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.

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.

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 described 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.

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.

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.

Can you specify this ?

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."

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 to 5 and 6: I don't see that these points are aquequately 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.

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.

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

- 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.

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).

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.

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.

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: see new comment on 5.

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: 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?

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.
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 (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.
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. → where ??

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.

New comment: 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'.

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,

→ No, since I wrote about the temperature range < -38C .

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

→ 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.

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.

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.

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.

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 theenvironmental conditions.

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.

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.

Where can I find that or how is the new statement?

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.

Where can I find that or how is the new statement?

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.

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."

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 ...