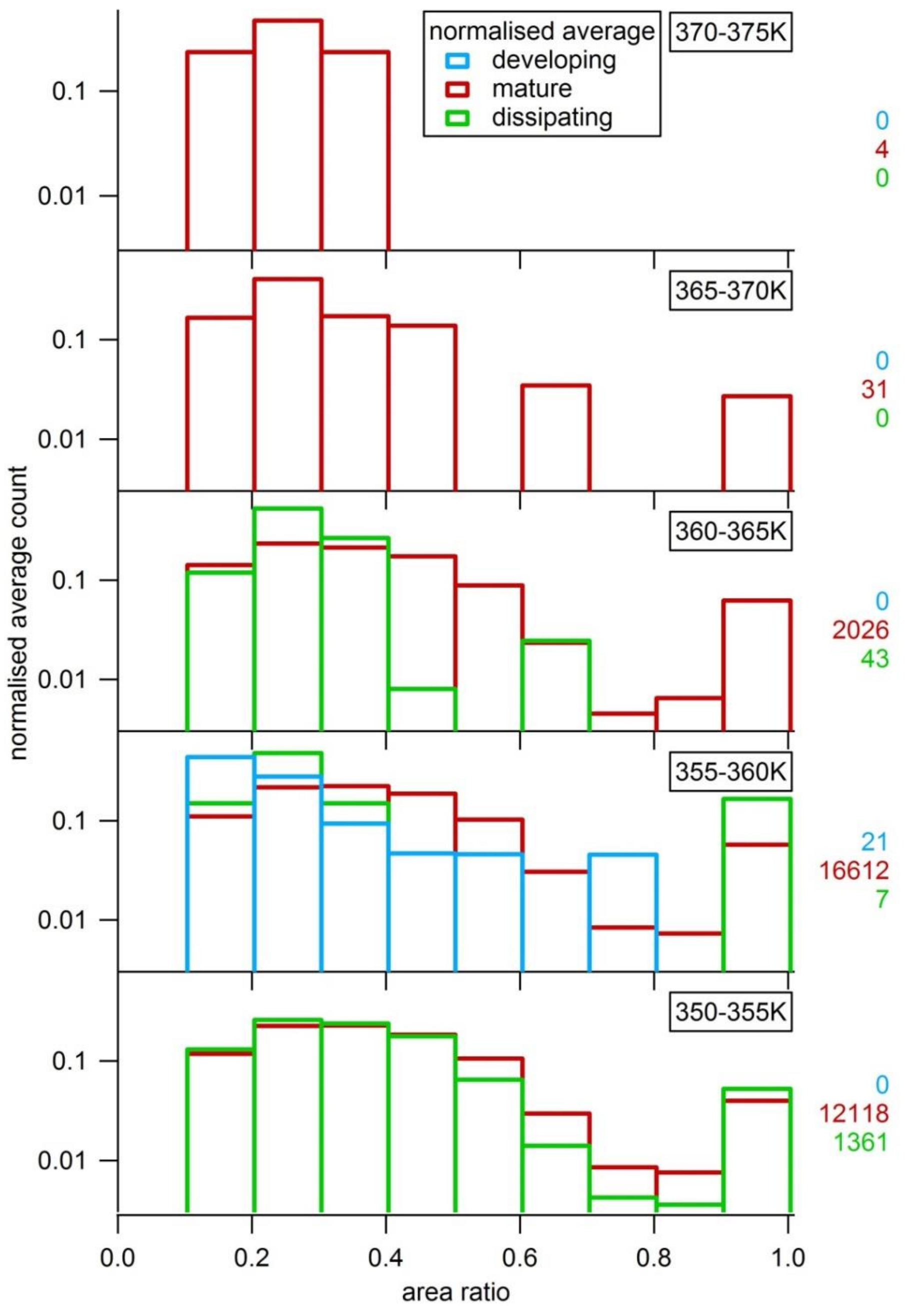

Figure 1: Normalised histograms (to a total value of 1) of area ratio occurrence. The number of analysed particles is given by the coloured numbers on the right hand side.

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.

Specific comments:
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.

Introduction:
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.

Section 2:
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 $0.917 g/cm^3$ 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 $0.917 g/cm^3$ 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.

<u>Section 3:</u>
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