Reply to:
Second 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.

We would like to thank the reviewers again for their work and help to improve/polish the manuscript. Also, we apologize for the extra work we caused here. Some of the issues addressed will remain open -and we try to point these out better-, but to really resolve the questions a detailed numerical simulation of the cloud system would be necessary. This unfortunately is beyond our reach at this time. Please find below our replies to the specific comments. All our replies are written in green.

Anonymous referee #3:
The paper is improved and provides new and useful information on convective cloud development. I find it acceptable in its current form, subject to the technical corrections given below:
p. 21, section on "In-situ Nucleation": Please make clear that this refers to nucleation of ice crystals, not aerosol particles.
Reply: We changed the heading to 'In situ ice particle nucleation in aged anvil' and also changed 'particles' to 'ice particles' at several instances in this paragraph.

Fig. 11: Please specify aerosol and cloud particle size range in the figure caption, as that is important in interpreting it.
Reply: We added the following sentence in the Figure caption:

> "The size ranges are 2.7 to 1600µm for the cloud particles and larger 15nm for the aerosol particles (up to approximately 1µm)."

Also we added a sentence concerning the upper detection limit in Section 2.2 'Submicron aerosol number concentration':

> "The upper size detection limit is largely determined by the sampling inlet which becomes inefficient for particles with sizes above 1µm (Weigel et al., 2009)."

Anonymous referee #2:
Reviewer General Comment 1:
For convenience, I'll write my new comments in blue behind the author's replies.
However, one new general remark in advance: I found it not very convenient to read the author's reply since the layout was in a way that makes it difficult to get the message of the text. The lines are very long and the space between lines and paragraphs small. In addition – as you will see in the comments – often it is not clear what is changed in the manuscript, and a manuscript with tracked changes is not provided. Comparing the two versions of the papers to find out what has changed was difficult. The effect is that one needs long time to work through the material and at a certain point lose patience/interest ... I like to suggest here that the authors make it more easy for the referees to revise their material in the next version.
Maybe this is a somewhat untypical comment, however, I think it is important to make the review as easy as possible for the referees who spend time in reading papers and writing recommendations.

Reply:

Sincere apologies here, and we see the point clearly. (SB is in particular sympathetic with the reviewer's remark as he fairly often runs into the same problem when reviewing manuscripts.) We had thought the layout of the reply with indent/noindent for comments and replies, plus the numbering, would help to lead through the reply. A certain problem also arose with this particular manuscript from the fact that sometimes two reviewers addressed the same parts of the text with different remarks. Then we changed the text trying to accommodate both criticisms, and as result it may be difficult afterwards to identify which remark of which reviewer caused which change. Then, one reviewer provided his/her comments as annotations in form of little yellow comment bubbles inserted right into the pdf file of the submitted manuscript. This was not easy to handle in terms of making our changes traceable, especially when using the LaTeX typesetting of ACP. So, we are sorry for causing this inconvenience to all reviewers, which certainly was not on purpose.

It might be a suggestion to ACP to explore ways of a more standardised and simplified review-reply-tracking scheme, where (1) reviewers and authors alike are requested to adhere to a certain formalised, transparent procedure, and (2) where initial submission of the manuscript and revised version are juxtaposed automatically with highlights of the changes, possibly in different colours for the different reviewers. The editing capabilities of Copernicus should allow for this.

Here now we prepared a manuscript version with colour-marking for new/changed text: blue are the changes between the ACPD and the first revised manuscript versions, green are those changes added after this second revision. In our next submission, for sure, we will be mindful of this issue and the reviewer's perspective.

Reviewer General Comment 2:

The revised manuscript has vastly improved. However, I find that it needs a final polishing to be scientifically sound. Particularly, I am still not completely happy with the discussion of the freezing mechanisms. For further details see the specific comments.

Reply:

Please find our responses below your new comments.

General comment from the authors:

From this second review it becomes clear that there is still some confusion around the involved freezing processes. Particularly, the reviewer and the authors here disagree on how the papers Heymsfield et al. (2005) and Heymsfield et al. (2009) are meant in respect to where / at what temperatures freezing happens (please find more details in the replies to the specific comments). We have taken care now in this revision to make clear better, whether we refer to freezing processes at T>-38°C or at T<-38°C.

Specific comments:

Abstract

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.
New comment 1:
Can you specify this ?
Reply:
What was meant is that if the developing Hector forms under freezing mechanism a, and the mature under freezing mechanism b, then it is not only a different freezing mechanisms, but also the freezing mechanism changed from time of freezing during the developing stage and time of freezing during the mature stage.

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 (you don't say how the shape in the developing phase is) to rimed and aggregated particles in the mature and dissipating stages.”
New comment 2:
(you don't say how the shape in the developing phase is)
Reply:
That is correct: The particle images (CIP) obtained during the developing phase are too small to distinguish specific shapes. From the backscatter sonde (MAS) observations we can see a change in morphology, again we cannot distinguish specific shapes from backscatter measurements. Thus, we can only say that there is a change to rimed and aggregated particles, but not from what shape in the developing phase. We added a remark in the abstract:

> “…, the specific shape of particles in the developing phase cannot be distinguished from the measurements.”

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.
New comment 3, to old comment 5 and 6: I don't see that these points are adequately adapted, especially **point 5**. The introduction is still very long and too much material is presented to point out the focus of the study. Nearly 4 pages … and with only few paragraphs, still hard to read.
Reply:
We did shorten the introduction, but due to the new paragraph about freezing (as rightfully requested by the reviewer) it has been extended by the same amount. We think that the aspects covered in the introduction are all relevant (deep convection, TTL, freezing, problems in modelling, SVC, dissipating cloud). This time we only found very little shortening potential (a few sentences). In the type-set version of ACP the introduction is now one full page containing two columns plus one column of the second page, i.e. a total of three columns. SB looked at a couple of other papers including one he currently reviews, and he found that our introduction in the end is not really longer than the others. In particular there are 17.5 columns of results plus 2.5 columns for the instrument description and 1.5 columns with the conclusions. In terms of journal space we do not really see an imbalance between introduction and results. Another aspect is that we tried to mention/include/adequately cite the relevant literature which takes space.

By the way, it would have been very helpful to have a manuscript with tracked changes– the new review would have been ready much earlier ...

With respect to **point 6**, a paragraph is included but the explanations about the freezing processes remain puzzling. Below you find some new comments on the paragraph included in the new manuscript:

a) P 4, line 9-10: '... others show the possibility of supercooled liquid water to reach the homogeneous freezing threshold in case of strong updrafts (Heymsfield et al., 2009). ' better '... others show the possibility of supercooled liquid water to reach the homogeneous drop freezing temperature in case of strong updraft (Heymsfield et al., 2009).
See comment d) for explanation.
Reply:
Changed according to the reviewer's suggestion. We do thank the reviewer here for providing us with better formulations.

b) - P 4, line 10-12: 'This is possibly due to shorter transit times in the stronger updrafts that allow the liquid drops to reach higher altitudes. '
Here you mean the mixed phase temperature region, yes? And
Yes, T>-38°C

- P 4, line 12-: 'When pre-existing ice is present (e.g. by entrainment from downdrafts or uplift of heterogeneously frozen ice from lowe levels), homogeneous freezing can be suppressed.

this is below -38C, yes (pre-exeisting ice only makes sense in this temperature range)?
This should be clear to the reader, please note.

Reply:

No, in this second part we still talk about pre-existing ice at T>-38°C. Ice can pre-exist here from heterogeneous freezing or inmixing/entrainment from downdrafts (that originate above, i.e. T<-38°C).
We added:

> "When pre-existing ice is present at T>-38°C…".

c) P. 4, lines 13-15: 'However, in strong updrafts the pre-existing ice might be unable to cause depletion of water vapour and suppressed droplet activation (Heymsfield et al., 2005).'
Below -38C no droplets can be activated. Better: 'However, in strong updrafts the preexisting ice might be unable to deplete enough water to suppress new ice nucleation by homogeneous freezing of soluble solution particles (Heymsfield et al., 2005).

Reply:

Heymsfield et al. (2005) analyse observations taken at temperatures between -33°C and -36°C (e.g. stated in the first sentence in their abstract). In this temperature range, the authors talk about homogeneous ice particle nucleation and not about freezing of solution particles, which becomes only important at lower temperatures. Thus, we think that the statement here should be 'droplet activation', which also is a formulation used in that paper.

d) P. 4, lines 15-16: 'The role of homogeneous freezing may change throughout the cloud life time (e.g. being more important in young updrafts), ... '
I am not sure if the role of the two homogeneous freezing processes
(i) spontaneous homogeneous freezing of liquid clouds drops at -38C and
(ii) homogeneous freezing of supercooled solution particles at temperatures < - 38C (dependent on the RH_ice freezing threshold) becomes clear to the reader. Please explain.

Reply:

Indeed, so far we were only talking about freezing at T>-38°C. We 'screened' the whole manuscript for instances where this is unclear and tried to remove those passages. Also, we now write in the introduction, highlighting the possibility of freezing at colder temperatures:

> "At lower temperatures (T<<-38°C) in situ formation of ice particles can occur by freezing of solution droplets (Koop et al., 2000). As air is pushed upwards, sufficiently low temperatures and high relative humidities can be reached."

e) P. 4, lines 17-18: ' Once the deep convective cloud ages and the anvil loses a large fraction of its IWC by sedimentation, new ice nucleation can occur, ...'
This is the same process as in lines 13-15: the supersaturation can reach the homogeneous freezing threshold in-spite of already existing ice, in one case by high updraft, in the other case by the loss of ice due to sedimentation. Please explain.

Reply:

We added:

> "Thus, eventually not enough ice particles are left to reduce water vapour excess and suppress homogeneous freezing, as in the high updraft scenario mentioned above."

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.
New comment 4: see new comment on old comment 5.
Reply:
In our reply to new comment 3 above we try to justify how we proceeded handling the need for shortening on one side and request for more explanation on the other. In particular for the SVC formation we referenced the papers Jensen et al., 1996; Thomas et al, 2002, and Jensen et al., 2013, towards the end of the introduction. We believe repeating some of their results here would again expand the paper providing not much more insight. Actually, in order to assess whether the observed layers indeed could thin out into SVC would require a model study on the given data.

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.
New comment 5: Sorry, I cannot follow this argumentation. Shattering of large ice crystals will shift the ice crystal fragments in smaller FSSP+CIP bins. Summing up all bins will give the same IWC as without shattering, since the mass of the ice crystals is preserved.

In other studies (I forgot the reference...) the IWC from ice particles larger and smaller 100 micron is used to investigate the influence of shattering, which makes more sense since the IWC from crystals < 100 micron will be enhanced by shattered ice fragments while the IWC from crystals > 100 micron will be downscaled.

By the way: which lyman-alpha hygrometers were used for the comparisons?

Reply:

The FISH and FLASH lyman-alpha hygrometers were used, i.e. instruments well represented in the literature. Certainly all shattered fragments together have the same IWC as the original big particle. However, to see this in the data, all fragments would have to fly through the optical measurement volume to be detected and reveal the same IWC as the original particle. This is very unlikely because the optical volume is so small. Shattering on a probe tip will cause the shattered fragments to spread out in a lot of directions – thus only a subset of the fragments will be detected by the instrument. Therefore, the shattered IWC will be smaller than it would be without shattering. This will result in discrepancy between the lyman-alpha instruments and CIP/FSSP.

The paper the reviewer refers to might be the Jensen et al. (2009) study, where a correlation between large ice particle IWC and small particle number concentrations is adopted. We would like to note that that study uses data obtained at much lower altitudes (up to 12km) than our study. Here, a large proportion of large agglomerated ice particles can be expected – which naturally lead to more shattering. The conditions for our measurements were different as the measurements were performed at much higher altitudes with lower temperatures.

Generally, we carefully monitored publications on the shattering issue. Furthermore, we discussed the shattering problem in detail in the publications by (1.) de Reus et al., 2009 (which also includes data used here and the same comparison between the lyman-alpha instruments and CIP/FSSP) and (2.) in the supplement to Frey et al. (2011). During the peer review of both papers substantial discussion took place on potential shattering. Both studies are mentioned in the manuscript at the point of shattering and measurement uncertainty discussion, so we believe this should be sufficient to lead the interested reader to the different arguments.

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.

New comment 6a: Would be good to write this in the manuscript.

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 (Kramer et al., 2009, Spichtinger and Kramer, 2013). In the dissipating stage ice particles sediment out of the cloud, which does not affect RHi in first instance.

New comment 6b: Would be good to write this in the manuscript.

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.
New comment 6c: → where ??
Reply to new comment 6a, 6b, 6c:
This is all stated in an item added to the list in Section 4.1 (now with more information related to 6b):

> "The RHi in the mature and dissipating stages is generally close to saturation, considering the measurement uncertainty (12–17%). 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. Supersaturation in ice clouds will not be removed immediately, but RHi of up to 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 decrease RHi at first but when reaching subsaturation ice crystals will evaporate and thus, a RHi around saturation would be expected."

5 Backscatter and aerosol measurements and their implication for freezing history

New comment 7: the section reads much better now. One new suggestion to the 'hidden freezing history' : change the title of 'Depolarisation ratio' to something like 'Freezing history from depolarisation ratio'.
Reply:
We changed the heading to:

> "Freezing history revealed from depolarisation ratio"

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 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,

New comment 8a: → No, since I wrote about the temperature range < -38C .
Reply:
In our reply to new comment 3c we point out that the Heymsfield paper only uses data from T>-38°C, thus, liquid water droplets can potentially be present here. And in our paper we also refer to T>-38°C.

where they freeze homogeneously, which is how we understood the Heymsfield references.

New comment 8b: → I think that Heymsfield meant that homogeneous ice nucleation of supercooled solution particles at the RH_ice freezing threshold may occur in strong updrafts at < -38C (liquid water droplets do not exist here! They freeze spontaneously and homogeneously at -38C) and produces new ice crystals, fewer than liquid cloud droplets in warm and mixed phase clouds.
Reply:
In Heymsfield et al., 2005, observations ranging from -33°C to -36°C are reported and should thus inherently treat homogeneous water droplet freezing. Regarding the Heymsfield et al., 2009, paper, this is highlighted by the following statements taken from that paper:
"The central question we address here is this: Under what conditions will a portion of the cloud droplet concentration transported upward in updrafts avoid being accreted and/or evaporated by the growing ice particle population before reaching the level of -38°C where droplet freezing occurs spontaneously?" (introduction), *"This section further investigates the presence and significance of droplets lofted by the updrafts to temperatures required for homogeneous ice nucleation. Using the results from section 3 as input into a parcel model, we calculate the updraft velocities required for a population of droplets with given LWC to be lofted from -20°C to -38°C."* (discussion), *"A 1D parcel model based on our observed vertical profiles of ice water content and exponential size distributions fitted to measured values was developed to examine the fate of cloud water as it is lofted from the -20°C to -38°C levels where water droplets will homogeneously freeze."* (conclusions).
Indeed, Heymsfield et al. (2009) also mention nucleation from solution droplets two times (on 33 pages) but it is not the freezing process the authors are focussing their study on. Furthermore, homogeneous nucleation of ice from solution droplets will occur at (much) lower temperatures (than -38°C) and actually cause small ice particles (up to a few tens of µm, e.g. Gallagher et al., 2012), whereas our measurements also show larger ice particles (>100 µm) at the lower temperatures. These would, presumably, have formed by a different freezing mechanism.

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.

New comment 8c: The sentence is good, though it was not what I meant. I really like to suggest to carefully distinguish between heterogeneous and homogeneous drop freezing in the mixed phase cloud temperature range and heterogeneous and homogeneous freezing of solution particles in the cirrus temperature range. In my first comment, I meant the latter ones which might occur in addition to the frozen drops from below and produce a new mode of ice crystals.

Reply:
We now tried to pay particular attention to distinguish freezing of water versus solution droplets in the revised manuscript version since we agree on the careful distinguishing point. Hopefully the second revised version of the manuscript reflects this where we added:

> "As a second possibility, homogeneous freezing of particles at T<-38°C from solution droplets on top of Hector cannot be ruled out completely. This should also lead to ice particles with similar depolarisation, due to the narrow band of conditions under which the freezing would happen. However, such in situ formed particles would be expected to have sizes of only up to a few tens of µm (e.g. Gallagher et al., 2012), whereas sizes in the developing stage exceed 100 µm."

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.
New comment 9: Now I got somehow tired to search in the new manuscript if something of the author's answer has gone into the text ..... without a manuscript with tracked changes and/or notes here in the author's reply about the changes it is really hard to follow the new manuscript in comparison to the first.
In the meantime I got a reminder from the editor that my review is behind time. The reason is that I needed too much time to track the changes by myself. And, again, I'm somehow tired to do this. So I'll give only short comments on the remainder of the points and leave it as a task for the next version of the paper to make it easier for the referees to track the changes they have made.
Reply:
We added:

> "…, thus larger ice particles were formed as growth by diffusion plays a minor role for growing particles to those sizes."

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.

New comment 10: Where can I find that or how is the new statement?
Reply:
Section 5.2, p24 under the subheading 'Mature stage'

> "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."

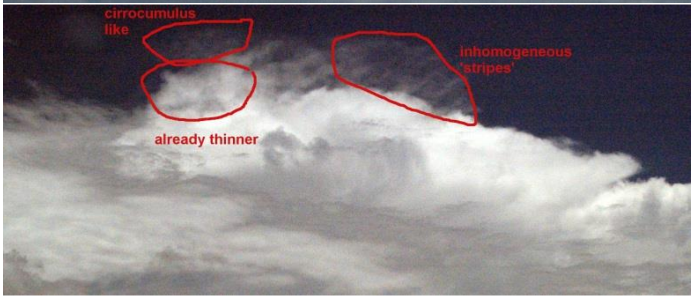
What about the speculation that the developing and dissipating stages are cirrus that formed in-situ, while the mature Hector represents the lifted mixed cloud from below that reached the high altitudes during the time of maximum updraft?
Only an idea …
Reply: We could visually observe the development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

New comment 11: Where can I find that or how is the new statement?
Reply 11a:
At the end of Section 5.1 you find a new paragraph: 'In situ ice particle nucleation in aged anvil'

A remark to this picture: I think here the difference between the upper Hector part (developing?) and the part below (mature Hector ?) is visible. The upper part looks like an in-situ formed cirrus (since the complete air mass is lifted, also the part with temperatures < -38C is affected), the part below looks like a glaciated mixed-phase cloud. Both parts might be lifted in the mature state to higher altitudes.
Reply:
Understandable argumentation. However, we think it remains speculative when considering the following: 1) the picture was taken from ground, not from the aircraft. Thus, the different-looking parts may rather spread out to the side than atop of the cloud. 2) When comparing e.g. with the picture of a Pileus cloud as in Garrett et al. (2006, Fig. 1 and also pileus and TTL cirrus in Fig. 2), the cloud here looks very inhomogeneous, while the Garrett et al. pileus cloud, and e.g. pileus clouds in the Mainz cloud gallery (see references for link), look rather uniform. 3) This is just one snapshot – to be able to properly identify if this cloud formed in situ or as part of an outflow (flowing out to the side) one would have to look at movies, or more consecutive snapshots, which, unfortunately, are not available. (As SB noted in his diary, commenting the series of photographs. According to his notes he quit taking pictures because Hector did not grow any further as far as one could see from the ground.)

zoom into upper cloud parts



picture with enhanced white balance

Please also see our Reply to comment 8c.

Conclusions

New comment 12: The new conclusions looks different than the old, but it is hard for me to follow if the points are answered. Also here I would need to see the changes tracked ...

Reply:

You will now find the changes coloured in blue.

References:

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., 9, 6775–6792, doi:10.5194/acp-9-6775-2009, 2009.

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., 11, 5569–5590, doi:10.5194/acp-11-5569-2011, 2011.

Gallagher, M. W., Connolly, P. J., Crawford, I., Heymsfield, A., Bower, K. N., Choularton, T. W., Allen, G., Flynn, M. J., Vaughan, G., and Hacker, J.: Observations and modelling of microphysical variability, aggregation and sedimentation in tropical anvil cirrus outflow regions, Atmos. Chem. Phys., 12, 6609–6628, doi:10.5194/acp-12-6609-2012, 2012.

Garrett, T. J.; Dean-Day, J.; Liu, C.; Barnett, B.; Mace, G.; Baumgardner, D.; Webster, C.; Bui, T.; Read, W. & Minnis, P. Convective formation of pileus cloud near the tropopause Atmos. Chem. Phys., 6, 1185-1200, doi:10.5194/acp-6-1185-2006, 2006.

Heymsfield, A. J.,Miloshevich, L.M., Schmitt, C., Bansemer, A., Twohy, C., Poellot,M. R., Fridlind, A., and Gerber, H.: Homogeneous ice nucleation in subtropical and tropical convection and its influence on cirrus anvil microphysics, J. Atmos. Sci., 62, 41–64, doi:10.1175/JAS-3360.1, 2005.

Heymsfield, A. J., Bansemer, A., Heymsfield, G., and Fierro, A. O.: Microphysics of Maritime Tropical Convective Updrafts at Temperatures from -20 degrees to -60 degrees C, J. Atmos. Sci., 66, 3530–3562, doi:10.1175/2009JAS3107.1, 2009.

Jensen, E. J., Lawson, P., Baker, B., Pilson, B., Mo, Q., Heymsfield, A. J., Bansemer, A., Bui, T. P., McGill, M., Hlavka, D., Heymsfield, G., Platnick, S., Arnold, G. T., and Tanelli, S.: On the importance of small ice crystals in tropical anvil cirrus Atmos. Chem. Phys., doi:10.5194/acp-9-5519-2009, 9, 5519-5537, 2009.

Koop, T., Luo, B., Tsias, A., and Peter, T.: Water activity as the determinant for homogeneous ice nucleation in aqueous solutions, Nature, 406, 611–614, doi:10.1038/35020537, 2000.

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., 9, 3505–3522, doi:10.5194/acp-9-3505-2009, 2009.

Spichtinger, P. and Krämer, M.: Tropical tropopause ice clouds: a dynamic approach to the mystery of low crystal numbers, Atmos. Chem. Phys., 13, 9801–9818, doi:10.5194/acp-13-9801-2013, 2013.

Vardiman, L.: The Generation of Secondary Ice Particles in Clouds by Crystal-Crystal Collision, J. Atmos. Sci., 35, 2168–2180, doi:10.1175/1520-0469(1978)035<2168:TGOSIP>2.0.CO;2, 1978.

Yano, J.-I. and Phillips, V. T. J.: Ice-Ice Collisions: An Ice Multiplication Process in Atmospheric Clouds, J. Atmos. Sci., 68, 322–333, doi:10.1175/2010JAS3607.1, 2011.

Mainz cloud gallery (as of 30/10/2014): http://www.cloudgallery.mpich.de/Cumulus/Cu_pileus/cu_pileus_new.htm