We thank reviewers for their important comments to improve our manuscript. We have now addressed all of the comments raised in the revision. The main revisions we made include:

1. We have avoided the discussions about evolution of clouds, and now focus on the discussion and comparison of cloud microphysics at different stages. The ice mass fraction (IWC/TWC) is now used to indicate the different development stages of clouds, by considering a more mature cloud has a more glaciated fraction.
2. We have carefully revised and responded to the related parts which reviewers consider to be speculative.
3. We have added the radar times and the flight time windows for the four stages, and the time windows of targeting analysis periods.
4. We have carefully examined the manuscript for editorial and grammatical errors, addressing them to enhance the overall readability of our work.

In addition, the relative development stages of clouds are now renamed as P1, P2, P3, and P4 according to the reviewer2's comment. The related descriptions and figures have also been amended in the revised manuscript.

Reviewer #2

This manuscript presents airborne cloud microphysical measurements measured in a mid-latitude extratropical cyclone over China. The authors use the data to explore mechanisms responsible for ice production in different regions of the cloud field and make efforts to link the observed differences to the temporal evolution of the microphysical properties. They show compelling evidence of active secondary ice processes (SIP) in the cloud studied and I particularly liked the section on the production rate of secondary ice. That said, I do have some significant concerns about the analysis that I feel the authors need to address before this manuscript can be considered for publication.

We thank reviewer for the important comments, we have carefully addressed your comments and have made the following revisions to our manuscript.

Major comments

The authors need to provide evidence that the observations from different regions of the cloud field are showing the temporal microphysical evolution of the cloud microphysics, rather than just presenting measurements that simply document the horizontal variability of cloud properties in the wider cloud field i.e. effectively measuring different clouds. This is key to how the discussion of the observations in the paper is structured, and I am not convinced that the data can be linked together in the way the authors propose. As a result, many of the discussion points made in the paper are speculative. I did wonder if using the ground-based radar measurements to track the temporal evolution of the clouds sampled by the aircraft (before and after the aircraft measurements) might at least enable the airborne data to be put into better context with the "local" cloud development.

We thank reviewer's comments. The clouds observed were in the same cyclonic system, where the clouds underwent continuous generation, development and dissipation. The microphysical properties under different development stages of clouds are therefore different. However, though in the same synoptic system, it is difficult to explicitly rule out the "evolvement" from the same cloud, because we were measuring different clouds. We therefore now avoid the discussions about evolution of clouds, but using the ice mass fraction (IWC/TWC) to indicate the different development stages of clouds (Fig. 2), by considering a more mature cloud has a more glaciated fraction, for the discussions of cloud microphysics at different stages. The related parts are also amended in the revised manuscript.
Newly added Figure 2:

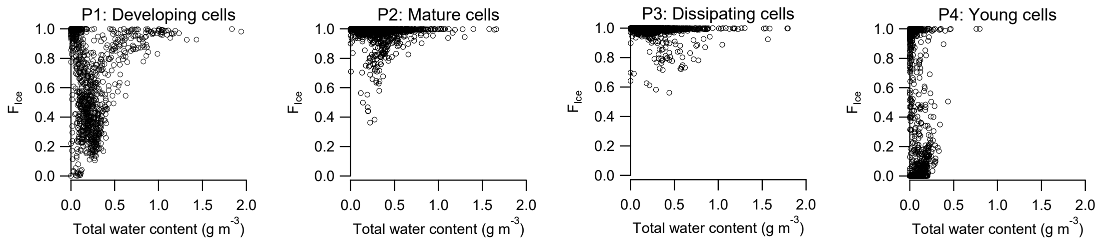


Figure 2: Ice mass fraction (F_{Ice}) as a fuction of total water content at four stages.

Related discussions are added:

P5, Line 156: "Four relative stages during the lifecycle of clouds were identified during experiment, which corresponded to the developing (P1), mature (P2), dissipating (P3) and young cells (P4) in cloud system, according to the different glaciation extents of clouds. The ice mass fraction (F_{Ice}: IWC/TWC) was used to indicate the different development stages of clouds (Fig. 2), by considering a more mature cloud has a more glaciated fraction, for the discussions of cloud microphysics at different stages. The cloud system was formed through the combined effects of dynamic forcing induced by the frontal uplift and the moisture transport provided by the prefrontal southerly air mass. Therefore, this study postulated that the continuous clouds within the cloud system had similar dynamic and thermodynamic properties."

Are these clouds best described as stratocumulus as stated in the title and various other parts of the manuscript? There certainly seems to be convection embedded in the cloud field e.g. updrafts of 10m/s in Fig 3. Would convection embedded in widespread (post-frontal or frontal?) stratiform cloud be a better description? It might be useful to see some satellite imagery of the cloud field.

We thank reviewer to point this out, and the cloud type is now changed to "stratiform clouds with embedded convection".
The title is changed as:

Microphysical view of development and ice production of mid-latitude stratiform clouds with embedded convection during an extratropical cyclone.
The related parts in manuscript are also revised.

Additional comments

Line 57: What is meant by "on top of the convective core"?

This is now revised as:

P2, Line 55: "Supercooled large drop may play important roles in the SIP process, which can fracture when freezing and emit ice splinters (Lawson et al., 2015), and this process could extend the SIP to a lower temperature under the influence of strong updraft."

Line 99: Is the spatial resolution of 1km in the horizontal? If yes, what is the vertical resolution at the typical aircraft location?

The 1km resolution is in the radial direction, and the vertical resolution of the radar profile along flight track is 30 m (Fig. 4). This is now clarified in the revision:

P4, Line 102: "which can detect targets within a radius of 230 km with a time and radial spatial resolution of 6 minutes and 1 km, respectively."

Line 106: How good is the circularity threshold of 1.2 on removing out of focus drops i.e. as those show in the imagery in Fig 6? Have the authors performed any visual examination of particles classed as irregular for example?

The out-of-focus round particles have been corrected following the method by Korolev (2007). The 2D-S images have been visually examined during the data analysis, which are also shown for typical cases (Fig. 7).

Line 116: Is a different M-D relation used to calculate IWC for the different habits?

Yes, the different approximated mass formulas for different habits are used to calculate IWC (Holroyd, 1987).

Line 119: Do the authors use the PCASP data for the calculation of INP? If so, where are these measurements located in relation to the cloud microphysics measurements?

Yes, the PCASP data measured at the cloud base is used to calculate INP. This is now clarified in the revision:

P5, Line 129: "In this study, $n_{aer,0.5}$ measured by PCASP at the cloud base was used for calculation."

Fig 1: The caption refers to a blue line, but there is an orange line on the figure.

The Figure 1 is corrected now, and the relative humidity is indicated by green line in Fig. 1b:

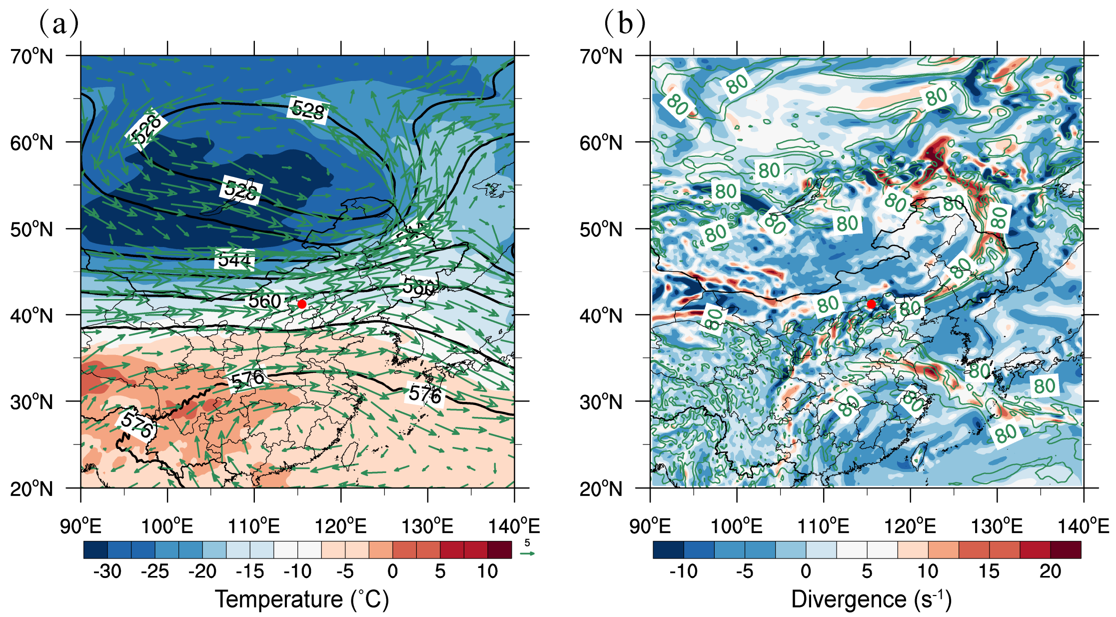


Figure 1: Synoptic overview during experiment. (a) The 500hPa temperature (color), height field (contour), wind field (arrow) at 08:00 (UTC+8h) on September 26th, 2017; (b) 850hPa divergence field (color), relative humidity (green line, only >80% is shown). The experimental region is indicated by the red dot on each plot.

The authors refer to both figures in the supplement as e.g. Fig. S1, S2,…etc and stages of the cloud development as S1, S2,….etc. I suggest that the authors differentiate these in any revision.

The relative development stages of clouds are renamed as P1, P2, P3, P4 now. The related description and figure are also amended in the revised manuscript.

Line 142-143: It is stated that "aircraft observation area was situated behind the cold front" and "aircraft sampled clouds formed….before the surface cold front". These seem to say the opposite thing. Clarification is needed.

It is corrected now:

P5, Line 151: "The aircraft sampled the clouds formed in this cyclonic system at this stage, i.e. behind the surface cold front line (Fig. S3) and for the newly formed, developing and matured clouds."

Line 149: Give more detail on how the different stages are defined.

The ice mass fraction (F_{Ice}: IWC/TWC) is now used to define different development stages of clouds (Fig. 2), by considering a more mature cloud has a more glaciated fraction. The related discussions are added:

P5, Line 158: "The ice mass fraction (F_{Ice}: IWC/TWC) was used to indicate the different development stages of clouds (Fig. 2), by considering a more mature cloud has a more glaciated fraction, for the discussions of cloud microphysics at different stages."

Fig 2: Can you indicate the times of the radar data on the figure? And what altitude is the reflectivity data from? Is it at the height of the aircraft data in each stage or is it at a fixed altitude?

The times of the radar plots (Fig. 3 in the revision) and the start/end time for each stage are added in Supplement (Table S1). The radar data used in Fig. 3 is the composite reflectivity data integrated over all altitudes.
Newly added Table S1 (Supplement):

Table S1. Radar times in Fig. 3 and the corresponding flight time windows for the four stages (all in UTC+8h).

Stages	Radar time	Flight time window	Targeting periods
Developing cells (P1)	10:06	10:03:35-10:23:55	1.1: 10:09:13-10:10:31 1.2: 10:11:54-10:12:41
Mature cells (P2)	10:42	10:25:35-11:05:17	2.1: 10:42:02-10:42:58 2.2: 10:45:47-10:46:25 2.3: 10:48:38-10:49:33
Dissipating cells (P3)	11:30	11:13:01-11:45:20	\
Young cells (P4)	12:00	11:46:16-12:15:52	4.1: 11:47:16-11:47:35 4.2: 11:51:02-11:51:05 4.3: 11:56:30-11:56:50

The related descriptions are added:

P6, Line 164: "The flight tracks mapping on the composite reflectivity of precipitation radar are shown in Fig. 3, colored by the LWC from FCDP and IWC from 2D-S respectively, the radar times and the flight time windows for the four stages are shown in Table S1."

Line 154: What does "evolved with almost opposite trend" mean?

It is revised:

P6, Line 166: "In developing cells, substantial LWC was detected up to 0.3 g m^{-3}, and the aircraft penetrated a high IWC region in this cloud at 10:09-10:11 BJT, with the highest IWC exceeded 2 g m^{-3} (Fig. 3a1, b1), and F_{Ice} at this stage could span from zero (pure water) to unit (pure ice) (Fig. 2)."

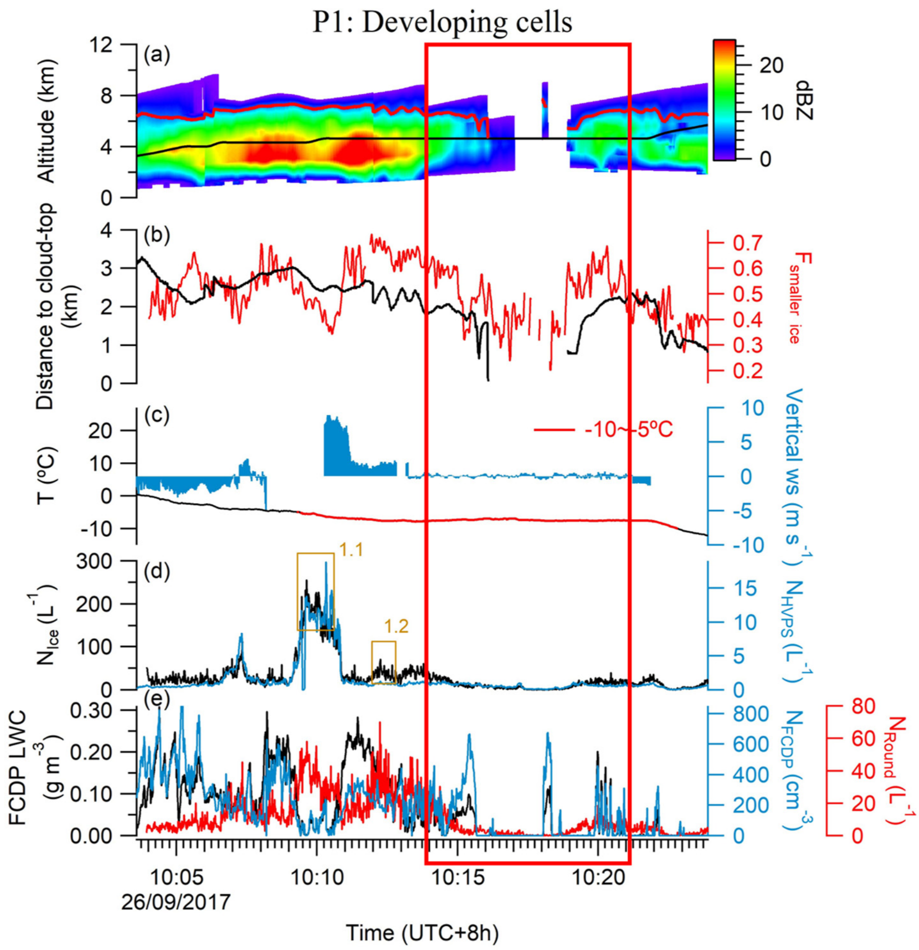
Line 160: What does "can tell the location of aircraft in cloud" mean?

This is now revised as:

P6, Line 176: "The cross section of radar reflectivity in Fig. 4a can provide information about the relative positions of aircraft with respect to the cloud top and base, as well as the echo intensity of the cloud."

Fig 3: What is the uncertainty in the vertical velocity (w) data shown in Fig 3. When looking at the time-series, there seems to be a general negative bias in w. Were any level runs out of cloud performed to see if there was an offset? Also, the uncertainty in these types of measurements is often large when aircraft are not flying straight and level, and Fig 2 shows that there were several large turns and profiles made during the flight. Has this data been quality-checked?

The AIMMS-20 probe equipped on the aircraft of this experiment is calibrated once every two years according to the procedure by the manufacturer. The vertical wind data during aircraft turns have been removed and are not used for data analysis. There was no offset for vertical wind speed when out of cloud, as the figure below shows.



Line 168: How sensitive is the fraction of smaller ice to the 180 micron threshold?

The sensitivity was tested by altering the threshold from 160-200 μm, and the resultant difference of smaller ice fraction was within 10%. This is added in the revision:

P6, Line 186: "The sensitivity was tested by altering the threshold from 160-200 μm, and the resultant difference of smaller ice fraction was within 10%."

Line 171: States that S2 is the most "vigorously developed clouds", yet the largest updrafts and downdrafts were in S1.

The stage is now classified as the ice fraction as P2 was more glaciated than P1, but the updraft is not the only criteria. The statement is also revised now:

P7, Line 197: "The cloud-top height in P2 reached 10 km (Fig. 4a), which was the highest cloud-top of clouds observed during experiment. The LWC in P2 was considerably lower compared to P1, while there were more large droplets and ice particles in clouds (Fig. 4d, e), and the distribution of large droplets and ice particles in P2 showed a bimodal distribution. The strength of turbulence 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)."

Line 175: The statement of consumption of liquid water in producing ice in the downdraft region is speculative. Could this just be ice precipitation from above?

The discussions regarding the evolution of clouds (such as consumption of liquid water here) are now removed. The high ice number (> 170 L^{-1}) corresponds with the updraft region, thus the ice was likely to be from the layer below. The related discussions in this paragraph are revised:

P6, Line 192: "The ice number peaked at a valley between two peaks of liquid water, but it was difficult to determine the vertical wind at the peak of ice number due to aircraft turns (Fig. S4). However, in the subsequent level flight, high ice number concentration (> 170 L^{-1}) was also observed in the strong updraft region. After the high ice number region, LWC up to 0.28 g m^{-3} was observed in the region with weaker updraft."

Line 177: Statements such as "The droplets at S1 grew to large droplets and were consumed by ice at S2 during the development of cloud" are speculative. Unless it can be demonstrated that the clouds measured at S1 were advected into the region of the measurements at S2 using e.g. trajectories, then these measurements cannot be considered to have been made in the same cloud.

Similar to above, the "consumed" is now removed. We have now removed all discussions regarding the evolution of clouds. The related discussions are revised:

P7, Line 198: "The LWC in P2 was considerably lower compared to P1, while there were more large droplets and ice particles in clouds (Fig. 4d, e), and the distribution of large droplets and ice particles in P2 showed a bimodal distribution. The strength of turbulence 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)."

Line 183: The measurements with the high drop concentration were also made at warmer temperatures ~ -3C and so it is perhaps not surprising that no ice was measured.

The descriptions are revised:

P7, Line 205: "This stage was rich of liquid water with LWC up to 0.27 g m^{-3} at a colder temperature (−11 ºC), while there was no appreciable IWC measured in the region (Fig. 4d, e)."

Paragraph at line 185: Speculation in statements linking different clouds to stages of development.

The inappropriate statements have been deleted, the authors now focus on the difference in glaciated fraction and characteristics of different clouds, and avoiding linking the microphysical evolution of different stages or different cloud regions. The related discussions are amended:

P7, Line 211: "The clouds in P2 were primarily composed of ice water, with the number concentration of cloud droplets significantly lower compared to P1. P3 was identified as dissipating cells, when the clouds were dominated by ice water, and had a higher F_{Ice} than P2 (Fig. 2)."

Line 192/Fig S5: The MODIS satellite imagery shows that there was large variability in cloud properties over the region sampled by the aircraft, which again highlights that it is not straightforward to link the observations in terms of stages of cloud development.

We have now avoided the discussions about evolution of clouds, but using the ice mass fraction (IWC/TWC) to indicate the different development stages of clouds (Fig. 2), by considering a more mature cloud has a more glaciated fraction, for the discussions of cloud microphysics at different stages.

Figure 6: The overlap between the FCDP and 2DS measurements is poor in the majority of example size distributions. Do the authors know why this is the case?

The particle size from the 2D-S measurement is determined by the image of casted shadow, previous studies showed this was biased larger for diameter smaller than 30 μm, especially in colder temperatures when some small irregular ice is present (Woods et al., 2018; Gurganus and Lawson, 2018). The FCDP is an optical counter based on

Mie-scattering thus has more accuracy for spheres. Previous studies also observed larger size determined by the 2DS than FCDP(Crosier et al., 2011; Lawson et al., 2006), and the agreement is largely improved for liquid clouds (the following figure is an example). This has been added in the revision:

P8, Line 238: "In addition, the larger size determined by the 2D-S than FCDP was found in Fig. 7, which was due to the lower accuracy for 2D-S to determine the particles in smaller bins (Gurganus and Lawson, 2018; Woods et al., 2018). This may be particularly the case when some small non-spherical ices were present at colder temperatures."

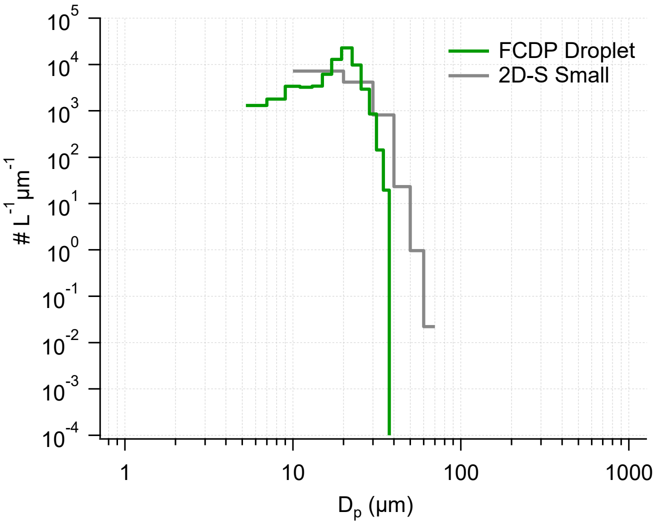


Figure: Particle size distribution of a liquid cloud

Figure 6: 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?

The out-of-focus round particles have been corrected following the method by Korolev (2007) during data processing.

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?

Yes, the authors multiply the calculated INP concentration by 10 to represent the upper limit of primary INP concentration. The factor of 10 was pointed out by (Demott et al., 2010) that "This new relationship reduces unexplained variability in ice nuclei concentrations at a given temperature from ~10^3 to less than a factor of 10, with the remaining variability apparently due to variations in aerosol chemical composition or other factors." And this factor has been used in previous studies to add additional constraining on the determination of secondary ice (Taylor et al., 2016; Sotiropoulou et al., 2020). By considering the potential 10-folds uncertainties, we still found the concentration of ice exceeded the calculated primary INP concentration, therefore this ensures the SIP identification.

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.

We agree with the reviewer's comment, while it is unlikely to separate the source of ice particles. The ice particles observed at the same level may involve the particles transported from other parts, and the particles generated by the ice production process. However, the ice production process also involves both the already-formed and ongoing particles, which is a continuous process, and what we have observed or modelled is a net production of ice.

Line 240: what upper layer?

This is clarified:

P9, Line 267: "The ice particles observed at this stage most likely originated from the ice nucleating process and the ice falling from above."

Line 260: Is DCT just a proxy for location with respect to convective cores? And if so, does it just illustrate the microphysical processes in the convection are different to the more widespread stratiform cloud? If so, I might expect a correlation between DCT and updraft strength or turbulence, but it is not obvious that is the case from Fig 3.

The DCT represents the location of observed region relative to the cloud-top, but may not be only limited to convective clouds. The clouds in this study have included both widespread stratiform and imbedded convective clouds, and the metric of DCT should all apply. The DCT essentially implies the amount of ice hydrometeors may fall from above, but may not be directly associated with updraft strength or turbulence. This is now added:

P10, Line 301: "It should be noted that the observed clouds have included both widespread stratiform and imbedded convective clouds, and the metric of DCT should all apply. The DCT essentially implies the amount of ice hydrometeors may fall from above, but may not be directly associated with updraft strength or turbulence."

Line 269: Again, speculation.

This is now revised:

P10, Line 299: "This suggested the development of cloud-top increased N_{Ice}, and considering the larger particles tended to fall to cloud base and form precipitation, the reduced N_{Ice} close to cloud base may be due to the coalesce of ice which reduced the number but enlarged the size of ice."

Line 275: But the aircraft is measuring different clouds and so there could be many reasons why the ice concentration is different from penetrations made at the same height.

We agree with reviewer that many factors could lead to different ice concentration but the DCT is a factor more apparently influencing the ice production based on the observation here.

Line 312: droplet > 25 microns?

This is clarified:

P12, Line 359: "Considering that the observation here was actually after the SIP process was initialized, when the smaller cloud droplets had been considerably consumed and most graupels were rimed, the number of large droplets (d > 50 μm) was the limited factor for SIP, and therefore used to calculate the modelled SIP rate."

Line 322: it is assumed that all ice is graupel, but in Fig 6 the habit classification shows that plates are the dominant habit.

Upon visual observation, most ice particles exhibit obvious riming characteristics, particularly larger ones, including plate ice, as shown in Fig. 6 (Fig. 7 in the revision). Therefore, ice particles larger than 250 μm are primarily considered as graupels.

Line 352: I think this is speculative.

This is revised with more evidence referenced.

P10, Line 314: "The explanation was also similar to the results reported by Li et al. (2021), which showed the columnar ice crystals were produced in the lower layer seeded by ice particles falling from the upper layer."

Line 364: The last sentence is rather generic. Can the authors provide some more information on how these measurements could be used to "improve the understanding of key processes" and "help find the region of supercooled water of clouds for the weather modification work".

This is now added:

P13, Line 403: "The results about the microphysical properties of stratiform clouds with convective cells under different stages suggest the falling hydrometeors associated with cloud-top height importantly controlled the cloud glaciation and precipitation process, and this may also help find the region of supercooled water of clouds for the weather modification work."

Finally, there are many instances where the English text could be improved on, and this is something that the reviewers should also try to address in any revision.

We have carefully examined the manuscript for editorial and grammatical errors, addressing them to enhance the overall readability of our work.

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