We thank both reviewers for their detailed and insightful feedback which has contributed towards significant improvements to the manuscript. Responses to each comment are structured as follows: (a) reviewer comment (in bold), (b) our response to the comment, (c) subsequent changes to the manuscript (in quotation marks and blue italics). A list of all new references to be added to the manuscript is provided at the end. In the revised manuscript modified text is highlighted using Track Changes.

Referee 1

The manuscript reports in-situ measurements from the M-Phase field campaign over the Labrador Sea, aimed at understanding cloud formation in marine cold air outbreaks (CAOs). The authors observe that ice crystal counts within clouds often exceed the concentration of ice nucleating particles (INPs) measured outside the clouds, suggesting secondary ice production processes being active. Their conclusions are based on in-situ data, including ice crystal shapes from CPI and 2D-S probes, size measurements of ice crystals and cloud droplets, and cloud microphysical properties such as temperature profiles measured during the ascent and descent flight sections and updraft and downdraft velocities recorded during straight-leg flights. However, the absence of remote sensing data (such as ground-based doppler radar to measure vertical velocities) and comprehensive in-cloud microphysics measurements limits the ability to definitively identify secondary ice production mechanisms. This gap raises questions about the conclusions regarding SIP processes presented in the manuscript.

Many recent in-situ studies have shown a discrepancy between ice crystal number concentration (ICNC) and ice nucleating particle (INP) concentrations, particularly in shallow stratiform Arctic clouds (Li et al., 2021; Luke et al., 2021; Pasquier et al., 2022; Sotiropoulou et al., 2020). Naturally, the immediate interpretation of this discrepancy is that secondary ice production mechanisms are active. However, key questions remain unanswered: what specific mechanisms are active under certain conditions, and how can these mechanisms sustain steady ice crystal production over days despite low updrafts, minimal water vapor supersaturation, and the apparent lack of INPs active above -10°C? Answering these questions is at least as important as delivering yet another evidence of SIP in the shallow mixed-phase clouds.

On a less critical note, the manuscript is clearly written and provides a concise overview of the M Phase field campaign and its main results. I am confident that the campaign data would provide a solid basis for numerous case studies aiming at reproducing the observed ICNC and potential SIP mechanisms. It is unfortunate that the manuscript describing the observational datasets from the M-Phase campaign (Clarke et al., 2025) is not yet accessible. Access to it would clarify some questions that arise while reading this manuscript.

We thank the reviewer for their detailed and constructive feedback. Note, a citable preprint of the Clarke et al. (2026) paper, describing in full the observational datasets from the M-Phase campaign, is now accessible and has been added to the reference list.

Major Comments

1. **Page 2, line 75: "*Early laboratory experiments involving a rotating ice-covered metal rod, representing a large riming ice particle, in a chamber full of supercooled liquid droplets were all successful in generating secondary ice; the process was most efficient when temperatures between -3 and -8 °C coincided with high concentrations of droplets with diameter > 24 μm and < 12 μm, and a relative rimer-droplet impact velocity between 2 and 6 m s^{-1} (Hallet and Mossop, 1974; Mossop, 1978; Mossop, 1985b; Saunders and Hosseini, 2001)".* This statement neglects the fact that even within the repeated experiments of exactly the same type (rotating icing rod) there was a wide spread of measured splinter production rates and these experiments repeatedly failed to provide any insight into the physical mechanism behind the rime-splintering (Korolev and Leisner, 2020).**

We thank the reviewer for this comment. We agree that our brief summary of classic rime splintering experiments does not acknowledge the variability in measured splinter production rates or the lack of consistency in experimental conditions (e.g. ambient temperature, rimer surface temperature or droplet impact velocity) observed to be most conducive to peak splinter production; as a result, the exact physical mechanism responsible for rime splintering remains under continued debate. The manuscript has been updated on Page 2 from Line 76:

"...were all successful in generating secondary ice; however, the measured rates of splinter production and the conditions most conducive to peak splinter production varied between experiments. Hallett and Mossop (1974) found the process to be most efficient when temperatures between -3 and -8 °C coincided with high concentrations of droplets with diameter > 24 μm; Mossop (1978) later observed splinter production to increase further when droplets smaller than 12 μm were also present. Saunders and Hosseini (2001) conducted similar riming rod experiments, investigating the importance of rimer-droplet impact velocity, observing maximum splinter production at 6 ms^{-1}, compared to 2.5 ms^{-1} observed by Hallett and Mossop (1974); however, Saunders and Hosseini (2001) measured a factor of five times fewer splinters generated per milligram of rime accreted."

See our response to Comment (4) below for further updates to the text immediately following the above section.

The manuscript has also been updated on Page 3 from Line 106:

"Rime splintering, frequently referred to as the Hallett-Mossop (H-M) process given findings from the original Hallett and Mossop (1974) experiments provide the basis for most SIP model parametrisations (Korolev and Leisner, 2020), is often the only ice multiplication mechanism represented in numerical models. Parameterisations of the H-M process..."

2. **The compliance of the cloud conditions observed in the M-Phase flights with the above-mentioned criteria (see my comment 1) for efficient riming-splintering SIP is the authors' main argument for HM being the dominant SIP mechanism. However, the evidence for two of these conditions is actually not convincing: a) the droplets size distribution is not shown and the presence of droplets smaller than 12 μm in diameter**

is not documented; b) the largest updrafts measured during the straight-leg flights seldom exceed 2 m/s, making the arguments for HM questionable. These issues have to be discussed in more details.

We thank the reviewer for their comment. We believe that the relative frequency histograms presented in Fig. 17 clearly show that the highest concentrations of large cloud droplets (> 24 μm) were more commonly observed in SIP versus non-SIP cloud regions, thus meeting the criterion for active rime splintering; this relationship is even stronger when measurements made within the H-M temperature zone are isolated (Fig. C2). However, we agree that it is important to also document the presence of small droplets (< 12 μm), given we have previously stated this to be a requirement for efficient rime splintering. Therefore, we have added mean cloud droplet size distributions for SIP and background ice regions measured during the H-M process case study flights C328 and C329 to Sect. 3.2.3 on Page 23; this is discussed in more detail below in response to Comment (9).

As discussed on several occasions within the paper, weak-to-moderate updrafts (0 to 2 ms^{-1}) were found to be most favourable for SIP during M-Phase. This result is consistent with previous in-situ measurement studies of SIP in mixed-phase, cumulus cloud regions (Hallett et al., 1978; Heymsfield and Willis, 2014); rime splintering was determined to favour regions where updrafts were (a) strong enough to generate a sufficiently saturated environment for aerosol activation and riming, but also (b) weak enough to hold newly generated splinters within the H-M temperature zone of enhanced SIP activity for long enough to facilitate sufficient ice growth for renewed riming and splintering. The reviewer's comment implies that much stronger updrafts are required for efficient rime splintering, and it is true that some studies have observed greater SIP in more turbulent, mixed-phase cumulus cloud regions where updrafts have exceeded ~ 3 ms^{-1} (Karalis et al., 2022; Wang et al., 2023); however, in these cases, ice multiplication was ultimately attributed to other SIP mechanisms, such as freezing droplet fragmentation or ice-ice collisional breakup.

The manuscript has been updated on Page 32 from Line 1004 to include more discussion on this:

"...riming and renewed splintering to occur. Heymsfield and Willis (2014) similarly suggested that locations where weak updrafts balance rimer fall speeds are most favourable for the H-M process as the time scales for riming are prolonged, enabling a more continuous production of secondary ice splinters....It should also be noted that more turbulent conditions, with updrafts regularly exceeding 3 ms^{-1}, have been found previously to be particularly conducive to strong SIP, as reported by Wang et al. (2023) in shallow mixed-phase clouds over China. However, in that case, as ice multiplication was determined to have mostly occurred at temperatures below the H-M zone, where graupel concentrations were very low, SIP was not attributed to the H-M process; instead, frozen droplet fragmentation was considered to be dominating, given the stronger updrafts facilitated the growth of many precipitation-sized liquid droplets that were collocated with high concentrations of small ice fragments."

3. **The ICNC measured in stratiform and convective clouds (figure 3b) both show significant enhancement of ice compared to numbers of INPs, indicating SIP activity. This enhancement is undoubtedly stronger in the convective clouds, but also clearly visible for stratiform cloud cases. Given a slow vertical velocity measured in the**

stratiform clouds (between -1.5 m/s and +1 m/s, Figure 4f), can rime-splintering SIP mechanism be held responsible for the observed enhancement of ICNC?

We thank the reviewer for this comment. Firstly, it is important to reiterate one of the primary aims of this study: to identify clear regions of active or recently active SIP defined where measured ice crystal number concentrations were several orders of magnitude above INP concentrations observed at the same temperature, and where the corresponding ice crystal imagery showed clear evidence of ice multiplication (e.g. rime splintering, droplet shattering, or ice-ice collisions). Following this, we looked in more detail at the associated microphysical and thermodynamics measurements, as well as the ice crystal imagery to learn about the possible active SIP mechanism(s).

Although the reviewer is correct in stating that we observed ice concentration enhancements in many stratiform cloud regions (Fig. 3b), their magnitude peaked at ~ 5 L^{-1} but was most frequently below 1 L^{-1}, which is 1-2 orders of magnitude below the ice enhancements observed in the convective cloud regime. Furthermore, most of the ice enhancements in stratiform cloud did not coincide with isolated pockets of sudden elevated ice concentrations, as was almost always the case for the strong SIP observed in convective cloud during M-Phase and has been regularly observed by other in situ aircraft campaigns in mixed-phase cloud, e.g. Korolev et al. (2022). This suggests that the ice enhancements measured in stratiform cloud were likely not a result of ice multiplication initiating when certain environmental thresholds were met, as in the convective regions, but more likely were due to primary ice sedimenting down to the measurement point from nearer cloud top, or possibly due to ice crystal or INP instrument/processing uncertainties. However, it should be noted that it is also likely that some weaker regions of SIP within stratiform cloud were not identified in this study and that the overall frequency of SIP activity has likely been underestimated; see our response to Referee 2 Comment (3) for more discussion on this.

4. **Page 3, lines 80 – 85*: "A more recent H-M experiment conducted by Seidel et al. (2024), using high speed video microscopy and infrared thermography, found little evidence of efficient ice multiplication by rime splintering; however, this was a controversial result given differences in the experimental setup compared to the earlier studies, such as the representation of graupel particles (single, fixed particle in the Seidel et al. (2024) experiment) and the droplet size distributions used, with almost no droplets of diameter < 12 µm used by Seidel et al. (2024)".* Being different from earlier studies does not automatically mean delivering controversial results. Earlier laboratory studies NOT reproducing the Hallett-Mossop type of experiments (Dong and Hallett, 1989; Emersic and Connolly, 2017) found no evidence for SIP upon freezing of cloud droplets on a rimer surface. The study of Seidel et al. (2024) provided evidence that the underlying mechanism of rime splintering is not spherically-symmetrical freezing of cloud droplets upon contact with riming ice crystal, whatever the droplet size distribution. With respect to the lack of rotation or lateral movement, no vigorous tumbling or rimer rotation or even turbulent-induced acceleration of a graupel particle in realistic cloud environment could account for lateral (centripetal) acceleration of 6g achieved in the Hallett-Mossop type of riming experiments (a rod sweeping the 30 cm diameter circular path with linear velocity of about 3 m/s).**

We thank the reviewer for their comment. We have updated the manuscript on Page 3 from Line 84 to include reference to the additional, non-Hallett-Mossop type rime splintering experimental studies highlighted by the reviewer:

"A more recent, non-riming rod, experiment conducted by Seidel et al. (2024), using high-speed video microscopy and infrared thermography, found little evidence of efficient ice multiplication by rime splintering; however, this result may be considered controversial given fundamental experimental setup differences, most notably related to the droplet size distributions used, with almost no droplets of diameter < 12 μm utilised by Seidel et al. (2024). Ultimately, the lack of consistency in experimental setups and results means the exact physical process underpinning rime splintering remains unclear, although several mechanisms have been proposed. For instance, in earlier riming experiments by Dong and Hallett (1989), accreted droplets were found to spread out on the rimer surface at temperatures above -8 °C, rather than freezing spherically on contact; splintering was concluded most likely to be caused by thermal stresses related to the droplet-to-rimer temperature gradient. Emersic and Connolly (2017) also observed droplet spreading; however, protruding rime spires were observed to grow as rime depth increased, encouraging glancing droplet collisions. This is consistent with the popular theory of Choularton et al. (1980), which proposed that large drops may freeze symmetrically when falling on a narrow ice neck created by previously accreted smaller droplets, due to poor thermal contact with the rimer; the freezing droplet subsequently shattering as its ice shell thickens."

5. **Page 3, lines from 103: "The ejected water freezes on the drop surface and seals the crack, progressively thickening the shell until a critical pressure is reached causing the drop to shatter, producing many small ice fragments (Korolev and Leisner, 2020). Previous studies suggest the frozen droplet fragmentation mechanism requires large, precipitation-sized drops and therefore favours strong updraft regions found in convective cloud systems".**

We thank the reviewer for their two-part comment below and have updated our description of the frozen droplet fragmentation mechanism accordingly.

A. Recent finding of (Kleinheins et al., 2021) suggests that the cracking and associated SIP events can happen multiple times during the freezing of a single droplet (up to 15 times for a droplet of 300 μm in diameter). The growth of ice shell does not occur via freezing of water ejected from the crack, but via progressive inward growth of ice shell and is controlled by the balance between release of latent heat of crystallization and diffusion-evaporation-convective heat removal into surrounding air.

The manuscript has been updated on Page 3 from line 116 to more accurately describe how freezing drops shatter, with reference to the Kleinheins et al. (2021) study:

"Frozen droplet fragmentation occurs initially via progressive inward growth of the freezing outer shell around a supercooled liquid droplet, which causes a build-up of internal pressure and eventual cracking of the ice shell, releasing pressure and allowing water to escape. The ejected water may freeze on the droplet surface and seal the crack, allowing the pressure to build again as the shell thickens; this process repeats until a critical pressure is reached, causing the droplet to shatter, producing many small ice fragments (Korolev and Leisner, 2020). Kleinheins et al. (2021)

found that each pressure release event may produce additional ice particles, meaning several SIP events are possible during the freezing of a single droplet…"

B. Droplet fragmentation has been observed across a wide range of temperatures (between -5°C and -30°C) and droplet sizes, from less than 100 µm (Pander, 2015), to raindrops of up to 2 mm diameter (Pfeifer et al., 2025), so that precipitation-sized drop are not a requirement. Maybe droplet fragmentation could be considered an alternative SIP mechanism for shallow stratocumulus clouds.

We have stated on several occasions within the results section of the manuscript that frozen droplet fragmentation may have occasionally been acting concurrently with the H-M process to amplify SIP under certain conditions within the shallow CAO clouds sampled during M-Phase; CPI images have been provided to support this, such as in Fig. 7a. We have updated the manuscript on Page 4 Line 124 to include the Pander (2015) reference to shattering observed at smaller droplet sizes:

…."; although, depending on the INP properties, Pander (2015) found that much smaller, drizzle-sized droplets (< 80 µm) can also readily freeze and shatter, suggesting frozen droplet fragmentation is possible in shallow, marine CAO clouds."

6. **Page 3, lines 114 and on: "…mechanical breakup of ice particles was shown to favour hydrometeors of varying sizes with different fall velocities, to maximise collisional momentum, and ice crystals with fragile surface characteristics such as riming protuberances or dendritic branches grown by vapour deposition (Sullivan et al., 2017)." The study of Sullivan et al. is a modelling study and cannot demonstrate the benefit of certain hydrometeors properties or cloud conditions on a particular SIP mechanism; it can only investigate the cloud response based on the numeric representation of the SIP process.**

We thank the reviewer for this comment, the modelling study by Sullivan et al. (2017) has been incorrectly referenced. The manuscript has been updated on Page 4 from Line 133 to also include reference to the more recent ice-ice collisional experiments conducted by Grzegorczyk et al. (2023), and the reference at the end of the sentence (previously Sullivan et al., 2017) has been removed altogether as the experimental studies have already been referenced in the first part of the sentence, before the semicolon:

"…is derived from early laboratory studies conducted by Vardiman (1978) and Takahashi et al. (1995), as well as more recent experiments by Grzegorczyk et al. (2023); mechanical breakup of ice particles was shown to favour hydrometeors of varying sizes with different fall velocities, to maximise collisional momentum, and ice crystals with fragile surface characteristics such as riming protuberances or dendritic branches grown by vapour deposition."

7. Page 6 line 202: "A threshold number of 10 µm pixels is set in the OASIS software,…" Please clarify this statement. What is the pixel size of the instrument?

The effective pixel resolution of the instrument is 10 µm, as stated on Page 6 Line 216. To define the minimum particle size accepted for further analysis, a minimum threshold number of pixels (20) was set in the processing software (OASIS), i.e. detected particles comprising fewer than 20 pixels

(D_p as small as 50 μm for a 4 x 5 shape) were rejected, given substantial uncertainties reported by Baumgardner et al. (2017) in determining the size and concentration of particles smaller than 50 μm. We feel this is clearly explained in Sect. 2.1.1, therefore the manuscript remains unchanged.

8. **Page 8 line 289: "INP concentrations at higher temperatures (> -9 °C) have previously been assumed to continue to decrease log-linearly in similar mixed-phase CAO clouds (Järvinen et al., 2022); therefore, if we also adopt this assumption here, expected INP concentrations at the warmest In-cloud temperatures observed during M-Phase (~ 0 °C) would fall between 10^{-2} and 10^{-3} L^{-1}". All off-line filter based INP measurements exhibit a steep drop of INP numbers between -10°C and -5°C, see e.g. Petters and Wright (2015). Log-linear extrapolation of the INP concentration trend into warm temperature would produce non-negligible number of INPs at 0°C. This assumption is not correct. On the other hand, this doesn't matter much because there are no reliable INP data points below -10°C.**

We thank the reviewer for their comment. The log-linear INP extrapolation assumption was based off a single set of ship-based INP measurements made in the Southern Ocean (McCluskey et al., 2018), so is not necessarily applicable to M-Phase and the different INP sources characterising the Labrador Sea region. We have therefore updated the manuscript on Page 9 from Line 311 to reference the Petters and Wright (2015) study, which presents composite spectra from several offline filter-based INP measurement campaigns, showing a steep INP concentration decrease (~ two orders of magnitude) at temperatures above -10 °C:

" ...Other offline filter-based INP measurements from a wide range of geographical locations were shown by Petters and Wright (2015) to decrease sharply (~ two orders of magnitude) at higher temperatures (between -10 and -5 °C); therefore, if we assume a similar trend here, expected INP concentrations at the warmest in-cloud temperatures observed during M-Phase had likely maximum values of ~ 10^{-4} L^{-1}..."

9. **Page 9, lines 315 to the end of the section. The CDP instrument is responsible for measuring droplet size distributions and thus providing the important evidence for the fulfillment of the HM requirements. Does CDP measure only droplets or also ice crystals? Is there any rejection algorithm allowing to filter out ice crystals? Showing a few size distributions for droplets would be very helpful.**

The CDP instrument is designed to detect cloud droplets only. The probe's collecting optics are configured to capture forward-scattered light, over a solid angle of 4-12°, from small (2-50 μm) spherical particles (i.e. droplets); droplet size is determined through Mie theory (Faber et al., 2018). Theoretically, large ice particles can shatter on contact with the instrument, introducing small ice fragments which may be erroneously detected as cloud droplets; however, as the CDP uses an open-path laser that passes between two arms outfitted with anti-shattering tips, potential uncertainty related to shattered ice particles is negligible, as shown by Lance et al. (2010). A larger potential source of uncertainty in the CDP measurement is coincidence, as discussed on Page 9 Line 340; however, coincidence error is a concentration-dependent issue that is generally considered minor for droplet concentrations below ~ 500 cm^{-3}. Overall, uncertainties with the CDP related to droplet sizing and concentration are estimated to be ~ 30 and ~20 %, respectively (Baumgardner et al., 2017).

As stated in response to Comment (2), we have added mean cloud droplet size distributions (Sect. 3.2.3 Page 23) for SIP and background ice periods during the H-M process case study flights C328 and C329. We show data for cloud droplets measured with the CDP probe up to a diameter of 50 μm and have also included data for Low Irregular particles (i.e. round) measured with the 2D-S probe as a proxy for larger cloud droplets (> 50 μm). On Page 23 from Line 746, we have added the following text to discuss the newly added Fig. 12:

"The importance of large, supercooled liquid droplets in initiating SIP, and the H-M process in particular, is further emphasised by the cloud droplet size distributions shown in Fig. 12. Concentrations of droplets with diameter greater than ~ 30 μm were substantially higher when averaged across SIP periods compared to neighbouring background ice regions during both flights C328 and C329 (Fig. 12). Interestingly, although there was clearly a strong presence of small liquid droplets (D_P < 12 μm) across SIP regions, consistent with the early experiments of Mossop (1978), their concentrations were generally higher across background ice regions. This suggests that certain mechanistic hypotheses for the H-M process may place too much importance on the role played by small droplets, such as that proposed by Choularton et al. (1980) regarding the symmetrical freezing and shattering of large droplets that impact regions of graupel particles where rime spires comprising small droplets have grown."

10. **Page 10, Figure 2. The GOES provides images every 10 min; only one GOES snapshot is provided per flight. How variable was the cloud field during a triangle flight? Was the transition line moving and if yes, how slow / fast? Why is the GOES imagery shown for the flight C332, which is not discussed in detail in the manuscript, but not shown for the flight C326, which is the ice-ice collision break-up flight?**

We thank the reviewer for their comment. Yes, during the cloud sampling leg of each flight (north-west to south-east section of flight over the Labrador Sea following the CAO flow), lasting ~ 1-1.5 hours, the regime transition line generally migrated ~ 100 km to the south-east. As there were only two flights that captured the regime transition, this wasn't something that generally needed to be taken into consideration. However, for flight C328 for example, as the aircraft was covering the ground ~5 times faster than the cloud field was migrating, the regime transition highlighted in Fig. 2a was still successfully captured before the aircraft had reached the sampling leg end point to the south-east at around 4.30pm; this is further evidenced by the sudden reduction in LWC and cloud droplet concentrations towards the end of the sampling leg, which had previously varied consistently with each sawtooth profile in the stratiform regime, shown in Fig. 6. This has been clarified on Page 10 from Line 369 of the manuscript:

"...Flight C328 also captured the regime transition to convective cloud, marked by the yellow lines in Fig. 2; at an airspeed of 100 ms^{-1}, the aircraft covered the ground around five times faster than the cloud field and regime transition was migrating to the south-east, thus Flight C328 successfully captured the stratiform-to-convective cloud transition before reaching the end of the cloud sampling leg at approximately 16:35..."

The GOES imagery for flight C332 is shown in Fig. 2c as we wanted to provide the clearest example of a flight which sampled the stratiform regime only; Figs. 2a and 2b showed a transition flight and convective regime flight, respectively. Flight C332 was also a special case where CAO cloud was associated with cold air drawn southwards behind the cold front of a mid-latitude cyclone, with a low-pressure centre located to the south-east of Greenland. The manuscript remains unchanged.

11. **Section 2.6, Splinter Production Rate (SPR) Calculation and Discussion (Section 3.2.4). These sections give rise to several questions:**

We thank the reviewer for the below series of questions as it provides an opportunity to discuss some of the fundamental differences between the SPR calculated in our study and the commonly applied Harris-Hobbs and Cooper (1987) SPR formula.

a. **If droplets smaller than 12 μm are required for rime-splintering SIP, why are they not accounted for in the calculations of the SPR, as it was done by Harris-Hobbs and Cooper (1987)?**

Firstly, it is important to reiterate that the physics underpinning all SIP processes, and in particular splintering associated with the riming of supercooled liquid droplets on the surface of large ice crystals, remain poorly understood; specifically, the relative importance of small and large drops in the rime splintering process is still an area of uncertainty and continued debate in the cloud microphysics community.

The formula developed by Harris-Hobbs and Cooper (1987) describing the rate of secondary ice production due to the rime splintering process is based on the hypothesis that when large droplets (> 24 μm) strike a region of a large ice particle (graupel) surface where small droplets (< 12 μm) have already accreted, the large droplets are in poor thermal contact with the graupel and therefore tend to freeze symmetrically from the outside-in (Griggs and Choularton, 1983). Subsequent shattering of the ice shell is then proposed to occur in a similar way to that described in the manuscript during the frozen droplet fragmentation SIP process. This mechanistic hypothesis is consistent with observations from Mossop (1978) suggesting the rate of splinter production was strongly dependent on the presence of both small and large droplets.

During M-Phase, the in-situ observational evidence suggested SIP, and particularly the H-M process, has a much greater dependence on concentrations of large than small droplets. In fact, as can be seen from the cloud droplet size distributions produced in response to Comment (9) above, concentrations of small liquid droplets were higher within background ice regions compared to SIP regions; therefore, calculating an SPR which scaled with increasing small droplet concentrations would not have been consistent with the observations. The SPR formula adopted for this study was developed by Hallett et al. (1978) to account for the observed rapid glaciation of cumulus clouds with cloud top temperatures generally above -12 °C, similar to those sampled during M-Phase. The Hallett et al. (1978) SPR calculation assumes a much greater role played by large droplets in rime splintering; it accounts for the initial contact nucleation of some large droplets on collision with newly formed ice splinters, enabling rimer-sized ice crystals, needed for further ice multiplication, to form much quicker than if ice growth was primarily by vapour deposition. Additionally, the cumulus clouds observed by Hallett et al. (1978) had peak ice crystal concentrations within weak-to-moderate updrafts, like during M-Phase, where the positive feedback process of splinter production, growth, riming, and further splintering had more time to occur within the H-M temperature zone.

In Sect. 3.2.4, on Page 23 from Line 766, we have updated the manuscript to summarise the above justification for not including small droplets (< 12 μm) in the SPR calculation:

"...concentration of liquid droplets with D_P > 24 μm; note, unlike the other commonly implemented splinter production rate formula derived by Harris-Hobbs and Cooper (1987), small liquid droplets (D_P < 12 μm) are not considered here. The Harris-Hobbs and Cooper (1987) calculation, used by Crosier et al. (2011) and Taylor et al. (2016), is based on the previously discussed theory of symmetrical droplet freezing and shattering, requiring the prior accretion of small droplets (Choularton et al., 1980; Griggs and Choularton, 1983). The formula adopted here (Eq. 4) assumes a much greater role played by large droplets, as described by Hallett et al. (1978); as shown in Fig. 12, concentrations of small liquid droplets were higher within background ice regions compared to SIP regions during M-Phase; therefore, calculating a splinter production rate that scales with increasing small droplet concentrations would not be consistent with the observations.

Rates were calculated..."

b. Is the concentration of rimers kept constant in this approach?

No, the rimer concentration varies at 1 Hz resolution. The 1 Hz-calculated splinter production rates are averaged across each individual cloud segment, as stated in Sect. 3.2.4..

c. The collection efficiency is calculated based on the terminal velocities of both rimers and droplets. Does this also determine collision velocity? Would it not be too small for efficient rime-splintering SIP? Why is the collision velocity not part of the splinter production rate formula?

The collection efficiency is strongly dependent on ice crystal size/habit, cloud droplet size and rimer-droplet relative fall velocity (i.e. collision velocity); therefore, collision velocity is already, to some extent, accounted for in the collection efficiency term. Furthermore, the terminal velocity of a cloud droplet of size less than ~ 50 μm (comprising the majority of cloud droplets sampled during M-Phase) is around an order of magnitude below the terminal velocity of rimers/graupel (Beard, 1976), thus the collision velocity will be dominated by the graupel terminal fall velocity, which is included in the SPR formula used in this study.

Mossop (1985) showed that the rate of ice splinter production from riming is relatively constant between collisional velocities of ~ 2 to 5 ms^{-1} and, in fact, remains high at velocities as low as 0.5 ms^{-1}, with some degree of splintering likely to take place at the lowest velocities at which riming is possible, around 0.2 ms^{-1}; therefore, the relatively small collisional velocities likely in the mostly low turbulence in-cloud conditions observed during M-Phase should still have been conducive to efficient SIP by rime splintering.

d. The CPI provides quite clear images of rimed ice crystals (Figures 7, 10, 16). Would that be possible to derive the fraction of rimed ice crystals and use it for the estimation of SPR?

Theoretically, the fraction of CPI-imaged ice crystals with visible riming could be used to derive a more representative collection efficiency term for the shallow, mixed-phase clouds sampled during M-Phase; however, information on how this riming fraction varies with ice crystal size and cloud droplet number would also be needed. Such detailed statistical analysis of cloud particle imagery requires use of an automatic ice crystal habit classification scheme, as in Jaffeux et al. (2022); the timescales and resources involved in the development, testing and implementation of such a scheme are outside the scope of this study. Furthermore, accurate determination of riming fraction would likely also require higher resolution ice crystal images than those presented in Figs. 7, 10 and 16; for instance, Waitz et al. (2022) analysed riming fraction during several in-situ aircraft campaigns in the Arctic and Southern Ocean using much more detailed imagery collected by the PHIPS probe.

e. **Can the mass of rime be estimated from the CPI images and how would that translate into the SPR using the Harris-Hobbs and Cooper relationship?**

Image processing algorithms can be used to estimate riming mass fraction, as in Maherndl et al. (2024); rimed crystals are automatically identified from optical array probe imagery and their level of shape complexity (more regular, spherical shapes assumed to be more heavily rimed) is combined with assumptions on theoretical fully-rimed maximum particle dimensions and rimed ice density to calculate an approximate mass of accreted rime per ice particle. Similar analysis for M-Phase may be possible using the 2D-S imagery (CPI images likely not clear enough); however, as stated above in response to (d), the timescales and resources required for this analysis are well outside the scope of this study.

Additionally, it should be emphasised that (as already stated in the manuscript on Page 250 Line 802 in Sect. 3.2.4) the SPR calculations and subsequent glaciation timescale estimates in this study act as order-of-magnitude feasibility tests for the H-M process dominating SIP during M-Phase, as in Järvinen et al. (2022). They provide a way to quantitatively evaluate the likelihood that rime splintering alone can account for the highest ice concentrations measured during the campaign; in Järvinen et al. (2022), it was concluded that other SIP processes were dominant based on estimated glaciation timescales via rime splintering that were orders of magnitude greater than the expected lifetimes of the sampled clouds. In our manuscript, we determine that the H-M process is likely the main source of ice multiplication, though other mechanisms may be acting concurrently. The additional analysis suggested in (d) and (e) may improve the accuracy of our SPR estimates, but the conclusions would likely remain unchanged; however, the application of automatic image processing algorithms to determine riming fraction and mass during M-Phase could provide the basis of an excellent future stand-alone study.

f. **Why is the SPR plotted against the mean ICNC (figure 12), and not against the concentration of rimers (ice crystals larger than 300 μm)?**

We plot total measured ice number concentration (N_{ice}), instead of the rimer concentration, against the SPR (dN_{ice}/dt) as this allows us to derive a relationship in the form $\frac{dN_{ice}}{dt} = aN_{ice}^{b}$, which assumes that all measured ice crystals have been generated by rime splintering. Subsequently, we can estimate a timescale (dt) for any given ice concentration increment (dN_{ice}) that corresponds to ice production exclusively by rime splintering. In reality, some proportion of observed ice will have been generated by primary production or other SIP processes; thus, the glaciation timescales via rime splintering discussed for M-Phase in this study are likely conservative estimates.

g. Comparing the SPRs shown in Figure 12 with the SPRs from Harris-Hobbs and Cooper (1987) I find very similar maximum values on the order 0.1 to 1 L^{-1} s^{-1}, which is surprising given the much stronger updrafts and ten-fold higher number concentration of droplets compared to this study. A direct comparison of both datasets would be interesting.

Yes, the range of SPRs calculated for cumulus cloud segments sampled during M-Phase, shown in Figure 12, is very similar to those presented in Harris-Hobbs and Cooper (1987); ~ 10^{-4} to 10^{0} L^{-1} s^{-1}. However, we are not in agreement with the reviewer's suggestion that this is a surprising result. It's true that the updrafts measured during the three aircraft campaigns discussed in Harris-Hobbs and Cooper (1987) are stronger than in M-Phase, but (a) updraft speed is not included in either SPR formula (though it should be somewhat correlated with large droplet concentration), and (b) as previously discussed, strong updrafts are not necessarily required, or even most conducive to strong rime splintering; weak-to-moderate updrafts may be more likely to promote the positive feedback of splinter production, growth, riming and repeated splintering (Hallett et al., 1978). Additionally, the mean large droplet concentrations (D > 24 μm) in cumulus regions were similar during M-Phase (4.8 cm^{-3}) and two of the aircraft campaigns in Harris-Hobbs and Cooper (1987); 3.7 cm^{-3} in California and 5.6 in Montana cm^{-3}. The largest SPRs shown in Harris-Hobbs and Cooper (1987) from the Florida measurements corresponded with much higher large droplet concentrations of around 16 cm^{-3}, which should produce higher splintering rates than M-Phase if all other conditions were broadly similar; however, during M-Phase the mean graupel concentrations in cumulus clouds (16.5 L^{-1}) were nearly an order of magnitude higher than during the Florida campaign (2.3 L^{-1}), which will partially compensate for the impact of lower droplet concentrations. Note, Harris-Hobbs and Cooper (1987) defined graupel (or rimer particles) as those larger than 250 μm, with a threshold of 300 μm used in our study; thus the large discrepancy is not simply a result of differing definitions between studies.

It is reassuring that both SPR calculations produce similar results in cumulus clouds of similar microphysical and thermodynamic properties.

We have updated the manuscript on Page 26 from Line 845 to state the similarities between the splinter production rates calculated in our study and those presented in, Harris-Hobbs and Cooper (1987):

"Figure 13 also shows splinter production rates estimated by Mossop (1985) and Järvinen et al. (2022), following the same approach as used in this study, for wintertime cumulus…"

On Page 26 From Line 854:

"It is reassuring to note that the range of splinter production rates calculated by Harris-Hobbs and Cooper (1987) for mixed-phase cumulus clouds at several locations across the United States (~ 10^{-4} to 10^{0} L^{-1} s^{-1}), using their alternative formula that additionally incorporates small liquid droplets (D < 12 μm), is comparable to our results for M-Phase (Fig. 12); both studies sampled cumulus clouds of similar microphysical characteristics. Mean concentrations of large, supercooled liquid droplets (D > 24 μm) during two of the three aircraft campaigns

discussed in Harris-Hobbs and Cooper (1987) (3.7 and 5.6 cm^{-3} in California and Montana, respectively) were strikingly similar to those observed within cumulus clouds during M-Phase (4.8 cm^{-3}). Large droplet concentrations were much higher during the other campaign (16.0 L^{-1} in Florida); however, this difference is compensated for to some extent by the graupel concentrations that were on average an order of magnitude greater during M-Phase. Finally, although updraft speeds were a factor of 2-3 greater in Harris-Hobbs and Cooper (1987) than measured during M-Phase, the role of turbulence in initiating and sustaining rime splintering remains uncertain, with some evidence suggesting that weak-to-moderate rather than strong updrafts are more favourable for ice multiplication (Hallett et al., 1978; Sullivan et al., 2017)."

12. **Case study 3 (flight C326), section 3.2.5 and 3.2.6. As in the case studies 1 and 2, the only "hard evidence" for SIP is the higher ICNC as compared to the numbers of INPs active at the temperature range where the ice crystals have been observed. However, the concentration of INPs at -20°C is strikingly equal to the ICNC observed at the same temperature (Figure 13). Could that be an indication that ice crystals have been actually formed as "primary ice" via immersion freezing and then got mixed into the cloud, with the concentrations strongly fluctuating in the alternating updraft-downdraft cells?**

We thank the reviewer for this comment. Firstly, providing "hard" evidence for SIP from in-situ aircraft observations is very difficult, if not impossible; the best we can do is supply as much supporting evidence and analysis as possible from the cloud microphysical and thermodynamic measurements available, as well as the cloud particle imagery. In the case of flight C326, we have shown ice crystal enhancements of ~ two orders of magnitude in the exact temperature zone (-15 to -17 °C) found by previous laboratory experiments to be most conducive to ice-ice collisional break up, in addition to CPI images of heavily rimed, branched ice crystals and aggregates that are consistent with the crystal habits previously observed to favour ice breakup (Takahashi et al., 1995; Grzegorczyk et al., 2023). We cannot rule out that some of the ice observed at -15 to -17 °C has mixed or precipitated down after forming nearer cloud top at ~ -19 °C; however, even if we assume that all the ice observed at -15 to -17 °C is primary ice formed by immersion freezing at cloud top, concentrations would still exceed the INP number at ~ -19 °C by at least an order of magnitude. Furthermore, Takahashi et al. (1995) observed a clear peak in the number of ice crystals ejected following collisions at ~ -16 °C, sharply decaying at colder temperatures towards -20 °C, which is consistent with the comparable N_{ice} and N_{INP} observed at cloud top during flight C326. We have updated the manuscript from Page 27 Line 881 to mention the likely dominance of primary ice production near cloud top, but that greatly reduced ice breakup is expected at ~ -19 °C:

"...and utilised sawtooth profiles throughout, initially sampling stratiform cloud during the outbound leg over the Labrador Sea, which broke up into convective elements for the return leg. Cloud top temperatures as low as ~ -19 °C were reached, corresponding to a maximum theoretical N_{ice} from primary ice production (i.e. N_{INP}) of ~ 1 L^{-1} (Fig. 13); very similar N_{ice} and N_{INP} were observed at ~ -19 °C (Fig. 13) suggesting primary ice production via immersion freezing, rather than SIP, was dominant at cloud top. The highest ice particle concentrations during flight C326 were measured between -15 and -18 °C, reaching 37.4 L^{-1}, which exceeded the associated N_{INP} by around 1-2 two orders of magnitude (Fig. 13); Takahashi et al. (1995) observed a clear peak in the number of ice

crystals ejected following collisions at ~ -16 °C, sharply decaying at colder temperatures towards -20 °C, which is consistent with the comparable N_{ice} and N_{INP} observed at cloud top during flight C326. Järvinen et al. (2022) reported similar ice crystal enhancements…"

13. **Page 26 Line 850: "This broad size distribution suggests that ice crystals of varying stages of growth were collocated during SIP 5, thus meeting the requirement for ice breakup that colliding particles have greatly varying vertical velocities (Korolev and Leisner, 2020)." I don't see how the broadness of size distribution alone is sufficient to decide if ice-ice collision breakup is taking place. Collisions between freely settling compact ice crystals of 400 µm 800 µm do not carry enough kinetic energy to ensure fragmentation; and the information on the in-cloud updrafts is missing. The situation could change if one of the colliding partners would be a fragile snowflake as in the study of Grzegorczyk et al. (2023), but I don't see any diffusion-grown dendrites in the Figure 16a; all I see are rimed ice crystals.**

We thank the reviewer for their comment. We have not claimed that the broad size distributions alone are evidence that ice-ice collisions are occurring, or have recently taken place before the point of observation, as is suggested by the reviewer. As we have explained on Page 28 from Line 908, ice-ice collisional breakup has been found to favour "cloud regions where ice particles with a broad range of sizes are well-mixed, as fragmentation upon impact between ice crystals is most efficient when the difference in their terminal fall velocities is large, maximising the kinetic energy of the collision". Therefore, the broad ice particle size distributions during the SIP periods of flight C326 (Fig. 16) are simply consistent with results from the limited number of previous laboratory studies of ice breakup; this, in conjunction with the temperature zone where maximum ice enhancements were observed and the ice particle imagery, are provided as evidence that ice-ice collisional breakup was active. The manuscript remains unchanged.

14. **Page 29 lines 916-917. "SIP periods were predominantly observed in updrafts (80%), particularly low turbulence, weak updraft (< +0.5 ms-1) regions, but also frequently in moderate updrafts of up to ~ +2 ms-1 (Fig. 17b) …" This is misleading. All discussed SIP regions are not collocated with the regions where vertical velocities have been measured. On one hand, the authors demonstrate the enhancement of ICNC in the SIP regions as compared to the "background" ice crystal measurements; on the other hand, the mean values of updraft velocities measured outside of the SIP regions are used to identify the cloud conditions beneficial for SIP.**

It is not clear what the reviewer is querying here. The vertical wind speed relative frequency histograms shown in Fig. 18b include data from all straight-and-level run (SLR) flight legs across the full campaign; this covers SIP periods from other flights that aren't discussed in detail in the earlier case studies. The pink 'SIP' histogram includes SLR data coinciding with SIP periods only; therefore, the reviewer's statement that 'mean values of updraft velocities measured outside of the SIP regions are used to identify the cloud conditions beneficial for SIP' is incorrect. Of course, as we can only focus on SLR data for reliable vertical wind speed analysis, the amount of data included in Fig. 18b is lower than the other histograms presented in Fig. 18; however, the SLR data subset is ~ 15 % of the total campaign sample size, with a similar split between SIP and non-SIP periods, so still provides a statistically meaningful result. The manuscript remains unchanged.

15. **Page 29 lines 918-921. "As previously discussed, updrafts provide a favorable environment for SIP, and particularly rime splintering, by supplying moisture to generate sufficient water supersaturation for the activation of aerosol into cloud droplets, which grow efficiently within updrafts to the necessary size (Dp > 24 μm) for riming and the initiation of the H-M process." This is very true, but not very helpful for the data analysis, since the supersaturation has not been reported in the manuscript. Besides, the droplet measurements show similar numbers of droplets larger than 24 μm in background and within SIP ranges, which, according to this argument, should imply sufficient updrafts everywhere.**

We thank the reviewer for their comment. We believe that the relative frequency histograms of large droplet (> 24 μm) concentrations (Fig. 18d) and the newly added droplet size distributions (Fig. 12) for SIP and background ice periods (flights C328 and C329) on Page 23 clearly show that concentrations of large supercooled liquid droplets, needed for riming and initiation of the H-M process, were consistently higher in SIP than non-SIP regions; this supports our conclusion that updrafts strongly contributed to SIP activity in the shallow, CAO clouds sampled during the M-Phase campaign. The manuscript remains unchanged.

Referee 2

This study examines secondary ice production (SIP) events during the M-Phase field campaign, which took place during Oct-Nov 2022. Peak concentrations in ice number concentrations were observed in the Hallett-Mossop temperature range (-3 to -8 degrees C), which seemed to be predominately in convective CAO regimes. The collocation of INP measurements with in-situ aircraft cloud probes during M-Phase observed CAO events nicely extends previous airborne analysis performed for both the Arctic and Southern Oceans. I think this manuscript does a very good job of examining a complicated cloud microphysics process in dynamical "grey area" clouds generated by CAOs that are notoriously difficult to assess in models. Evaluations of SIP are limited in general, and especially in CAO clouds. The topic, therefore, is a very important one and results of this study will represent valuable contributions to the scientific community and will be of high interest to the EGUsphere readership. The figures and tables are highly detailed, and I especially like Figure 18 as a nice summary figure. With this said, I think the authors need to address a few critical caveats before this manuscript is ready for publication. Namely, I believe the manual inspection components need to be replaced with something automated (i.e., implementing a habit classification scheme) so these results can be reproduced independently. The authors also need to include analysis and further discussion of the implications of cutting off 2D-S data at 100 μm as many cloud particle-sized ice particles are going to form in the < 100 μm range. Finally, I also think there's some coincidence here that peak ice concentrations in relatively warm regions (-3C to -8C) were in convective clouds, which is where H-M was understandably dominant. I think the authors also need to expand their analysis to more directly compare stratiform and convective clouds in an "apples to apples" comparison to further support their conclusion about H-M being the dominant process. I have a few main comments to address, as well as several technical comments.

We thank the reviewer for their positive feedback and constructive criticism.

Major Comments

1. **The Clarke et al. (2025) is mentioned repeatedly as the main overview paper, but it appears to not (yet) be published either. I would expect this work will be published before this present manuscript is ready, however, in the event it's not – it will be critical that the reader can independently review the most essential details from the Clarke et al. study in the event publication of that manuscript is delayed. For this reason, I think the "complete list and full description" of instruments mentioned in L180-182 should be adapted and reproduced as appendix material in this manuscript.**

We thank the reviewer for their comment. A citable preprint of the Clarke et al. (2026) paper is now accessible (https://essopenarchive.org/doi/full/10.22541/essoar.15004594/v1) and has been added to the reference list on Page 40 from Line 1224:

"Clarke, S. J., Abel, S. J., Nott, G. J., Raif, E. N., Tarn, M. D., Biggart, M., Wu, H., Ashmore, D. W., Barr, S. L., Barrett, P. A., Bower, K. N., Bowles, J., Connolly, P. J., Cotton, R., Evans, M. D., Flynn, M., Foster, P. B., Fox, S., Gallagher, M. W., Hu, K., Kent, J., Lloyd, G., Marsden, N. A., McQuaid, J. B., Robinson, J., Tiddeman, D., Wilson, A., Wivell, K., Field, P. R., Choularton, T. W., and Murray, B. J.: Marine cold-air outbreaks over the Norwegian-Barents and Labrador seas: observational datasets from the ACAO and M-Phase aircraft campaigns, ESS Open Archive, https://doi.org/10.22541/essoar.15004594/v1, 2026."

This preprint now provides any reader of our manuscript with the ability to review the fine details of every instrument deployed during the M-Phase campaign, as well as the complete flight strategy. However, having said this, we believe that the campaign and instrument details provided in our current manuscript (Sect. 2) are sufficient for a strong understanding of the data presented, without needing to consult Clarke et al. (2026). The manuscript remains unchanged.

2. **Given the focus on secondary ice production, ice particles < 100 µm would be logical to examine during periods of known SIP. The 100 µm cutoff for 2D-S ice particles is understandable instrumentally (as this is well known in 2D-S studies), however, this cutoff reduces the size range that SIP studies use as a key tracer of generated secondary ice. In my view, given this understandable-but-critical limitation, this limits the authors' ability to diagnose the onset of SIP and to reconcile inferred splinter rates against observed small-ice populations.**

We thank the reviewer for this comment and agree that ideally it would make sense to examine ice particles of size less than 100 µm when investigating SIP processes with in-situ optical probe data. However, as explained in Sect. 2.1.1: (a) when initially processing the raw 2D-S data we had to set a minimum particle size threshold to be accepted for further analysis, and based on previously reported large uncertainties determining both the size and concentration of particles with $D_P < 50$ µm (Baumgardner et al., 2017), we set the lower limit at 50 µm, and (b) early analysis of 2D-S particle size distributions for M-Phase revealed systematically high concentrations of particles between 50 and 100 µm across both flights which captured strong evidence of SIP (e.g. C328 and C329), as well as those showing negligible evidence of active ice multiplication (C330 and C331); this is consistent with previous 2D-S testing (Lawson et al., 2006) that revealed measurements of small particles of size less than 100 µm were likely to be contaminated with out-of-focus liquid droplets erroneously identified as ice crystals. To support (b) further, we have added 2D-S ice particle size distributions averaged across flights C328, C329, C330 and C331, as well as an example of out-focus-droplets imaged by the 2D-S from C330, to Appendix A. We have also updated the manuscript on Page 7 Line 226:

"...revealed systematically high concentrations of ice between sizes of 50 and 100 µm across all flights, including those that captured strong evidence of recently active SIP processes (e.g. C328 and C329) and those showing negligible evidence of ice multiplication (e.g. C330 and C331) (Fig. A1 in Appendix A); this is consistent with results..."

Particles of size < 100 µm comprised 44% and 33% of total N_{ice} during flights C330 and C331, respectively, compared to only 9% and 12% during flights C328 and C329, respectively. This is counterintuitive if we assume all observed 2D-S particles of size < 100 µm were, in fact, ice crystals and not out-of-focus liquid droplets; flights C330 and C331 had mean measured N_{ice} (including particles smaller than 100 µm) of 0.6 and 1.8 L^{-1}, significantly more comparable to the associated INP concentrations versus flights C328 and C329, which had mean N_{ice} of 15.4 and 27.1 L^{-1} and captured strong evidence of active SIP process, as discussed in detail in Sect. 3.2. SIP-dominated clouds should contain a higher proportion of smaller, more recently generated ice crystals than clouds with a stronger primary ice component.

Furthermore, flight C328 captured the most explosive ice multiplication event throughout the entire campaign, with SIP period 1 (Sect. 3.2.2) having a mean N_{ice} of 174.6 L^{-1} (including particles smaller than 100 µm); however, only 11% of these ice particles were of size < 100 µm. From Fig. 8 (Sect. 3.2.2), SIP 1 was clearly dominated by ice crystals of size between 200 and 250 µm and we are confident that ice multiplication had occurred close to the point of observation. This demonstrates both how fast newly formed secondary ice grows by vapour deposition in mixed-phase clouds and how difficult it is to detect secondary ice at the exact onset of SIP with in-situ aircraft observations.

To explore the extent to which the exclusion of ice particles smaller than 100 µm may affect our splinter production rate analysis, and specifically how our estimated glaciation timescales via the H-M process are impacted by potentially underestimating N_{ice}, we have re-plotted Fig. 12 with particles of D_P < 100 µm included and added it to Appendix D. As an upper-bound estimate, if we assume all HI particles detected with the 2D-S of size less than 100 µm were ice crystals, as opposed to out-of-focus droplets, estimated timescales for an N_{ice} increase of 1 to 100 L^{-1} due solely to the H-M process would increase to ~ 7hrs (all clouds together), 4hrs (convective), and 19 hrs (stratiform), i.e. ~ a factor of 2. However, as discussed above, it is likely that a large fraction of observed particles smaller than 100 µm were indeed out-of-focus droplets rather than ice, thus our previously discussed glaciation timescales from Table 4 can still be considered reasonable and informative estimates. We have updated the manuscript on Page 24 from Line 792:

"...Note, as discussed in Sect. 2.1.1, particles of D_P < 100 µm were excluded from our analysis given the likelihood that they would be heavily contaminated with out-of-focus liquid droplets. However, if in-situ measurements are made within a few minutes of the onset of SIP, we would expect to measure high concentrations of particles smaller than 100 µm, following previously

reported ice crystal depositional growth rates within the H-M process temperature range (Fuchs et al., 2025). Therefore, as an upper-bound estimate, if we assume that all HI particles detected with the 2D-S of size less than 100 µm were ice crystals, N_{ice} will increase on average by ~ 45 % in stratiform and ~ 10 % in convective cloud regions (Fig. D1a in Appendix D); subsequent glaciation timescales by rime splintering are estimated to increase to ~ 7 hours (all clouds), ~ 4 hours (convective), and ~ 19 hours (stratiform), i.e. around a factor of 2. However, the bimodal PSDs (and 2D-S imagery) shown in Fig A1 strongly indicate that a large fraction of particles < 100 µm were likely not ice crystals, but instead out-of-focus droplets (Sect. 2.1.1), thus our $\frac{dN_{ice}}{dt}$ *- N_{ice} relationships and glaciation timescales presented in Table 4 can still be considered reasonable approximations of conditions during M-Phase."*

3. **Furthermore, the paper's SIP/non-SIP period classification seems to rely heavily on manual image inspection (habit classification is explicitly acknowledged as an avenue for future work in the conclusions). The authors, therefore, need to address this caveat. I believe this can be accomplished by adding a sensitivity test for the lower-size cutoff (i.e., provide some estimate for how many ice particles are likely excluded or describe how excluding this size range might affect conclusions drawn across both convective/stratiform regimes), or by implementing a habit classification scheme that can clearly classify primary vs. secondary ice. I think the authors need to replace the manual inspection components as doing so is very hard to reproduce, and implementing a habit classification will allow for clear reproducibility in identifying SIP-generated (and non-SIP generated periods). This manuscript needs to implement something automated here so the relative frequency histograms can be independently computed and verified.**

We thank the reviewer for their comment. The impact of excluding particles smaller than 100 µm on mean N_{ice} in stratiform and convective cloud, and how this may affect conclusions drawn regarding estimated rime splintering rates, has been addressed with the sensitivity study described in response to comment (2).

As already mentioned in Sect. 4 of our manuscript, a future more statistical approach to diagnosing SIP and quantifying relative SIP frequencies in stratiform and convective regimes would greatly benefit from the implementation of an automatic ice crystal habit classification scheme, such as described by Jaffeux et al. (2022). However, as already discussed in response to Reviewer 1's comments, the timescales and resources involved in the development, testing and implementation of such a scheme are unfortunately well outside the scope of this study.

Furthermore, we do not believe that a habit classification scheme would sufficiently improve the current study to justify the work required in implementing it. In the current manuscript, manual inspection of the imagery is conducted by way of simply clarifying that the observed isolated pockets of elevated N_{ice}, several orders of magnitude above corresponding INP concentrations, are likely due to active or recently active SIP processes. We subsequently select representative samples of the imaged ice crystals to show the most common habits within a particular SIP region to help inform on the likely active SIP mechanism; we have not quoted any statistics regarding ice crystal habits. Given all the data and imagery has been made readily available, we believe our work should be reproducible by any expert in the field; of course, we acknowledge there is a degree of subjectivity involved in identifying SIP periods and manually inspecting representative samples of ice crystal images, but the general findings are undoubtedly reproducible, specifically regarding the trends in microphysical properties in SIP and non-SIP regions shown by the relative histograms in Fig. 18.

The only quantification of SIP frequency provided in the manuscript is at the end of Sect. 3.3 where we have stated the % of observations in stratiform and convective regimes that were made within identified SIP regions; this was provided by way of summarising the clear differences in SIP activity across the CAO regime transition. Overall, given our methodology was based on identifying clear and obvious regions of SIP activity, we acknowledge that we have likely underestimated SIP frequency, particularly in stratiform cloud where SIP may be active but weaker under less turbulent conditions and thus harder to identify. To clarify this, we have updated the manuscript on Page 33 from Line 1025:

…"quantification of SIP frequency is limited by (a) our general methodology based on manually identifying distinct regions of strong SIP activity, possibly leading to some weaker regions of ice multiplication being excluded, with SIP frequency in less turbulent stratiform cloud, in particular, likely underestimated, and (b) the previously discussed sampling biases…"

4. **I like the idea of separating convective and stratiform regimes, however, it seems only flight C324 is the only unambiguous stratiform CAO regime < -10C according to Table 1 though there were twice as many overall stratiform cloud segments. The central claim that the H-M process was the most likely explanation should be softened unless the authors can provide stronger quantitative evidence between the convective events and the stratiform CAO events – Table 3 strongly suggested (based on mean and 95th percentile Nice) that the H-M process is confined mainly to convective clouds (this is concluded as well in the manuscript. I think the manuscript needs at least one matched or "apples to apples" comparison where representative stratiform and convective subsets are restricted to common temperature bins, common cloud-relative heights. This could also be done by sub-setting flights that sampled both regimes within the same CAO event. I think this is necessary to assure the readers that the conclusions drawn are for a real regime effect rather than a temperature-sampling effect.**

We thank the reviewer for their comment and agree that an "apples to apples" comparison between stratiform and convective subsets is required to confirm that the manuscript's main conclusions are real and not based on a temperature-sampling effect.

Firstly, we note that flight C322, in addition to C324, unambiguously sampled stratiform cloud at temperatures > -10 °C and largely within the H-M zone; flight C323 took place later the same day (24th October 2022) and exclusively sampled the convective regime downwind within the same CAO event. Therefore, to provide an "apples to apples" comparison we have reproduced the relative frequency histograms of cloud microphysical properties from Fig. 4 for flights C322 and C323 only (added to Appendix C as Fig. C1). Cloud top temperatures during both flights were similar, averaging -8 to - 9 °C, with 68 % (692 s) and 85 % (611 s) of in-cloud measurements taking place within the H-M temperature zone during C322 and C323, respectively. Panels (b) - (d) in Fig. C1 show data restricted to temperatures within the H-M range only. We have updated the manuscript on Page 16 from Line 538:

"To help verify whether this stratiform-convective cloud contrast was a real regime effect and not driven by temperature-sampling biases, we repeated the analysis from Fig. 4, but for flights C322 and C323 only (vertical wind speed excluded as flights consisted solely of sawtooth profiles) (Fig. C1 in Appendix C). These flights sampled the stratiform (C322) and convective (C323) regimes of the same CAO event on the same day, enabling a more direct regime comparison. Both flights sampled the same air mass, with cloud top temperatures remaining relatively consistent throughout (Table 1), and observations were predominantly made within the H-M temperature range, with 68 % (692 s) and 85 % (611 s) of in-cloud measurements taking place between -3 and -8 °C during C322 and C323, respectively. Ultimately, the relative frequency histograms shown in Fig. C1 for both flights, which only include data recorded at temperatures within the H-M range to further eradicate any potential sampling bias effects, mirror the key stratiform-convective regime differences shown for the full campaign in Fig. 4; cumulus cloud regions generally comprised higher concentrations of large liquid droplets (D_P > 24 µm) but were substantially more glaciated than the stratiform cloud layer sampled upwind of the regime transition."

Specific Comments:

L43: "McCoy et al.; 2017" should be "McCoy et al., 2017." Check elsewhere in the text for consistency.

L53-L57: This sentence beginning with "The composition of these mixed-phase clouds…" could be broken into two sentences

L78: "Hallet and Mossop, 1974" should be "Hallett and Mossop, 1974" (Hallet has a missing t).

L111: Consider "breakup" or "break-up" instead of "break up"

L144: "Korolev et al, 2020" should be "Korolev et al., 2020" (missing period after al)

L247-248: Do you mean to say "sample volume is airspeed dependent"?

L280: "Constant-altitude filter legs lasted for typically 20-30 minutes" would read better as "Constant-altitude filter legs typically lasted 20-30 minutes"

L312: Consider rewriting it as "Cloud regions with µ3 < 0.1, 0.1 < µ3 < 0.9, and µ3 > 0.9 were classified as liquid, mixed phase, and glaciated (ice), respectively."

L440: Should be a semicolon after Takahashi et al., 1995;

L443: "1 to 0.1 Hz resolution" should be "1 Hz to 0.1 Hz resolution"

L513: Do you mean to reference Fig. 4e (not 4d) since you're referring to fully glaciated cloud regimes (hence making 4e more appropriate as it's the ice mass fraction panel)?

L518: I think you meant "specific temperatures zones" and not "specific temperatures zones"

L543: I think you mean "innovative" and not "innovate"

L601: Please check the "154.3.1" value.

L715: Should Production be capitalized here?

L726: "an N_{ice} increase" should be "an increase in N_{ice}."

L784: I'd say either 45 min or 45 minutes.

L809: "1-2 two orders of magnitude"... I think the two is put in here by accident.

Figure 13: Is this a single-flight figure (i.e., just 28th Oct 2022)? Please clarify your caption.

L951: "high resolution" should be "high-resolution"

L924-925: "weak-to-moderate updraft regions (0 to +2 ms-1) were particularly suitable..."

We thank the reviewer for these specific comments. The manuscript has been updated accordingly to address each of them.

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