We once again thank reviewers for their comprehensive review and comments on our manuscript. We have now addressed the remaining issues reviewers raised.

Reviewer #1

The authors have provided good responses to most of the reviewers' comments and have made appropriate changes to most of the manuscript. There remains some speculation that should be stated as such. Details are given in the comments below. Some of the responses to reviewers' comments require clarification. There are several instances where the English text could be improved upon. Many of the mistakes come from incorrect word use.

We appreciate the important comments from reviewer. We have addressed all of the comments raised in the revision, clarified related responses, and carefully reviewed the manuscript for editing and grammatical errors.

Comments on responses to the Reviewer 2's comments

Line 99 Is the spatial resolution of 1km in the horizontal? If yes, what is the vertical resolution at the typical aircraft location?
What does it mean that the vertical resolution of the radar profile along flight track is 30 m (Fig. 4)? Has interpolation been done? The beam width will be significant at a range of 50-200 km.

Yes, the radial spatial resolution of radar is 1 km, and the radar reflectivity in radar profile is calculated by linear interpolation in nearest neighbor combined with a vertical direction (NVI) method, and the vertical resolution of the radar profile after interpolation is 30 m. Related discussions are revised:

P6, Line 184: "A vertical line was first determined according to the latitude and longitude of the aircraft, then the azimuth angles, elevation angles, and range bins of equidistant points with a resolution of 30 m in the vertical line were obtained, and the radar reflectivity of each equidistant point was calculated by linear interpolation in nearest neighbor combined with a vertical direction (NVI) method. Hereby the radar profile with vertical resolution of 30 m along flight track was obtained."

Line 217 and the INP spectra in Fig S6 Is this calculated from the Equation on page 4 using the PCASP aerosol concentration measured, and then increased by a factor of 10 to account for uncertainty in the measurements of Demott? And does it therefore represent a likely upper limit on primary INP concentrations?
Make it clear in Figure S7 caption that the number concentration has been multiplied by a factor of 10 ..."to represent a likely upper limit on primary INP concentrations"?

We thank reviewer's comment, and the caption of Figure S7 (Figure S5 in the revision) is revised now:

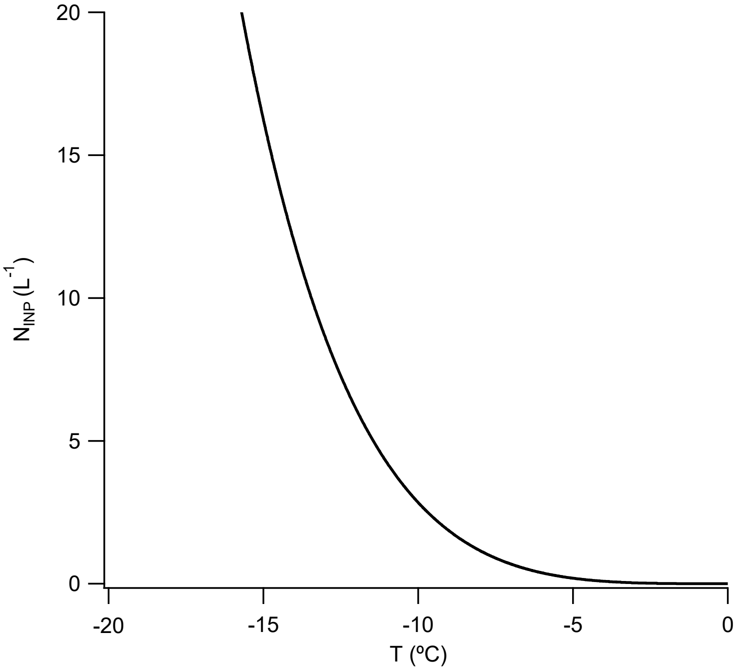

Figure S5: The number concentration of ice nucleating particles (N_{INP}) as a function of cloud temperature, and the N_{INP} has been multiplied by a factor of 10 to represent a likely upper limit on primary INP concentrations.

Line 233 but you do not know where this ice was generated and if it had been transported from other parts of the cloud e.g. that could have been in the H-M zone. Has there been any change to the text in response to this comment? It isn't clear what is meant by observing "a net production of ice."

The related discussions are added now:

P9, Line 259: "It should be noted that the observed N_{Ice} may involve hydrometeors transported from other parts of clouds, along with the locally produced ice. The ice production can therefore be considered as a continuous process and the observed N_{Ice} is a net production of ice after considering all the input (local production and transport in) and output (fall out and transport out) factors at the observation level."

Comments on the revised paper

Line 253 However, the dynamic vertical or horizontal transported of produced ice might induce some uncertainty when evaluating the concentration at the supposed same aircraft position.
This does not describe the problem. It would be useful to describe a possible scenario of ice being transported from e.g near cloud top by the circulations in convective thermals.

The discussion is revised according to reviewer's suggestion:

P8, Line 246: "However, the dynamic vertical or horizontal transported of ice, e.g. in convective thermals, the ice near cloud-top can be circulated downwards surrounding the convection core, while being transported upward in the convection core (Korolev et al., 2020). This might induce some uncertainty when evaluating the concentration at the aircraft observed position."

Line 260 Although the average temperature of P2.1 was as low as -11.7C the abundant large ice particles triggered the active SIP process at P2.1 with high NIce about 300 L-1, indicating that the SIP process was not restricted by temperature.
This is speculation. There is no direct evidence that the large ice particles triggered the active SIP process. The fact that there is a high concentration of ice particles may be due to vertical and/or horizontal transport previous to the time the pass was made.

It is now revised as:

P8, Line 255: "Although the average temperature of P2.1 was as low as -11.7 °C, the abundant large ice particles seemed to trigger the active SIP process at P2.1 with high N_{Ice} about 300 L^{-1}, indicating that the SIP process might not be restricted by temperature, though the possible transport of ice from other cloud regions is not able to be completely excluded."

Line 262 Similarly, the history of development leading to the cloud regions in P2.2 and P2.3 cannot be determined, can it? I suggest that sentence be modified or deleted.

It is now modified as:

P9, Line 258: "The period 2.2 that lacked enough large ice was likely still in the glaciation process, and P2.3 might be difficult to trigger a more active SIP process due to the smaller number of large ice and limited liquid water."

Line 265 Aircraft penetrated the cloud-top at P4.3, where several primary ice particles could be observed (Fig. 7c)
It can only be speculation that these are primary ice particles.

It has been revised:

P9, Line 268: "Aircraft penetrated the cloud-top at P4.3 and observed several ice particles (Fig. 7c), which were likely primary ice particles."

Line 269 The size spectrum and 2D-S images in Fig. 7c showed that large ice presented at P4.1, and ice grew through riming and Bergeron processes, while the ice at P4.2 was mainly smaller ice, which was still in the process of growth.
Again, it is speculation that the smaller ice was still in the process of growth. Also, the wording of the first part of the sentence should be corrected

The discussions are revised:

P9, Line 269: "The size spectrum and 2D-S images in Fig. 7c showed that large ice particles presented at P4.1, and the images suggested these were likely formed through riming and Bergeron processes, while the ice at P4.2 was mainly smaller ice, possibly still in the process of growth."

Line 272 The large ice falling from the upper layer played a very important role in ice production process, the primary ice crystals formed through the nucleation process and grew up in the upper layer or during the fall, then fell to the lower layer to trigger the ice production process.
This should be stated as the likely or possible process.

The description is revised:

P9, Line 272: "The large ice falling from the upper level likely played a very important role in the ice production process, where the primary ice crystals might form through the nucleation process and grow up in the upper level or during the fall, then fall to the lower level to trigger the ice production process."

Line 276 There was a positive correlation between NIce and NRound, where more large droplets generally corresponded to a higher NIce.
Considering Reviewer 2's comment: There are many examples of out-of-focus drops (circles with holes in the centre). How were these handled in the processing of 2DS data. And the authors' response: The out-of-focus round particles have been corrected following the method by Korolev (2007) during data processing. How are the out-of-focus non-circular images handled? Is it possible that many of the images with holes are not drops, but larger out-of-focus ice particles?

The out-of-focus images are because of light diffraction by sphere, which is less likely to occur for non-spherical particles, i.e., ice particles (Mcfarquhar et al., 2017; Vaillant De Guélis et al., 2019). We therefore consider this group of images are mainly from droplets. A number of previous studies also consider these mainly resulted from droplets (Crosier et al., 2011; Lawson et al., 2015; Woods et al., 2018).

Line 303 "...but may not be directly associated with *the current* updraft strength or turbulence."

It is revised:

P10, Line 302: "The DCT essentially implies the amount of ice hydrometeors may fall from above, but may not be directly associated with the current updraft strength or turbulence."

Line 308 This suggested the DCT tended to be a more important factor than temperature.
It is possible of course that the cloud top had ascended to a greater altitude, but then

collapsed by the time of the observation.
It seems wrong to say that DCT is more important than temperature in the HM temperature zone. Most likely they are of similar importance.

Thank reviewer to point this out, and the statement is revised:

P10, Line 308: "This suggested that the DCT played an important role in SIP process, and in the region with lower temperature than H-M temperature zone, the DCT tended to be a more important factor than temperature in determining the intensity of SIP."

Figure 10 Is it not likely that graupel particles will pass through the HM zone in Mature cells and so SIP will occur in the HM zone? It should be emphasised that the discussion on p10 that concerns Fig 10 is a possible scenario (or likely?). There is no direct evidence

The related discussions are amended according to the reviewer's comments:

P10, Line 320: "Then the likely schematic plot of ice production at different stages of clouds was given (Fig. 10)."

P11, Line 327: "However, it should be noted that the larger ice particles may also falling to H-M zone in mature cells and trigger the SIP process."

Line 391 The seeder-feeder process was found...
It is only a possible explanation.

It is revised:

P13, Line 394: "The possible seeder-feeder process was found to extend the SIP process beyond the slightly supercooled temperature region for the typically considered H-M process."

Reviewer #2

Main comments

1. The removal of much of the wording and discussion on the evolution of cloud properties from different measurement periods is a good improvement from the original submission. The authors now use the ratio of IWC/TWC to document differences in the microphysics in clouds with different degrees of glaciation. I think that some additional description in section 3.1 is needed though. i) The authors refer to figure 2 to illustrate how the different periods were identified for subsequent analysis. However, periods P2 and P3 look very similar and so using this metric alone does not seem to be sufficient. Consider adding some text that provides more information on how the periods were selected. ii) Line 161: "this study postulated that the continuous clouds within the cloud system had similar dynamic and thermodynamic properties". This seems to be the key sentence that the authors use to justify the discussion about the "development" of clouds within the larger system, based on the glaciation metric. Please add some additional justification for this assumption. Are there previous studies that follow this approach in similar cloud systems for example?

We thank the positive comments from reviewer on our current manuscript. We have now addressed the remaining comments reviewer raised.

A paragraph is now added after the discussions for P2 and P3:

P6, Line 162: "Although the glaciation extents between P2 and P3 were similar, P3 showed a narrower cloud band (Fig. 3) and a lower cloud-top (Fig. 4) for dissipating cells compared to the mature clouds in P2."

P6, Line 165: "Therefore, this study postulated that the continuous clouds within the cloud system had similar dynamic and thermodynamic properties. Previous studies also pointed out the exchangeability between temporospatial domains of cloud properties in the same cloud system, where properties and evolution of individual clouds were similar (Lensky and Rosenfeld, 2006; Yuan et al., 2010; Coopman et al., 2020)."

2. The break-down of figure 4 into multiple panels is very nice as it allows the reader to examine the data from the different periods more easily. I would suggest using the new figure (Fig S4) instead of Fig 4 in the main paper. You could always put the current Fig 4 in the supplement if needed.

We thank the reviewer's comment, and Fig. S4 has been used instead of Fig. 4 in the revised manuscript, and Fig. 4 is deleted now.

3. Consider removing the MODIS satellite analysis (paragraph beginning line 214, Fig S6), as the satellite data is just a single snapshot in time, and so it is not straightforward to link it to the different time-periods from the aircraft data. Also, it

is worth noting that the satellite cloud effective radius data is at cloud-top and so not necessarily comparable to microphysics measurements made lower down in the clouds. As per my original review though, I do think it would be more useful to see a satellite image (visible or IR) of the cloud field presented early on in the text, to give the reader a better sense of the cloud system studied.

Fig. S6 and related analysis have been removed in the revision considering this suggestion.

4. In the description of the microphysical measurements the authors use the terminology of cloud "cells" and cloud "layers" e.g. "developing cells" and "ice particles fell from the upper layer to the lower layer". Can you clarify how you differentiate a cloud "cell" from a cloud "layer" in the measurements? It cannot solely be on the measured updraft strength, as period P4 is characterized as "young cells" but there are no significant updrafts shown in Fig S4d. Is it based on the radar echoes? Or something else?

The "cells" is used to denote different clouds discussed in manuscript, while the "layer" is used to indicate different height levels within clouds, not cloud layers. The "upper layer" or "lower layer" described in analysis does not refer to a specific height, but rather broadly denotes higher altitude levels or lower altitude levels within the cloud. In the revised manuscript, the "layer" has been changed to "level" for clarification.

Additional comments

1. Line 70: What do you mean by a "typical mid-latitude cloud"? What features make it "typical"?

The "typical" is now removed for clarification.

2. Line 121: Please define what TWC is when you introduce it. I assume it is the sum of the liquid and ice water contents. But it wasn't clear which cloud probes were used to calculate the TWC (FCDP, 2DS, HVPS). And if using a combination of probes, how was this done e.g. considering probe overlap in particle sizing.

The definition of TWC is added:

P4, Line 123: "The total water content (TWC) was obtained by adding the IWC calculated from the 2D-S (diameter 10-1280 μm) and LWC measured by the FCDP (diameter 2-50 μm)."

3. Line 130: Please clarify if the PCASP measurements were made below cloud base? From the current text it isn't clear that this is the case. It is important to mention as PCASP measurements in cloud often exhibit artifacts from cloud particles shattering on the inlet.

These discussions are now added:

P5, Line 130: "In this study, the PCASP measurement was conducted below cloud base, and the in-cloud PCASP data was excluded for analysis due to cloud particle shattering on the inlet. Therefore, $n_{aer,0.5}$ measured by PCASP below cloud base was used for calculation."

4. Line 186: I think the last two sentences in this paragraph would read better if the order was switched i.e. the sentence beginning "The fraction of smaller…." was before the sentence beginning "The sensitivity was tested…."

Thank the reviewer' comments, and it is revised now:

P7, Line 192: "The fraction of smaller ice with d < 180 μm ($F_{\text{smaller ice}}$) was defined to imply the freshly formed smaller ice which had not experienced sufficient growth (Fig. 4b). The sensitivity was tested by altering the threshold from 160-200 μm, and the resultant difference of smaller ice fraction was within 10%."

5. Line 200: Please clarify what you mean by "turbulence". It is not obvious that measures like the vertical velocity variance are greater in P1 for example. Or do you just mean the peak updraft strength?

In the revised manuscript, the turbulence was replaced by updraft for clarification:

P7, Line 195: "P1 featured strong updraft with vertical wind speed up to 8.9 m/s, and the strong updraft region was dominated by ice particles and precipitation particles (Fig. 4c-e)."

P7, Line 204: "The updraft strength in P2 was weaker than P1 (Fig. 4c), but P2 was more glaciated than P1 with F_{Ice} spanning from 0.36 to 1 (Fig. 2)."

6. Line 206: Fig S4d shows that there were measured ice concentrations of 80-120 L-1 in P4. Yet the text states that "there was no appreciable IWC measured in this region".

Thank the reviewer to point this out, the calculated IWC in this colder temperature region is also significantly lower compared to other stages (Fig. 5), the related discussion is revised:

P7, Line 210: "This stage was rich of liquid water with LWC up to 0.27 g m^{-3} at a colder temperature (−11 ºC), while the IWC measured in the region was significantly lower compared to other stages (Figs. 4d, e and 5)."

7. Line 254: What is meant by "at the supposed same aircraft position"?

This is more clearly stated:

P8, Line 246: "However, the dynamic vertical or horizontal transported of ice, e.g. in convective thermals, the ice near cloud-top can be circulated downwards surrounding the convection core, while being transported upward in the convection core (Korolev et al., 2020). This might induce some uncertainty when evaluating the concentration at the aircraft observed position."

8. There are still many instances where the English text could be improved on.

We have carefully reviewed the manuscript for editing and grammar errors.

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