Response for Reviewer #1:

Comment to “Measurement Report: Aircraft observations of secondary ice production in convective clouds over the Tibetan Plateau”

This study makes a useful contribution to the observational literature on SIP, particularly in the under-sampled Tibetan Plateau region. The finding of an extended SIP temperature window and the emphasis on droplet size thresholds are potentially important for model parameterization. However, the heavy reliance on a single flight day for process-level conclusions, the uncertainty surrounding INP estimation in a pristine environment, and the tentative nature of mechanism attribution need to be more rigorously addressed.

General comments

(1) While the statistical analysis in Section 3.3 draws on seven flights, the detailed process-level investigation (cases 1–5, size thresholds, and mechanism attribution) relies almost entirely on a single flight on 6 July 2014. The authors acknowledge this limitation in Section 4.2, but it remains a significant caveat. The convective regime on that day may not be representative of all summer convective clouds over the TP.

Reply: We agree with your suggestion. We have clarified this point in Section 3.1 (Lines 145-147: Among them, flight 20140706 provided the most complete collocated microphysical sampling and was selected for the detailed process-oriented case analysis in Sect. 3.2. The pooled analysis in Sect. 3.3 uses observations from all seven flights as descriptive campaign context.). Moreover, we have revised the wording to be more rigorous when drawing conclusions based on the aircraft measurements from a single case.

(2) The SIP ratio is central to this study, yet the INP concentrations are estimated using the DeMott et al. (2010) parameterization, which was developed from mid-latitude and marine data. The TP is characterized by extremely clean aerosol conditions (N_{500} < 10 cm^{-3}) over complex topography, and recent studies suggest that global parameterizations can substantially overestimate INP in such pristine high-altitude environments (while I am not sure about the influence of topography). The authors briefly note this in Section 4.2, but the implications deserve more thorough treatment.

Reply: In Section 4.2, in our discussion of the uncertainty in INP concentration estimation, we have incorporated the results of INP measurement studies in the TP region, namely, INP concentrations of 0.002–0.675 L^{-1} between −7.1°C and −27.5°C, which are comparable to those in the Arctic and lower than typical mid-latitude continental values (Chen et al., 2021). In addition, we have added two references (Tan et al., 2021; Zhou et al., 2024) to discuss the influence of complex terrain on INP concentrations. The revised text is shown below (Lines 474-486):

A further source of uncertainty concerns the estimation of INP concentrations. The SIP ratio used in this study relies on the empirical parameterization of DeMott et al. (2010), which was developed from mid-latitude and oceanic data and may not fully represent the unique low-aerosol environment of the TP, especially at low temperatures. Recent precipitation-based measurements over the TP support this concern, reporting the INP concentrations of 0.002–0.675 L^{-1} between −7.1°C and

−27.5°C, comparable to those in the Arctic and lower than typical mid-latitude continental values (Chen et al., 2021). Moreover, given that the clean background aerosol concentrations over the TP are substantially lower than those used in the DeMott parameterization development, the estimated INP concentrations are likely overestimated in this study, implying that the reported SIP ratios should be considered as conservative lower bounds of the actual ice enhancement. Additionally, the complex topography of the TP can modulate aerosol vertical transport and spatial distribution (Tan et al., 2021; Zhou et al., 2024), adding further uncertainty to INP estimates. Despite these limitations, the observed ice enhancement and its environmental dependences are robust and provide useful constraints for improving microphysical parameterizations in high-altitude cloud models. These findings also highlight the urgent need for in situ INP measurements over the TP, particularly at temperatures below −10°C, to refine SIP efficiency assessments and to develop region-specific INP parameterizations.

(3) The attribution of HM, DS, and BR relies on indirect evidence (particle habits, temperature ranges, and coexistence of hydrometeor types). While this is a common challenge in observational SIP studies, the authors should be more cautious in their descriptions. For example, for Case 3 (−4°C, no graupel or columnar crystals), DS is inferred because large droplets (>50 μm) are present. However, without direct imaging of droplet fragmentation, this remains speculative. For temperatures below −8°C, the persistence of columnar crystals is interpreted as evidence of upward transport from the HM zone. An alternative explanation is that columnar crystals can grow at temperatures down to roughly −10°C (Bailey & Hallett, 2009), so their presence alone does not require vertical transport. Please clarify why upward transport is the preferred explanation.

Reply:

① Correction regarding the inference of DS in case 3

In case 3, the aircraft sampling region was predominantly composed of water clouds, with drizzle concentrations reaching up to 100 L^{-1}. Airborne images (Fig. S4) showed the presence of large droplets (>50 μm), but neither graupel nor columnar ice crystals were detected, and ice concentrations remained below 20 L^{-1}. Since no direct images of droplet shattering were captured, we can only conclude that the conditions in case 3 at about−4°C were favorable for the occurrence of DS. The analysis in the manuscript has been revised as follows (Lines 225-231):

In case 3, the aircraft did not penetrate convective regions and the cloud consisted mainly of water with drizzle concentrations reaching up to 100 L^{-1} (Fig. S3b). Although the cloud layer temperature was within HM range (approximately −4°C), neither typical in situ observational evidence of the HM nor fragile dendritic snow crystals were observed. However, droplets larger than 50 μm were present in case 3, yet no columnar crystals or graupel were detected from images (Fig. S4). Such large supercooled droplets provide favorable conditions for DS (Lauber et al., 2018; Korolev et al., 2020; Wildeman, 2017), which may explain the SIP ratio exceeding 1 in cases 3.1 and 3.4 (Fig. S3e). Nevertheless, ice concentrations (below 20 L^{-1}) did not increase as substantially as those observed in the convective regions of cases 1 and 2.

② Identifying the source of columnar crystals at low temperatures

We have corrected the values of vertical velocity from the ERA5 data in Figure 14. In the aircraft sampling region, the updraft was weak, only 0.03 m s^{-1}, and the vertical velocity directly measured by the aircraft was also weak, with even downdrafts occurring. Therefore, we have ruled out the possibility that columnar ice crystals were transported from lower levels. We thus suggest that the columnar ice crystals observed at low temperatures may have been generated through primary ice production or through the HM process beyond its classical temperature window. The detailed analysis is as follows (Lines 300-317):

Several processes may have contributed to the persistence of ice enhancement at temperatures colder than −8°C. We first examine whether the columnar crystals observed at low temperatures could have been transported upward from the warmer HM layer. ERA5 reanalysis data shows that the vertical velocity within the aircraft observation domain was only about 0.03 m s^{-1} during the sampling period (Figs. 14b–d), and the aircraft-measured vertical velocity between 11:20 and 11:35 was likewise weak, and even downdrafts were observed (Fig. 11f). Such weak vertical motion is insufficient to carry columnar crystals from the −5°C level up to the −16°C level. Two in situ explanations are therefore more plausible. First, columnar crystals can grow locally beyond the classical HM range through primary ice production. The ice crystal habit diagram of Bailey and Hallett (2009) shows that columnar habits extend down to approximately −10°C, so at least part of the columnar population observed between −8°C and −10°C may have formed in situ rather than being transported from warmer levels. Second, the effective temperature window of the HM process itself may be broader than the classical −3°C to −8°C range under TP conditions, where supercooled liquid water persists at low temperatures; recent observations likewise indicate that the HM process can remain active beyond its traditional window (Du et al., 2024). In addition, in situ DS and BR likely contributed at these low temperatures: large supercooled droplets (D > 50 μm) were still present below −8°C (Fig. 13). Moreover, Korolev et al. (2022) observed ice enhancement at temperatures down to −27°C and pointed to DS and BR as plausible explanations. These pathways can generate secondary ice independently of vertical transport and may act together, with their relative contributions depending on the local updraft strength, the availability of large droplets, and the concentration of preexisting ice particles. We must acknowledge that quantitatively distinguishing these contributions remains challenging with the current in-situ dataset.

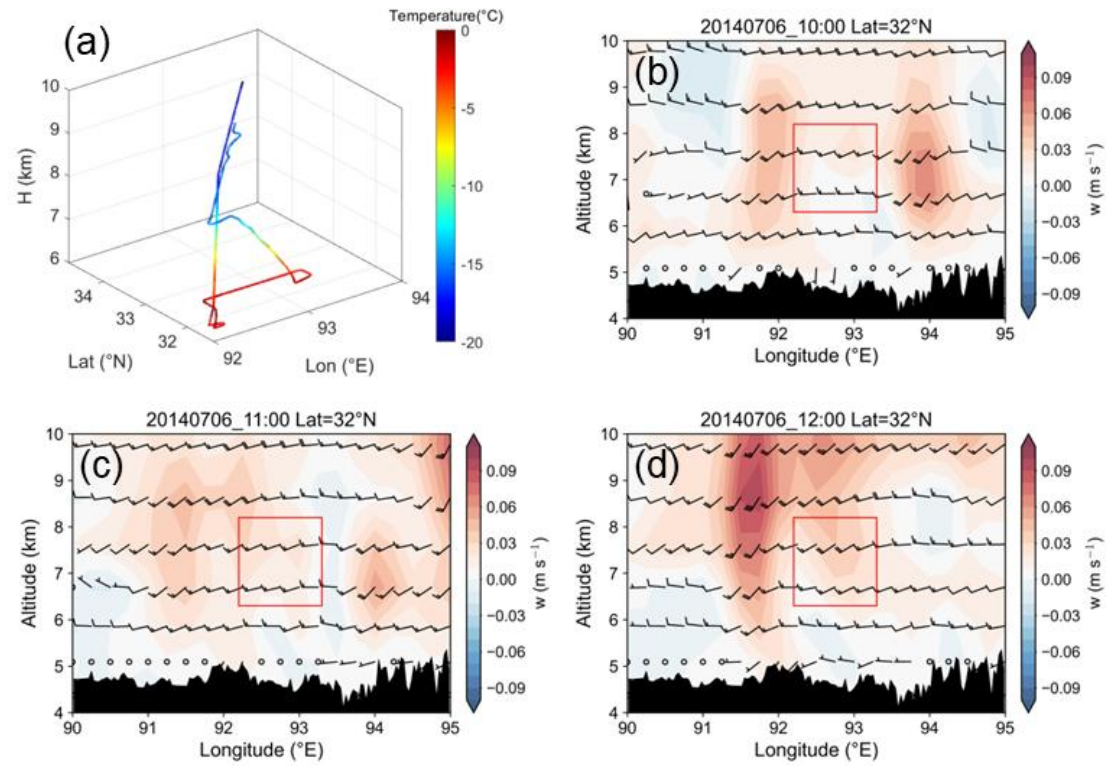

Figure 14: (a) Three-dimensional aircraft trajectory coloured by temperature; (b), (c), and (d) are vertical cross-sections of vertical velocity (shaded, m s^{-1}) along 32°N at different time. Vectors denote the zonal (u) and vertical (w) wind components, where the w wind component has been enlarged by a factor of 100. The red rectangle indicates the aircraft observation domain.

(4) The BR mechanism is invoked several times, but direct observational evidence (e.g., images of freshly broken crystal fragments with matching habits) is not explicitly shown. The authors should either provide more direct imaging evidence or temper their claims about BR's contribution relative to DS at low temperatures.

Reply: We have reselected the CPI images (Figure 9), choosing more images that support the BR and DS mechanisms. The analysis in the text has been further refined as follows (Lines 246-254):

In addition to the HM process, we also identified evidence of other possible SIP mechanisms in Fig. 9. The CPI images highlighted by green circles capture fragile dendritic snowflakes undergoing collision-induced fragmentation, with broken branches and detached fragments visible adjacent to the parent crystals, consistent with the BR mechanism. Collisional breakup of ice particles near –5°C has been documented to produce fine needles and columnar fragments (Hou et al., 2023; Knight, 2012). Korolev and Leisner (2020) also noted that BR is most likely in mixed-phase convective regions. Furthermore, the CPI images highlighted by red circles in Fig. 9 show frozen droplets captured at different stages of shattering, with small fragments surrounding partially frozen drops, providing in situ observational support for DS. Therefore, multiple SIP mechanisms may have collectively contributed to ice enhancement in the convective regions of cases 1 and 2.

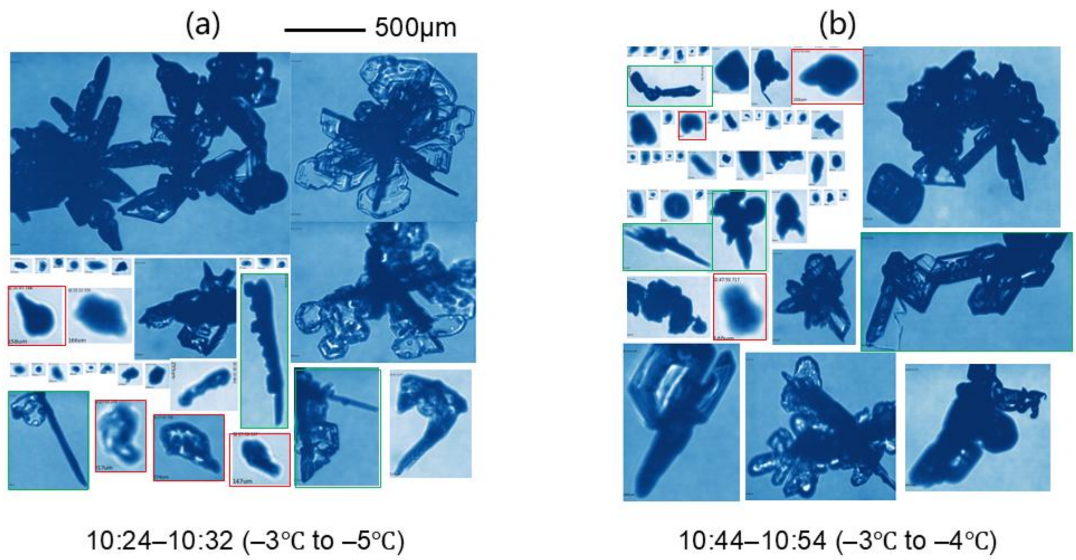

Figure 9: Example CPI images of panel (a) case 1 and panel (b) case 2.

Detailed comments

Line 33-36, two sentences could be merged into one.

Reply: We have revised as follows (Lines 34-36): The most widely studied process is the Hallett–Mossop (HM) process, also known as rime splintering, in which ice particles collide with supercooled droplets during riming at –3°C to –8°C, producing numerous small columnar ice crystals (Hallett and Mossop, 1974; Mossop, 1985).

Line 38-39, Recent studies could also be mentioned, such as Li et al. (2024, doi: 10.1016/j.atmosres.2024.107288).

Reply: We have added this reference as follows (Lines 39-40): In addition, the interaction between seeder-feeder and the HM processes from −3°C to −6°C has been observed in convective clouds during the MeiYu season (Li et al., 2024).

Section 2.1, Uncertainties associated with aircraft observations should be described or discussed, particularly over the TP.

Reply: We have revised Section 2.1, adding and discussing the uncertainties in aircraft observations. Lines 101-104: For imaging probes, the choice of maximum dimension definition can alter derived number concentrations by up to a factor of 6, and different data processing algorithms applied to the same raw dataset can yield concentrations differing by nearly two orders of magnitude (McFarquhar et al., 2017).

Lines 111-117: Different choices in data processing algorithms for airborne in situ observations can lead to uncertainties in derived cloud microphysical parameters (McFarquhar et al., 2017). Over the TP, the complex terrain and rapidly developing convective clouds may further magnify these statistical uncertainties. Nevertheless, previous studies in this region have largely depended on remote sensing and ground-based measurements, leaving in situ cloud microphysical observations notably sparse. Aircraft observations therefore remain the most direct and effective approach for depicting cloud microphysical characteristics in this high-altitude area. Despite these limitations, the consistency of our key findings across independent flight legs and missions lends support to the robustness of our principal conclusions.

Line 98, LWC and IWC should be defined first before they are used.

Reply: The full names of LWC and IWC were first introduced with their abbreviations in the Introduction (Section1, Lines 64-65): Using ground-based millimeter cloud radar, unimodal distributions of both ice and liquid cloud microphysical properties were identified over the TP, with distinct relationships between ice effective radius, ice water content (IWC), and temperature in high-level ice clouds, as well as increasing trends of droplet effective radius and liquid water content (LWC) with height in low-level liquid clouds (Zhao et al., 2016, 2017).

Therefore, the abbreviations are already defined prior to their use in Section 2.1. To avoid repetition, IWC and LWC are used directly in Section 2.1.

Line 109-113, Reliability of support of the phase classification method should be provided or discussed.

Reply: We have added three supporting references for the classification between ice clouds and water clouds (Ke et al., 2024; Korolev et al., 2003; Wu et al., 2022). The subdivision of water clouds is primarily based on droplet size. The revised text is as follows (Lines 124-130):

Cloud phase was classified from the IWC-to-LWC ratio and the particle categories. Samples with IWC/LWC > 10 and irregular particle concentrations from 2DS10 more than 10 L^{-1} were classified as ice cloud (Ke et al., 2024; Korolev et al., 2003; Wu et al., 2022). The remaining samples were classified as liquid water cloud. Based on droplet size, with 20 μm as the condensation–coalescence transition (Jonas, 1996) and 50 μm as the cloud–drizzle threshold (Sanchez et al., 2023), the liquid water clouds were subdivided into small water cloud (maximum droplet diameter smaller than 20 μm), large water cloud (round particle concentrations from 2DS more than 10 L^{-1}), and middle water cloud (all remaining liquid water samples).

Figure 3, I wonder why there are no droplet with diameters larger than 20 um for small water clouds, which is kind of weird to me for clouds over relatively clean environment over the TP.

Reply: As defined in Section 2.2, clouds with a maximum droplet diameter smaller than 20 μm are classified as small water clouds in our study. Accordingly, the droplet effective diameter shown in Fig. 3a is 9.3 μm. In field observations, it is physically reasonable for shallow cloud layers to exhibit droplet effective diameters below 20 μm, as the limited vertical development and short residence time restrict the activation and growth of collision-coalescence processes. This does not contradict the clean background of the TP, but rather reflects the substantial inhomogeneity of cloud microphysical properties across different stages of cloud development.

Line 193, “with in” should be “within”

Reply: We have revised as follows (Lines 210): In case 1.1, the cloud was predominantly composed of supercooled liquid water within updraft regions.

Figure 9, a legend regarding the size could be helpful.

Reply: We have added a legend regarding the size (500 μm) in Fig. 9 as follows:

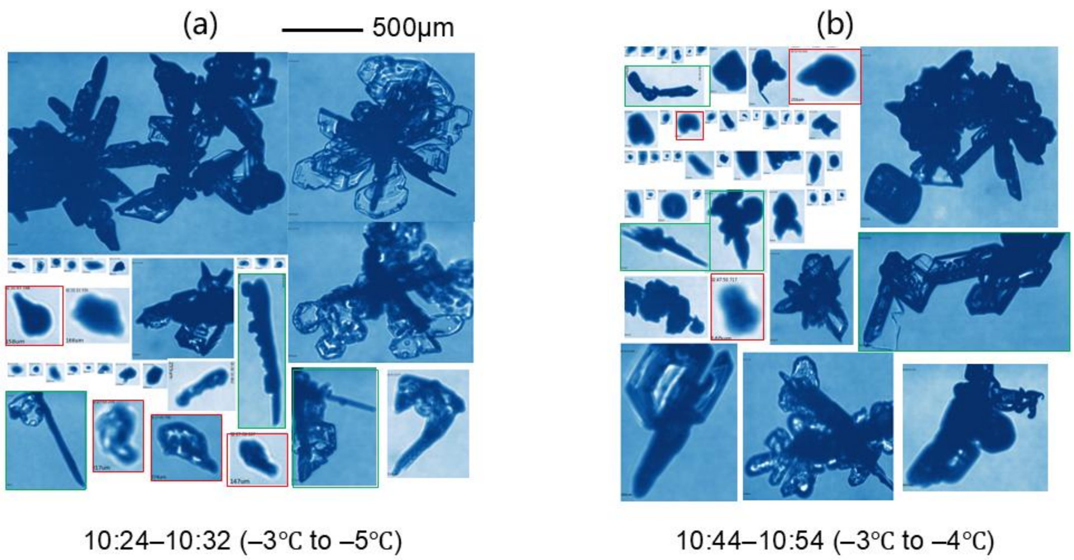

Figure 9: Example CPI images of panel (a) case 1 and panel (b) case 2.

Line 243-244, Why not both?

Reply: We agree and have revised as follows (Lines 257-258): However, a key question remains: how do the concentration and size of supercooled droplets that critically determines SIP occurrence?

Fig. 10: The dotted grey lines in panels (a), (c), and (e) need a legend entry. Are these extrapolated or smoothed fits?

Reply: We thank the reviewer for pointing out this ambiguity in our figure presentation.

We would like to clarify that all curves in Figures 10 and 15 are purely observational data, and no extrapolation, smoothing, or fitting has been applied. The "dotted grey lines" noticed by the reviewer are not a separate dataset, nor are they fitting results. Rather, they are an artifact of the step-plot representation used to display binned size distribution data: when a particular size bin contains zero measured concentration, the plotting routine naturally breaks the line segment, creating a visual gap. When such gaps occur across consecutive sparse bins, the line appears fragmented and visually resembles a "dotted" or "dashed" line.

To eliminate this ambiguity, we have revised the caption of Figures 10 and 15 as follows:

Figure 10: Mean size distributions (step plots) of ice particles (left) and droplets (right): (a, b) case 1, (c, d) case 2, and (e, f) case 3. Different colored lines represent selected legs within each case.

Figure 15: Mean size distributions (step plots) of ice particles (left) and droplets (right): (a, b) case 4 and (c, d) case 5. Different colored lines represent selected legs within each case.

Line 252-255, Is the finding applicable to other cases over other locations? More general conclusion is appreciated. If not, a short discussion could be helpful. Also, "Droplet size controls SIP initiation" — this is a strong causal statement. Since the study is observational, consider softening to "Droplet size appears to be the controlling factor for SIP initiation" or similar.

Reply: We agree with your suggestions. We have revised the description throughout the manuscript. For example (Lines 341-343): This contrast suggests that, in the analyzed cases, supercooled droplet size rather than concentration alone appears to be the primary controlling factor for SIP initiation at low temperatures.

Figure 15, Why are the droplet size with maximum values around 300 um?

Reply: We have added a red arrow with the annotation "~200 μm" in Figure 15 to indicate the peak size of large droplets. We acknowledge that the maximum droplet diameter observed in the spectra reaches approximately 300 μm. However, our study focuses on the spectral peak diameter rather than the absolute maximum size. As clearly shown in Figure 15, the legs that exhibit a pronounced spectral peak near 200 μm correspond well with the SIP-active legs, whereas such a spectral feature is absent in the SIP-inactive legs. This reinforces our conclusion that the presence of a droplet spectral mode near 200 μm, rather than the absolute maximum size, is the critical factor for sustaining SIP at low temperatures.

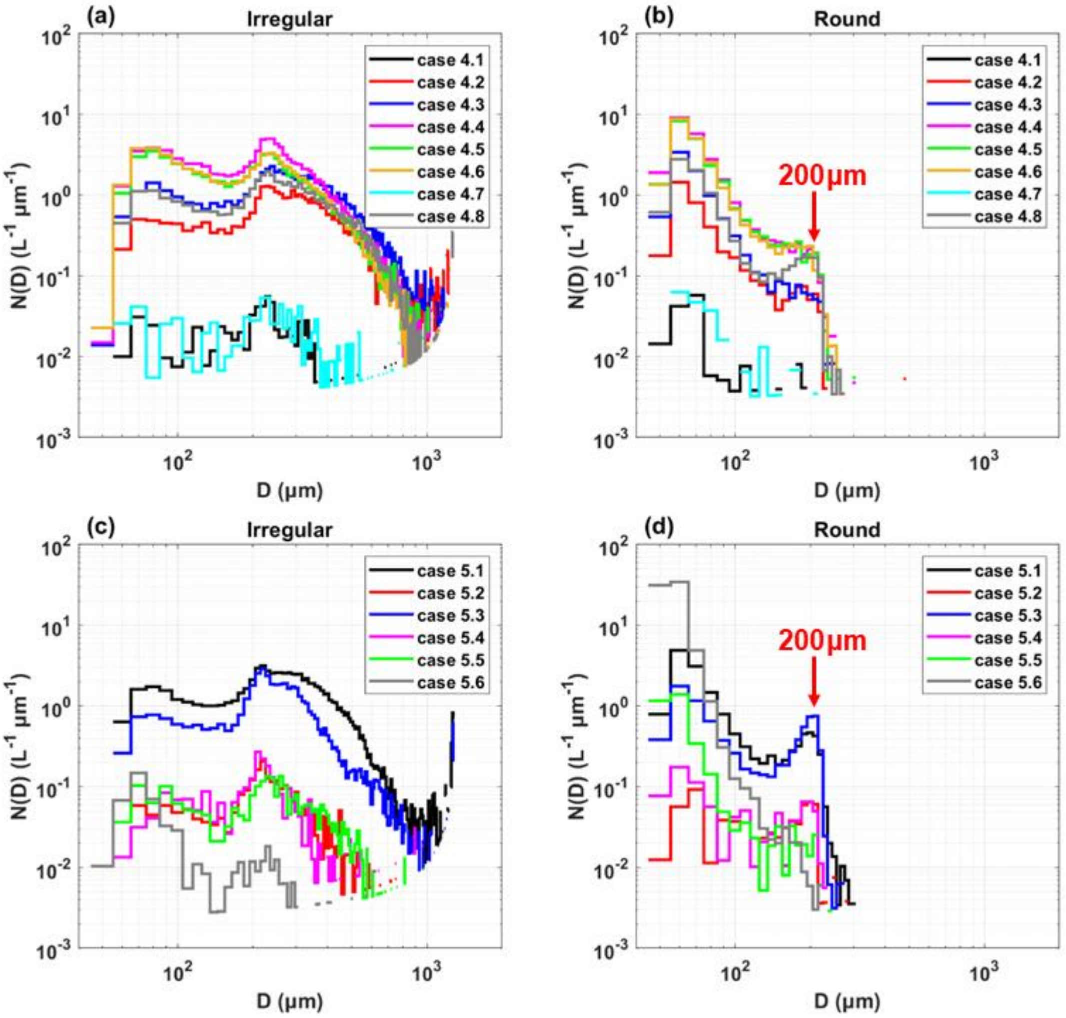

Figure 15: Mean size distributions (step plots) of ice particles (left) and droplets (right): (a, b) case 4 and (c, d) case 5. Different coloured lines represent selected legs within each case.

Line 424: "clean aerosol conditions minimize competition from primary nucleation" — this is an important point. Could you elaborate on why low aerosol loading favors SIP?

Reply: We have added supporting literature (Huang et al., 2008) indicating that, under clean background conditions, the formation of supercooled large droplets is favored. In addition, we have included results from SIP studies in the Arctic region (Sotiropoulou et al., 2020), which have a clean background similar to that of the TP. The revised text is as follows (Lines 447-451):

Firstly, clean aerosol conditions promote large supercooled droplet formation through reduced CCN competition (Huang et al., 2008), providing essential raw material for HM and DS. Sotiropoulou et al. (2020) noted that a single SIP mechanism is often insufficient in pristine Arctic environments, only the combined action of BR and HM can explain the observed high ice concentrations. This suggests that in clean environments (INP-scarce) such as the TP, the synergistic action of multiple SIP mechanisms may play an important role in sustaining high ice concentrations