Referee #1, 2nd review of Part 2 of CALIPSO paper

Author responses to the Reviewer 1

Format: The reviewers' comments are in normal font while author responses are in red font. Text in red font italics indicates revised or added text in the revised manuscript.

Again, we thank the reviewers for their thoughtful comments and the considerable amount of time they invested for improving this manuscript. Below are our responses to the comments concerning their second review:

General comments from Reviewer 1:

Review of Mitchell and Garnier, 2024, round 2

I would like to thank the authors for addressing many of the comments. In my opinion, the manuscript is now ready for publication. The authors did a better job of condensing their figures in the revised manuscript. I believe this is high-quality, innovative work of great scientific importance that deserves immediate publication in ACP.
However, due to its length and the large number of figures and subplots, I still believe that the broader community will not fully appreciate or understand the important science presented by the authors.

Please not that I still added a number of additional minor comments that the authors may want to consider including in the manuscript:

The two studies led by Tim Lüttmer that examine the origin of ice clouds by tracing each source of ice separately in a high-resolution model could be mentioned in the introduction (or other parts of the manuscript):

Lüttmer et al., 2025, https://acp.copernicus.org/articles/25/4505/2025/, see their Fig. 6 for the example of deep convection.
Lüttmer et al., 2025, in typesetting right now, preprint at: https://egusphere.copernicus.org/preprints/2025/egusphere-2025-185/ for the example of a WB cirrus.

Appendix A, but maybe also lines 130–141 and other discussions related to the classification of WBC and in situ cirrus, particularly in the tropics:
In my opinion, Figure 7a shows that the WBC classification does not work very well in the

tropics if its purpose is to highlight the microphysical origin of cirrus.
There are many samples to the right of the two theoretical curves. This is less problematic for other climate bands.
One option would be to state in the Appendix that discerning the microphysical origin is beyond the scope of such a simple classification, and to justify the use of the current classification scheme.

We have classified cirrus in terms of in situ and WBC clouds because of (1) its similarity to the in situ vs. LOC classification and cloud property differences thereof and (2) the practicality of implementing this classification for CALIPSO sampling. The motivation for using this classification derives from (1) and we explored whether similar differences in cloud properties are observed. Similar differences were observed (namely IWC) but it turns out that the largest differences were IWP and τ (which was not evident in previous studies based on in situ sampling from aircraft). This was the main finding for this classification. We agree with the reviewer that our study was not designed to assess microphysical origin, and we have added this disclaimer near the top of the 3rd paragraph in the Appendix: "*While our cloud classification method (in situ vs. WBC) was not designed for evaluating the cloud's microphysical origin (i.e., using back-trajectory analysis) like the in situ-LOC classification was, it may be sufficiently similar to capture some of the microphysical distinctions observed between the in situ and LOC classifications.*"

Figure 3:
The authors could cut the colormap at around 80 microns. This would provide greater contrast for the most commonly encountered ice crystal sizes. In its current form, it is difficult to spot large size differences.

In the current colormap, we use 10-μm steps between 10 and 80 μm and only one color between 80 and 137 μm, to show where D_e is found larger than 80 μm while keeping sufficient resolution for D_e < 80 μm. It is not clear that cutting the colormap at around 80 μm will improve the figure.

Lines 270-272:
Isn't it simply the detrainment of ice formed at warmer temperatures at higher altitudes, which appears to violate the Clausius-Clapeyron limitations? I think it is incorrect to talk about 'moisture', as deep convection and freshly detrained clouds typically have a relative humidity (RH) of about 100%. What we really have there is the transport of ice from lower to higher levels.

The word "moisture" has been replaced by "ice mass".

Lines 286-300:
It would be useful to include the full derivation of the equations used in the appendix, particularly equations 2 and 4.
How does one derive equation 2 from the standard Clausius–Clapeyron equation? While this may sound obvious, it is still good practice to document it.

There is a very eloquent derivation of Eq. 2 in Ch. 3 of Lamb and Verlinde (2011). Therefore, we have modified the sentence introducing Eq. 2 as follows:
"Reformulating Eq. (1) *as described in Sect. 3.3.1 of Lamb and Verlinde (2011)* gives an expression useful in calculating the supersaturation required for homogeneous ice nucleation:"

Similarly, an intermediate step could be included to clarify equation 4, e.g. by explicitly writing the ideal gas laws: Rho = e/RvT and Rho_(hom) = e_(hom)/RvT, Rho_(si) = e_(si)/RvT. These derivations could go in the appendix.

The sentence introducing Eq. 4 has been modified as:
Noting that the water vapor pressure at S_i^f, e_{hom}, is simply $e_{hom} = S_i^f e_{si}$, water vapor densities are obtained from the Gas Law *($\rho_{hom} = e_{hom}/(R_v T)$ and $\rho_{si} = e_{si}/(R_v T)$, R_v = gas constant for water vapor)* to predict the maximum IWC resulting from hom:

Lines 324-326:
Because of close agreement, do we really need both versions in all the figures?

Because we evaluate different quantities in Fig. 7 (N_i) and in Fig. 8 (IWC and De), it makes sense to show the empirical and theoretical predictions for hom. However, in the supplement showing the equivalent of Figs. 7 and 8 but over land (Figs. S7 and S8), only the theoretical prediction for hom is used. Similarly, only one version is shown in Fig. 11, and in Fig. S12 which is the equivalent of Fig. 11 but over land.

Section 3.3.1
I think the language could be simplified here.

We have rewritten this section to simplify it and make it clearer.

Figure 13:
Is there a need for the Hom fraction axis and information in this figure? It's already plotted in Figure 12.

Including the hom fraction profile allows the reader to see how it influences the τ difference between the het τ profile and the overall τ profile. This would not be very evident if the hom fraction profile was only in Fig. 12.

Also, it's tiring to fight through 12 subplots in 6 of the manuscript's figures. This clearly doesn't help convey your key messages, as it dilutes the message too much. Readers lose focus and motivation to continue.

We show these because we think there are often significant differences between latitude bands and/or seasons. Some readers may be intrigued by this. We could have submitted to Science or Nature, but we chose ACP since they allow more research findings to be published.

Note on some of the figures:
It might be better to show NH-JJA and SH-DJF close to each other because we want to be able to compare summers and winters in both hemispheres.

We think that the current figure arrangements allow readers to do this adequately.

Lines 575-576:
I think it's clear that homogeneous drop freezing isn't the main source of ice in WBC. The fact that most GCMs simulate this phenomenon is likely due to the biases in numerical models, especially low-resolution GCMs.
The Lüttmer et al., 2025 study (https://egusphere.copernicus.org/preprints/2025/egusphere-2025-185/) may be a more updated and trustworthy modeling reference.

Even after considering one of the key figures (Figure 16, specifically the COD-weighted part), there's still a lot of content in Figures 17 (again, 12 (!) subplots) and 18. I know that this is important for proposing the cirrus cloud formation and evolution idea and the Figure 19, but that could also be published e.g. in a letter format, as a separate publication.

We think that the context in which the cirrus cloud characterization idea is presented adds significantly to the concept. Therefore, we prefer not to separate this concept from the paper.

Figure 20b should include a legend. Additionally, the cloud scene in the photo could be highlighted in the satellite image.

A red "X" was added in the satellite image to highlight the location of the cloud scene in the photo.

The caption for Fig. 20b (right) was amended as follows: "*Low clouds are white-grey while high clouds (e.g., blue colors) tend to have lower brightness temperatures (BT), with lowest BT in dark blue and green. The red "X" indicates the location of the cloud scene in the photo.*"

Author responses to the Second Review from Referee #2

The format below is identical to that used by Referee #2 in her/his second review, except that our **responses to this second review are in green font**. That is, the first review from Referee #2 is in black font, followed by our responses in red, followed by the second review from Referee #2 in blue, followed by our second author responses in green. New text added is in italics (green font).

Second review of

Advances in CALIPSO (IIR) cirrus cloud property retrievals – Part2:
Global estimates of the fraction of cirrus clouds
affected by homogeneous ice nucleation

David L. Mitchell and Anne Garnier

General:

The manuscript has improved considerably. The authors put in a lot of work and incorporated almost all of the suggestions, for example, the Figures in the new version are much much better! However, there are still a few points for discussion regarding the previous comments and some new recommendations, which are all listed below.

Old G 4 Hom-affected and het-only cirrus clouds (Sections 3.2 and 3.3 etc.)

E.g. Line 293ff: *Characteristic in all plots (Figure 12 and subsequent figures) is a broad region on the left side (relatively low αext) where Ni < 30 L -1, apparently corresponding to het only. To the right of this region is a gradient of increasing N i, culminating in values of Ni >2000 L-1. This gradient region is likely produced by varying degrees of hom activity.*

I am not convinced by this classification and would interpret this central point of the paper differently, as I will explain in the following.

I agree that there are two cirrus regimes, as described in Kramer et al. (2016, 2020). Here α_{ext} (color coded by N_i) is used to make this visible. The region of high α_{ext} and N_i (hom acitivity') corresponds approximately to the area of high IWC and N_i in Fig. 6 (top panel) of Kramer et al. (2020). In this region, both in-situ and liquid-origin cirrus clouds are present. For the in-situ cirrus clouds, the interpretation that they are formed by hom (of soluble aerosol particles) is correct, but not for the liquid-origin cirrus. Hom (of cloud drops) can occur within liquid origin cirrus clouds, but is rather rare outside the tropics. **The predominant freezing mechanism of liquid origin cirrus is het**, nevertheless, they can have high N_i .

Dekoutsidis et al. (2023) show that RHi conducive to hom (RHi > 140%) occurs near cloud top irrespective of whether the cirrus clouds are in situ or liquid origin. This is consistent with our findings that show hom contributes to both in situ and liquid origin cirrus (LOC) clouds.

Old G4, new comment: this is true – hom can contribute to liquid-origin cirrus in a second step: liquid-origin cirrus enter the cirrus temperature range completely glaciated by het freezing. When RHi rises again up to the homogeneous freezing threshold due to further ascent, a new (in situ!) ice nucleation event occurs.
I recommend to explain this in your paper, otherwise readers might be confused about homogeneous freezing in liquid-origin cirrus (which can be only drop freezing at 235 K).

By the way, these liquid-origin cirrus are a special case, a mixture between in situ and liquid origin cirrus. This is discussed in a new article by Gasparini et al. (2025) https://egusphere.copernicus.org/preprints/2025/egusphere-2025-203/ (their Figure 2) and these cirrus clouds are referred to as an 'dual-origin'. Maybe you want to mention this in your paper.

Figure 2 of Gasparini et al. (2025) shows dual-origin cirrus mostly near the top of a simulated tropical anvil cloud, where in-situ nucleation has occurred more recently than detrainment. Figure 7 of our revised paper (May version) is shown for in situ cirrus clouds (panels a-c) and for warm-base cirrus clouds (WBC, panels d-f) and indicates that hom is very active in WBCs even at the warmest cirrus radiative temperatures. This can be understood if the cloud is glaciated by het freezing at T > 235 K and supersaturation s_i is already near the hom threshold. Then some cloud parcels are transported upwards across the 235 K isotherm with haze solution droplets freezing as they cross this isotherm. We have added new text to make this clear at Line 285:
"*For WBC, hom appears quite active even at the highest temperatures. This can be understood if the cloud is glaciated by het freezing at T > 235 K and ice supersaturation s_i is already near the hom threshold. Then some cloud parcels are transported upwards across the 235 K isotherm, increasing s_i, with haze solution droplets freezing as they cross this isotherm.*"

The region with low αext and Ni is defined here as 'het only'. However, I think the composition of the cirrus clouds in this region are much more complex. First to mention, in this region there are also both in-situ and liquid origin cirrus present. Further, the in-situ origin cirrus could have formed either hom or het, since hom also produces only few ice crystals at warm temperatures and low updrafts.

Agreed. We acknowledge this in text added to Sect. 3.2 that is given next in italics.

But, most importantly, the concentration of hom cirrus with initially high Ni (and thus αext) decreases quite rapidly in the warming phases of the ubiquitous mesoscale temperature fluctuations where the environment is subsaturated (Jensen et al., 2024). This means that they are moving from the hom affected regime to what is now defined as het only. This can be seen also in Fig. 6 (bottom panel) of Kramer et al. (2020) - the thinner the cirrus (and the lower N i) the more frequent the cirrus clouds are in a subsaturated environment.
The fact that the **cirrus clouds with low αext and Ni are in a subsaturated environment** is also indicated by the decreasing De to the left of Dmax (Figures 15 and 16), because under this condition, the thinner the cirrus clouds and the lower the Ni, the smaller the ice particles. In an at least saturated or supersaturated environment, the ice particles would be larger with decreasing Ni, i.e. there would be no maximum in De, but an increase, maybe with a change of the slope during the transition from one to the other regime.

Old G4 Reiteration: My comment was:
'In summary, I believe that this region is a mixture of in-situ origin cirrus clouds of different ages, which could have formed either het or hom, and aged liquid origin in the dissolution stage. I recommend reconsiding the naming and the discussion of the 'het only' cirrus regime.'

It is now mentioned in manuscript that hom might also produce a low number of ice crystals. However, that the cirrus with low αext and Ni coud also be aged liquid origin or dissolving cirrus is missing.

The naming of this cirrus type has changed from 'het only' to ‚het'. However, in all discussions, the impression is still that these cirrus originated purely heterogeneously. While it would be ideal if the cirrus types could be clearly separated, unfortunately, this is not the case. Therefore, I recommend that this cirrus class be better described and named (maybe 'het-mixed' ?).

We agree with this concern and we now define the het category to include dissipating cirrus clouds. Most of the first paragraph of Sect. 3.3 has been rewritten as: "*To address this question, a method is presented that estimates the fraction of hom-affected cirrus clouds such that these clouds are strongly altered microphysically relative to cirrus associated with relatively low N_i, such as cirrus formed through het and aged, dissipating/sublimating cirrus clouds (formed either through hom or het). Henceforth the former will be referred to as hom cirrus and the latter as het cirrus. These dissipating cirrus clouds are microphysically described in Fig. 6 of Krämer et al. (2020) where RH_i < 100%. These two categories will now be described and finally quantified*."

I also wrote:
'What I wonder (although I know it would be a lot of work) is whether this analysis would be better done separately for in-situ and liquid origin cirrus (the derivations presented in section 3.2 only apply to in-situ cirrus anyway)? Especially for in-situ cirrus, the interpretation of the freezing mechanisms would be much clearer, now the liquid origin probably blurs their features.'
Any thoughts about this ?

We followed the suggestion and separated in situ and WBC clouds in Fig. 7 (Sect. 3.2). Previously, these two cloud types were lumped together in this figure. On line 268 (May version), it states: "It is evident that hom-affected cirrus clouds are common in both in situ cirrus and WBC."

We also followed this suggestion for Fig. 9 (oceans) and Fig. 10 (land), which are for in situ cirrus clouds only, while the results for WBC are shown in Figs. S9 and S10, respectively. However, we did not comment clearly on the similarities between the in situ and WBC results. This sentence is now added in the second paragraph of Sect. 3.3: "*Note that these observations apply to both in situ and WBC clouds, suggesting that similar physics applies to both cloud types*."

We also followed this suggestion for Fig. 18.

We thank the reviewer for making us aware of the ambiguity of our results concerning Fig. 12 of the preprint (now Fig. 7). This figure has been revised to show these results for both in situ cirrus and WBC clouds during winter only. It is seen that the results are very similar for both cloud types, with a strong Ni gradient when related to extinction. Assuming WBC are mostly LOC, this indicates that hom is also important for LOC.

As indicated in our responses to Reviewer 1, the freezing mechanisms are now discussed and clarified, with hom ice nucleation in both in situ and WBC clouds proceeding primarily through homogeneous freezing of solution haze droplets. We agree that hom under warmer, low updraft conditions can also produce $N_i < 30\ L^{-1}$, and the relevant section of text has been rewritten as:
"*Characteristic in all plots is a broad region on the left side (relatively low α_{ext}) where $N_i < 30$ L^{-1}. Although hom can produce such low concentrations at warmer temperatures and low updrafts (Kramer et al., 2016), hom tends to produce much higher Ni (Barahona and Nenes, 2009). To the right of this region is a gradient of increasing N_i, culminating in values of $N_i >$ $1000\ L^{-1}$. This gradient region is likely produced by varying degrees of hom activity, although het may also contribute to this gradient under conditions of relatively high INP concentration. It is evident that hom-affected cirrus clouds are common in both in situ cirrus and WBC. The main difference between these cloud types is in the tropics where in situ cirrus often appear to the right of the region predicted for pure hom (i.e., the triangles or squares) which will be discussed below. This may be due to deep convection overshooting the*

temperature level predicted for hom to activate, depositing moisture at lower temperatures where in situ cirrus subsequently form. Results like Fig. 7 are shown in Fig. S7 for land where this 'overshooting effect' is more evident for in situ cirrus outside the tropics, perhaps due to stronger orography-induced updrafts over land ."

In addition, the descriptor "het only" has been removed from the paper, and such clouds are now referred to as het cirrus or het dominated cirrus clouds. See new comment above.

This has been addressed as described above.

Old S 11: Line 440ff: *Most evident when comparing Figs. 15 and 16 for $\alpha_{ext} < 0.3\ km^{-1}$ (where het is expected to prevail) is that median N_i is higher over land (up to a factor of 10), presumably due to higher INP concentrations over land. …*

Or stronger updrafts → enhanced hom over land ?

…. *higher INP over land (which can also be enhanced by stronger updrafts) may be producing a "Twomey effect" in het cirrus clouds over land.*

This is very speculative (over-interpreted?) …. to make this hypothesis more information about INP and updrafts would be necessary.

New text has been added at the end of Sect. 3.3.2 to address this concern:
"*While higher updrafts over land could also enhance INP and Ni concentrations, note that updraft effects are implicit in Figs. 9, S11, and 10. That is, higher updrafts are associated with higher IWC (Hu et al., 2021; Mitchell, 1988) and higher extinction is associated with higher IWCs. The apparent Twomey effect here is associated with $\alpha_{ext} < 0.3\ km^{-1}$ where updrafts are expected to be relatively weak over both ocean and land*."

Old S11, new comment: It would be nice to mention that updraft and hom produced Ni (and thus IWC) are strongly correlated; updraft being the parameter that most strongly triggers Ni production, more than temperature or INP concentration (see Kaercher and Lohmann 2002, 2003; JGR). That is the reason why I am a bit persistent here…

All that is true, but the mentioned correlations between hom Ni vs. updraft and hom IWC vs. updraft apply to the hom regime. This discussion about the Twomey effect in Sect. 3.3.2 applies only to the het regime. The first sentence of Sect. 3.3.2 has been slightly modified to make this more clear: "*Most evident when comparing Figs. 9 and 10 (or Figs. S9 and S10) for $\alpha_{ext} < 0.3\ km^{-1}$ (i.e., the het cirrus regime) is that median N_i is higher over land (up to a factor of 10), presumably due to higher INP concentrations over land*."

Old S 13: Line 550ff: *Also of interest are the seasonal changes in hom fraction between 30°N and 60°N in Fig. 20. Relative dust contributions of the world's main dust regions are …*

more likely to reach cirrus cloud levels in the UT due to ascent within frontal systems, orographic uplift, and dry convection. …

As in point S 11, seasonal changes in hom fraction might also be due to changes in updrafts and not only to be related to INP.

A new paragraph has been added below the paragraph indicated above, stating:
"*It can also be argued that the above seasonal differences in the hom fraction canbe attributed to seasonal differences in vertical velocities at cirrus cloud levels. However, this appears less likely when one considers that strong orographic lifting occurs over the southern Andes Mountains during all seasons, and these vertical motions should be much greater than other vertical motions at cirrus levels in this region (excepting deep convection in summer, but the thick anvils affected by such convection are not sampled by this method). If the hom fraction changes are sensitive to changes in updraft strength, the hom fraction over the southern Andes should not change much between DJF and JJA, but it does. A similar argument can be made for the Himalayas and the Rocky Mountains. Finally, two studies (Sporre et al., 2022; Lin et al., 2025) have documented large microphysical changes in cirrus clouds that were impacted by volcanic aerosol, which are consist with this reasoning.*"

Old S 13, new comment (see also my new comment to S11): A change in N_i over mountains between DJF and JJA is also reported by Sourdeval et al. (2018). They explain "*N_i… in the midlatitude storm tracks and orographic regions are found to be higher during winter months, consistent with stronger jets (Gryspeerdt et al., 2018a).*"

What I'm only now noticing is that Figure 8 in Sourdeval et al. (2018) is not consistent with your Figure 20/14 (old/new version of the paper), where the hom fraction is very low over the Andes during winter and over India during summer. In Sourdeval et al. (2018), Ni(5um) is higher over the Andes in winter (stronger jets) and higher over India in summer (convection). High Ni(5um) strongly suggests hom freezing…

In our Fig. 14, the hom fractions over the southern Andes (south of about 15 °S) are higher in winter (JJA) than in summer (DJF) and N_i in Fig. 2 is also higher in winter than in summer, consistent with the top two panels of Fig. 8 of Sourdeval et al. (2018) which are for temperatures between -60 and -50 C. North of 15° S in south America, N_i in Fig. 2 is larger in DJF (summer) than in JJA (winter), consistent with Sourdeval et al. However, the hom fractions in Fig.14 are smaller in summer than in winter, because the search for maximum D_e yielded larger extinction thresholds in DJF than in JJA (see panels d and j of Fig. S12) and as a result, hom fractions are lower in summer than in winter.

In the tropics, the differences between N_i from our work and from Sourdeval et al. (2018) is explained by the fact that we do not sample most of the convective clouds because our retrieval is limited to clouds having optical depth ≤ 3. We believe that this explains why N_i in Fig. 2 is smaller than in Sourdeval et al. (2018) in some areas. Indeed, Sourdeval et al. (2018) state that "Strong variations are found in the tropics and along the ITCZ, where large cloud structures are convectively driven. High Ni values are noted in these regions during summer seasons, which strengthens their link to freezing events associated with deep convective structures. These values typically decrease by a factor of 2–3 during winter seasons". This is why in the first paragraph of Sect. 2.3, we wrote: "The seasonal dependence of N_i in Fig. 2 was also found by Sourdeval et al. (2018) at mid-to-high latitudes for winter vs. summer." For clarity, we added the following sentence: "*The regions with large N_i in the tropics in Sourdeval et al. (2018) are absent in Fig. 2 possibly because thick cirrus here have $\tau < \sim 3$ and the deep convective cores (where hom is often active) are not sampled*".

Hom fractions over India are smaller in summer (JJA) than in winter (DJF), which is again due to the larger extinction thresholds in summer than in winter (see panels a and g of Fig. S12).

I now understand the different arguments regarding the influence of updraft or INPs, this comes from the differing impressions from the figures in Sourdeval et al. (2018) and here. I think these differences should be discussed ...

New General 1: you argue here and later in the manuscript that strong orographic lifting occurs over the southern Andes Mountains during all seasons. This is an important point for interpreting the results, but is not clearly documented here. This would need to be addressed, or the conclusions based on this argument should be formulated more speculatively.

Thank you for your persistence. We investigated this more, and the article by Hoffmann et al. (2016) shows strong evidence for higher winds at cirrus levels during winter over the Southern Ocean. The paragraph in question has been rewritten as:
"*It can also be argued that the above seasonal differences in the hom fraction can be attributed to seasonal differences in vertical velocities at cirrus cloud levels. This appears likely over the Southern Ocean when one considers the stronger orographic gravity waves occurring there during the winter season (Jiang et al., 2002; Hoffmann et al., 2016). Moreover, Gryspeerdt et al. (2018) note that N_i is higher during winter in the midlatitude storm tracks, consistent with stronger jets. Nonetheless, two studies (Sporre et al., 2022; Lin et al., 2025) have documented large microphysical changes in cirrus clouds that were impacted by volcanic aerosol, which suggests that the observed seasonal differences can be attributed in part to seasonal differences in INP concentrations*."

New comments (to version with tracked changes):

New General 2: The number of Figures has been reduced, but the text now frequently refers to figures in the appendix or supplement. This often interrupts the flow of reading, and also refers to two different places. Where possible, I would reduce these references and also consider creating either an appendix or a supplement.

The purpose of the Supplement was generally to reinforce the findings presented in the main manuscript. This was done by showing maps for additional seasons, maps for in situ clouds only (N_i and D_e; contasting these with maps combining WBC and in situ cirrus), results for WBC clouds (for comparison with in situ results shown in the main manuscript), and figures over land (for comparison with similar figures over ocean). The main purpose of the Appendix was not to reinforce findings but to present new findings concerning the microphysical and macrophysical differences between in situ and WBC clouds. There has been considerable interest among cirrus cloud investigators regarding such differences, but we wanted to offer another perspective. Therefore, we placed our findings on this topic in an appendix so this would not distract the reader from the theme of our manuscript, while also serving the interest of the readership concerning these microphysical and macrophysical differences. To merge the supplement and appendix into a single document (either an appendix or supplement) would confuse the purposes for which they were created. We have reviewed many papers for ACP, and our supplement that has 12 figures is comparable to other supplements published in ACP (consider, for example, the recently published cirrus cloud paper by Ngo et al. (2025) that has 12 figures in its supplement).

We think that it is important to make the reader aware that more details or more figures are available in the manuscript itself or in the Supplement; hence references were provided accordingly.

We agree that the text in the last paragraph of Sect. 2.3 contained excessive references to Appendix A and specific panels in Appendix A, which interrupted the flow of reading unnecessarily. Such references have been reduced, and the modified section is given here: "*This suggests that the D_e differences seen between Fig. 3 (for thick WBC & in situ cirrus) and the thick in situ cirrus in Fig. S6 (residing at a lower T_r on average) are due to temperature differences. Temperature could also be a factor in explaining the lower N_i in Fig. 2 (for thick WBC & in situ cirrus) relative to the thick in situ clouds in Fig. S5 since the later tend to form at lower temperatures (Fig. A2) and hom depends strongly on temperature (e.g., Lamb and Verlinde, 2011). Figure A3 shows that both the geometric thickness and the equivalent thickness seen by IIR are on average greater for WBC clouds than for in situ cirrus clouds. This is the main reason that IWP and τ tend to be substantially greater for WBC clouds. Finally, IWC and N_i are both higher in WBC, in agreement with Luebke et al. (2016), only when clouds with τ < ~ 0.3 are included (blue curves)*."

New S 1: Line 922 ff: '*Hom may occur through (1) the freezing of haze solution droplets (Koop et al., 2000) and (2) the freezing of supercooled cloud droplets advected across the isotherm ~ 235 K (e.g., Rosenfeld and Woodley, 2000). If (1) and (2) are comparable in their frequency of occurrence, an abrupt increase in median Ni should be evident in Fig. 12 in the 231 – 235 K range (given typical non-convective cirrus updrafts of 10 to 30 cm s -1).* ***Since such an abrupt increase is not evident in Fig. 12,*** *it appears that (2) does not contribute significantly to Ni, even in the tropics.*'

As can be seen in Fig. 12, N_i increases significantly towards higher temperatures in the TRO from approximately T = 220 K, which is not visible in the MID and HIGH regions. I would attribute this to droplet freezing, which is also consistent, e.g. Costa et al. (2017) observe liquid droplets coexisting with ice particles at temperatures close to 235 K more frequently in TRO than in MID or HIGH.

Notes: Yes, in the tropics, the median N_i associated with hom cirrus does increase with increasing radiative temperature for T_r > 215 K, but it applies only to a very small percentage of samples (2 % at 231-235K and less than 7 % at 219-223 K)) as shown by the hom fraction curves in the tropics. These hom fraction curves tend to increase with increasing T_r, which cloud droplet freezing does not explain.

This paragraph has been rewritten (mostly at the end) as: "*Hom may occur through (1) the freezing of haze solution droplets (Koop et al., 2000) and (2) the freezing of supercooled cloud droplets advected across the isotherm ~ 235 K (e.g., Rosenfeld and Woodley, 2000). If (1) and (2) are comparable in their frequency of occurrence, an abrupt increase in median all cirrus N_i should be evident in Fig. 12 in the 231 – 235 K range (given typical non-convective cirrus updrafts of 10 to 30 cm s -1). Since such an abrupt increase is not evident in Fig. 12, it appears that (2) does not contribute significantly to N_i even in the tropics. This is consistent with Avery et al. (2020), Costa et al. (2017), and Mitchell and d'Entremont (2012) where it was shown that liquid water is rare in clouds over the range 239 – 235 K. This applies to both in situ cirrus and WBC clouds and is consistent with the definition of LOC in Luebke et al. (2016) where it is stated that LOC are restricted to pure ice clouds having T < 250 K (-23° C). When considering only hom cirrus N_i, an increase in hom N_i with T_r is seen in the tropics for T_r > 215 K, which might be attributed to (2), but it applies to only 2 % of the sampled cirrus at 231-235 K, as shown by the hom fraction curve.*"

New S2: Figure 16: Why is the hom fraction so low in the tropics - that's not very likely given the high updrafts there.? I think it's written somewhere... but readers here probably don't remember it.

The explanation is that we do not sample convective columns because we only sample cirrus having optical depth smaller than 3. See response to " **Old S 13, new comment"**.