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/added text in the revised manuscript.

We understand that reviewing this paper took a lot of time and effort, and we sincerely thank you for your comments that have improved this paper. Below are our responses to the general and specific comments:

General comments from Reviewer 1:

This manuscript analyzes cirrus cloud properties using a satellite retrieval product. It introduces a criterion for distinguishing between microphysical formation mechanisms of cirrus, often based on simple thermodynamic theory. Notably, the study not only estimates the fraction of homogeneously formed cirrus but also assesses their optical depth-weighted contribution, providing a more accurate estimate of their radiative and climatic relevance.

This manuscript has the potential to make an important contribution to our understanding of cirrus clouds. However, several issues undermine its impact, mainly related with the manuscript length, logic, and figure presentation.

Key comments:
1. Excessive manuscript length (particularly figure number)

The manuscript includes 27 figures, many with 8-12 subplots. This abundance, coupled with occasional topic detours, dilutes the key messages and diminishes the paper's relevance for the community. Reducing the number of figures (or at least figure panels) while focusing on the paper's key results is recommended. For instance, tropical data could often be excluded, as it is not the main focus of the study.

- For the maps (Figs. 2 – 7 in preprint), only DJF and JJA are now shown in the main paper. Maps for MAM and SON have been moved to the Supplement. Colormap concerns have been addressed in these figures. Figures 6 and 7 have been moved to the Supplement.
- Figures 8 – 11 have been condensed into a single figure (Fig. 6) that illustrates the impact of hom on N_i, IWC, & D_e for midlatitude winter only. Other latitude bands and seasons are similar (noted in text).

- Figures 12 & 14 have been replaced by Fig. 7 (comparing hom theory with N_i retrievals + variation in sample density), featuring winter only, and by separating in situ and warm base clouds. Land retrievals are featured in the Supplement (previous Fig. S2 replaced with new Fig. S7).
- Figure 13 is replaced by Fig. 8 (comparing hom theory with IWC and D_e retrievals + variation in sample density), featuring winter only. Land retrievals are featured in the Supplement (previous Fig. S1 replaced with new Fig, S8).
- Figure 21 (Fig. 15 in revised manuscript) has been changed to show only one temperature bin (i.e., 229 K) and the text now states that this same behavior is seen at the other temperatures:

"Because this was unexpected, we examine in Fig. 15 the dependence of N_i, D_e, and IWC on extinction for the four seasons for the temperature bin at 229 K. Other temperature intervals (having a mid-temperature of 233, 225, and 221 K) exhibited the same behavior."

- Figure 26 has been removed.

This reduces the number of figures in the main paper from 27 to 20 and restricts all seasonal comparisons to winter vs. summer or DJF vs. JJA.

To improve readability/simplify the manuscript, the authors could limit the main text's analysis to a cloud optical depth range of 0.3 to 3, moving additional analysis to an appendix.

We have simplified the manuscript, but to restrict the analysis to an optical depth (OD) range of 0.3 to 3 would reduce the fraction of cirrus clouds sampled to less than half (relative to the original study as shown in Fig. 1b), restricting this study to the properties of only relatively thick cirrus clouds. This would make the findings of this study much less meaningful since we find cirrus cloud properties change with extinction which is related to OD. Considerable time and effort were devoted in Part 1 to extending the sampling range down to an OD of 0.01 over oceans to make the hom fractions in Part 2 representative of all cirrus clouds over oceans.

2. Colormap use:
The manuscript frequently uses a broken colormap, which is (in my opinion) visually appealing but requires justification. If the switch between cold and warm colors aligns with a physically meaningful threshold, this should be explicitly stated. Otherwise, a perceptually uniform colormap should be used to ensure clarity and accessibility.

All figures now use a perceptually uniform colormap whenever relevant (i.e., shades of a single color or gradations between two colors) to ensure clarity and accessibility.

3. Seasonality figures**:**
The current plots make it challenging for readers to discern seasonality in cirrus cloud properties. Key figures could represent seasonality more intuitively, for example, by showing relative anomalies from annual means.

Seasonal differences are now easier to discern by using maps for only DJF and JJA and headings have been improved for more clarity. Maps for SON and MAM are now in the Supplement. This preserves the magnitude of a variable (important findings we believe) while also showing seasonal changes.

4. Key science issue: Homogeneous freezing of solution droplets vs. homogeneous freezing of cloud droplets at homogeneous freezing temperature of water**:**
The manuscript treats homogeneous ice nucleation in situ and homogeneous freezing of cloud droplets as equivalent, which they are not.

If homogeneous freezing of cloud droplets was important, we would see evidence of this in Fig. 18 (new Fig. 12) as a large peak of N_i in the 231 – 235 K temperature bin. In general, outside the tropics during non-summer months, updrafts in cirrus clouds are not strong enough to move this cloud droplet freezing zone to much lower temperatures. Perhaps this study advances our knowledge of cloud physics in this way. Text has been added to Sect. 3.4.1 to indicate that our results do not provide evidence that homogeneous freezing of cloud droplets is an important process:

…In this way, Fig. *12* shows how the T_r dependence of the hom fraction affects median N_i. *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 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).*

Figure *13* is similar to Fig. *12* except …

This distinction is critical due to its implications for cirrus cloud thinning. While in situ nucleated homogeneous clouds are promising targets for seeding, current thinning methods cannot modify clouds forming at water's freezing temperature. Although alternative modification strategies (e.g., convective invigoration or, possibly, the opposite, weakening; e.g. Varble et al., 2023, https://doi.org/10.5194/acp-23-13791-2023) could be developed, they fall outside the scope of this study.

As noted above, to a first approximation, homogeneous freezing within liquid origin cirrus clouds (LOC) can be viewed as proceeding through the freezing of solution droplets and not cloud droplets. Since the way we define LOC differs slightly from the literature (e.g., Luebke et al., 2016; Dekoutsidis et al., 2023), we now describe our LOC category as warm-base cirrus or WBC. As shown in Fig. 9 of Avery et al. (2020, AMT) and in Fig. 8 of Mitchell and d'Entremont (2012, AMT), clouds are generally glaciated for T < -34° C, except those having strong convection (Rosenfeld and Woodley, 2000, Nature). Moreover, LOC as defined in Luebke et al. (2016, ACP) are restricted to pure ice clouds at T < 250 K (thus excluding mixed-phase clouds, although LOC cloud properties may be influenced by "upstream" mixed-phase microphysics where T > 250 K). Thus, WBC should generally exhibit the solution droplet homogeneous freezing that characterizes in situ cirrus and LOC clouds. This is consistent with our finding that the fraction of hom cirrus is comparable for in situ cirrus and WBC clouds. Moreover, results pertaining to both in situ cirrus and WBC clouds appear to be relevant to cirrus cloud thinning or CCT. The text below has been added to Sect. 3.4.2 to indicate this:

For the hom fraction outside the tropics, the fractions of in situ and liquid origin cirrus are often comparable, although liquid origin dominates south of 60°S latitude during winter. *Although hom in WBC clouds (and thus liquid origin cirrus) has been predicted to occur mostly through the freezing of cloud droplets (Gasparini et al., 2018), evidence for this was not found in Fig. 12 (as discussed in Sect. 3.4.1). Thus, it appears that hom proceeds through the freezing of solution haze droplets for both hom cirrus categories in Fig. 16, making both in situ hom cirrus and hom WBC clouds susceptible to modification by increasing the concentration of INPs, which is the physical basis of CCT (discussed in Sect. 5).*

5. Additional key comment - data accessibility:
To enhance the utility of the dataset for the community, the authors could consider publishing their post-processed data in a user-friendly format, such as NetCDF following CF conventions. This would facilitate its use by climate modelers.

The CALIPSO project plans to include all retrievals described in Part 1 and used in Part 2 in the upcoming version of the IIR Level 2 products, which will be publicly released by the end of this year. The post-processed data used to create the figures shown in this paper are currently stored in ascii format and are available upon request (which was added to the "Data Availability" section).

Author responses to the specific comments from Reviewer 1:

I'm sorry if some comments repeat points already made in the key comments section.

Line 117 and Appendix A:

Why should such cases be rare? This is hardly justified. It certainly cannot be the case for cirrus originating from deep convection, where anvils spread far from the deep convective core, see Gasparini et al. 2018, Fig. 5 (10.1175/JCLI-D-16-0608.1). In any case, it's hard to discriminate the origin with only snapshot data, without any means of tracking the cloud evolution.
There are more studies that discriminate between liquid and in situ origin cirrus that could be mentioned, e.g. Wernli et al. 2016 (10.1002/2016GL068922), which uses a large statistical sample of clouds (relying on the imperfect reanalysis data, but at least with good statistics).

We agree that evidence is lacking to justify this sentence (that misclassified in situ cirrus probably make a minor contribution to our in situ cirrus category). Following Gasparini et al. (2018), we retain these two cirrus categories but redefine the LOC category as warm-base cirrus clouds or WBC clouds to acknowledge the difference between LOC and WBC. This paragraph in Sect. 2.1 has been rewritten, describing these two categories (in situ and WBC) accordingly:
In this work, we take advantage of the improved ice/water phase assignment in the Version 4 CALIOP products to also include cirrus clouds with T_{base} warmer than 235 K (and T_r colder than 235 K), hereafter called *warm base cirrus clouds or WBC clouds. Even though these WBC clouds are identified as high confidence ice cloud layers by CALIOP, this assessment*

does not rule out the possibility of liquid droplets in the lower part of the layer. This classification method is an attempt to qualitatively contrast the properties of in situ and liquid origin cirrus (LOC) clouds, using WBC as a proxy for LOC clouds. A similar approach was used in Gasparini et al. (2018). This approximation may underestimate LOC clouds (overestimating in situ cirrus) since cloud condensate from below the 235 K isotherm may be advected across this isotherm upwind of the CALIOP nadir view when there is no cloud at nadir below this isotherm. In this case the cloud would be mistakenly classified as in situ cirrus. On the other hand, the modeling study by Wernli et al. (2016) estimates that roughly 50 % of in situ cirrus clouds occur on top of LOC, indicating a strong dynamical linkage. Relative to an air parcel back trajectory analysis as used in Wernli et al. (2016) and other LOC studies cited below, our approach should underestimate in situ cirrus if there is no clear layer separating in situ from WBC clouds. This classification scheme is evaluated in Appendix A in Figs. A2 and A3. Figure A2 shows the dependence of the in situ fraction on temperature, where this fraction is ~ 0.5 (indicating a transition from in situ to WBC) at about 227 K over oceans when all clouds are considered. In the LOC studies by Dekoutsidis et al. (2023) and Luebke et al. (2016), which are both based on the same field campaign, this transition occurs around 221 K and between 218 – 222 K, respectively. This suggests that the WBC approximation overestimates the in situ fraction somewhat (shifting the transition temperature by ~ 6 K relative to these measurement-based studies), but that WBC may still serve as a qualitative proxy for LOC.

Line 175, but related to many of the figures:

Wouldn't it be enough to show only DJF and JJA in the main manuscript and the other seasons in the supplement?

Yes, we agree and that has now been done.

Line 177: “Since this sampling criteria appears to render radiative properties representative of all cirrus clouds, the cloud property values predicted in GCMs for cirrus clouds should be similar to those shown in these figures.”

Should be predicted by the GCM, but only in the sampled range of cloud optical depths. The clouds at COD<0.3 are very common, and while not as radiatively important, will contribute significantly to the mean ice cloud properties.

This sentence has been changed to: "*From Sect. 2.2, this sampling criteria appears to provide cloud property values of cirrus clouds whose radiative properties are representative of all cirrus clouds*".

Line 188: "The correspondence between T_r (Fig. 5) and IWC (Fig. 4) is clearly seen. This is an expected result predicted by the Clausius Clapeyron equation."

But also the agreement with ICNC
Figures 2-7:

1. If one does not pay attention and focus on some features, all subpanels generally look the same.
2. In general, I like the choice of the colormap. However, it is a discontinuous colormap with a very sharp transition. This can only be used if there's a reason for such a choice. Otherwise, the reader will see patterns that aren't real, but just a result of the sharp colormap discontinuity.

The color bars were changed to show gradations between one or two colors without discontinuities or artificial sharp transitions. For seasons, only DJF and JJA are shown.

Section 3.1 is, in my opinion, not important to the main story of the paper. It could be moved to the Appendix, along with the corresponding figures (or at least parts of some of the figures).

As mentioned in our reply under General Comments, Figures 8 – 11 have been condensed into a single figure (Fig. 6) that illustrates the impact of optical depth on N_i, IWC, & D_e for midlatitude winter only. The main point is to show that hom mostly affects N_i and IWC, with some impact on D_e. Since this finding provides the rationale for Sect. 3.2 (which uses extinction, proportional to IWC/D_e, to separate het and hom cirrus; a central result of this study), Section 3.1 still remains, although the number of figures has been greatly reduced.

Section 3.2/Figure 12:
If the relevant threshold is 30 ICNC/liter, then the discontinuity in the color map should be set at that level.

There is no threshold mentioned at 30 ICNC (L^{-1}); the text is merely describing N_i regions strongly dominated by het and hom nucleation, and how a N_i gradient exists between these two regions. The colormap for Fig. 12 (now Fig. 7) now follows the new convention shown in

Fig. 2 for N_i (showing gradations between two colors without discontinuities or artificial sharp transitions).

Section 3.2:
The use of extinction coefficient is not motivated

The purpose of Sect. 3.1 was to explain why the extinction coefficient was used to separate predominantly het formed cirrus from predominately hom formed cirrus (i.e., het and hom cirrus). Sect. 3.1 has been modified to make this point more clearly. The opening sentence for Sect. 3.1 is now this:

The main purpose of this section is to justify the use of the extinction coefficient α_{ext} as a means of separating cirrus clouds formed primarily through het from those formed primarily through hom.

And the last sentence of Sect. 3.1 now reads:
This suggests that the ratio IWC/D_e and therefore the extinction coefficient (α_{ext} = 3 IWC/(ρ_i D_e, where ρ_i = bulk density of ice = 0.917 g cm^{-3}) may be sensitive to hom.

Line 320:
How is this equation derived? What is its source?

Lines 311-212 in the EGUsphere preprint state that “two formulations of the Clausius-Clapeyron equation and the supersaturation equation for hom (Lamb and Verlinde, 2011) were used”, and lines 318-319 state: “Finally, the supersaturation threshold where hom occurs, S_i^f, is predicted by”, followed by the equation in question. This equation for S_i^f comes from the cloud physics textbook by Lamb and Verlinde (2011) and is easily found in their chapter on nucleation (Eq. 7.33). To make things clearer, lines 311-312 are modified to read:

To explore this further, two formulations of the Clausius-Clapeyron equation and the supersaturation threshold equation for hom, first derived in Lamb and Verlinde (2011), were used.

Line 347:
Does "theoretical method" refer to the formula in Verlinde? I'm not sure where it comes from, but it doesn't seem like a theoretical relationship, more like an empirical fit to data?

Yes, “theoretical method” refers to Eq. 7.33 in Lamb and Verlinde (2011). The lead author emailed Dr. Lamb, who confirmed that he built upon the findings of Koop (2000, Nature) to present those findings in a simpler theoretical context that is more convenient to use. Note that Koop et al. (2000) “present a thermodynamic theory for homogeneous ice nucleation” and that Lamb and Verlinde are using the water activity offset of 0.305 found in Fig. 1 of Koop et al. (2000) that is based on laboratory data. Koop et al. (2000) does not give the S_i^f formula found in Lamb and Verlinde (2011, Eq. 7.33) which includes this water activity offset. Therefore, this equation is an original contribution from Lamb and Verlinde (2011). Line 347 has been modified to read:

There is close agreement between the theoretical *(i.e., Eq. 3)* and experimental methods*, where both methods address the homogeneous freezing of solution haze droplets based on the activity of water in the solution droplet (Koop et al., 2000) and do not address the homogeneous freezing of activated cloud droplets*.

Lines 348-350:
The authors here assume that homogeneous freezing of cloud droplets is the dominant source of ice in convective plumes. It's hard to know how important it really is, but given that most convective clouds freeze at T of about -30°C, freezing of cloud droplets in the mixed-phase regime should be very important. There are quite a few references on this, although it's harder to find good information on deep convective clouds (e.g. Coopmann et al., 2020 https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019JD032146; but at least in the tropics, most mixed-phase clouds are of convective origin).
Moreover, while associated with a large uncertainty, secondary ice production is thought to contribute substantially to ICNC in deep convective clouds, see e.g. Hu et al., 2021 (https://doi.org/10.1175/JAS-D-21-0015.1)

Lines 347-350 state: In the tropics the margin between the triangles and the “no data” region is wider. This may be due to the deep convective origin of most tropical cirrus, with convective plumes overshooting the temperature level where hom is first activated, carrying ice to lower temperatures.

There is no mention of homogeneous freezing of cloud droplets here, which occurs when activated, spontaneously growing cloud droplets are advected across the 235 K isotherm. Rather, the process addressed here and in Koop et al. (2000) is the homogeneous freezing of solution haze droplets based on the activity of water in the solution droplet (henceforth hom). This is what the Lamb-Verlinde and Schneider et al. relationships describe. New text has been added, associated with line 347 above (in italics), to clarify this issue.

We agree that ice multiplication processes in convective clouds, such as those described in Lawson et al. (2015, JAS; 2017, JAS), can produce Ni levels similar to hom conditions, but this generally occurs when T < -30° C (e.g., Hu et al., 2021) as the reviewer indicates. It seems very speculative to invoke ice multiplication and long-range vertical transport to explain CALIPSO samples above the theoretical/experimental hom activation regime (i.e., the triangles and squares) in Figs. 13 and 14 of the preprint (Figs. 7 and 8 in the revised manuscript) when hom is producing relatively high IWC and Ni in closer T_r proximity to these CALIPSO samples.

Figure 14:
Not sure we need panels g-l, since they show the same as panels a-f.

Agreed. We have removed panels g-l from Fig. 14. Fig, 12 and Fig. 14 are now combined into the new Fig. 7.

Line 400: "Note that this analysis is based on clouds having τ <~3 and thus does not consider deep convective columns where τ > 3 and hom is probably quite active."

We cannot know how active hom is, and in any case it would be homogeneous freezing of cloud droplets.

As indicated above, the process described is homogeneous freezing of solution haze droplets, not cloud droplets, as indicated in the additional new text associated with line 347. Nonetheless, line 400 has been clarified to read:

Note that this analysis is based on cirrus clouds having τ <~3 and thus does not consider *thick cirrus clouds originating from deep convection* where τ > 3 and hom is probably *active in the strong updrafts.*

Figures 15 and 16:
Is there a physical meaning to the colormap boundary at 211 K?

There was no physical meaning to the colormap boundary at 211 K. The colormap was modified to have the darkest colors corresponding to the coldest and warmest bins and lighter colors around the transition from cold to warm colors to attenuate the color boundary contrast. In addition, because there is often more coherency in the N_i – and D_e – extinction relationships for T > 215 K, which are referred to as "warm-colored" data in the manuscript, we modified to colormap to have only warm colors for T > 215 K. The color change makes this easier to recognize, but we do not advocate mechanistic reasons for this coherency change.

Lines 440-441:
What if updrafts are the key difference between land and ocean?

Good question. Higher updrafts are associated with higher IWC (Hu et al., 2021; Mitchell, 1988, JAS) and higher extinction in Figs. 15 & 16 (now Figs. 9 and 10) is associated with higher IWCs, noting that $\alpha_{ext} = 3\ IWC/(\rho_i\ D_e)$. Thus, updrafts should tend to increase with extinction in Figs. 15 & 16, especially when D_e is increasing with extinction. In this way, updrafts are implicit here with relatively weak updrafts associated with the apparent Twomey effect. New text has been added:

While higher updrafts over land could also enhance INP 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 α_{ext} < 0.3 km^{-1} where updrafts are expected to be relatively weak over both ocean and land.

Figure 17:

I see no difference between the dashed and solid lines, they seem to be parallel. Therefore, one of them could be removed from the plot for clarity.

Having a step increment of 0.1 (rather than 0.2 as suggested by the reviewer) provides more precise knowledge, and for some this may be important, so we have retained this information.

Figure 20:
Why is there a seasonality in the parameterization for the Northern Hemisphere (discontinuously moving from 30° to 60°N) but not for the Southern Hemisphere?

There is a seasonality for both hemispheres (i.e., a discontinuity moving from 30° to 60° latitude) as described in the text (lines 522-525 and lines 550-564 of preprint). This appears to be due to changes in mineral dust concentration as discussed in this section.

Why is the word "corrected" in the title?

This is to refer to the correction factor required over land (see Eq. 6). We have removed it.

Figure 21:
Why do we need 4 temperature bins when the behavior seems to be pretty much the same in each of the bins (also, we are at figure 21 already, and it's starting to get harder to keep the focus).

We used four temperature bins in Fig. 21 (now Fig. 15) to show that we see the same behavior in each instance. We now show only one temperature bin for illustration (at 229 K) and say that the same behavior is seen at the other temperatures as described in the new text below:

Because this was unexpected, we examine in Fig. *15* the dependence of N_i, D_e, and IWC on extinction for the four seasons *for the temperature bin at 229 K. Other temperature intervals (having a mid-temperature of 233, 225, and 221 K) exhibited the same behavior.*

Figure 22 is, in my opinion, the key figure of the manuscript, but unfortunately it gets lost a bit due to the large amount of information presented.
Another comment, similar to the one above: I don't think it's fair to lump together in situ cirrus and cirrus of liquid origin. Only in situ cirrus can be modified. So the readers might want to have numbers of hom vs. het for in situ cirrus only.

The reviewer brings up a common misconception among cloud physicists that this study addresses. The theoretical and AIDA chamber results presented in Figs. 7 and 8 (revised paper; triangles and squares) are based on the homogeneous freezing of solution haze droplets and not cloud droplets, and since they coincide with the highest retrieved N_i and IWC (for a given temperature level), this suggests that hom in natural cirrus clouds (both in situ and WB cirrus) is predominantly from the homogeneous freezing of solution haze droplets. As described above, we now discuss that mixed phase clouds are predominantly all-ice for T < -34° C (< 239 K), implying that even near 235 K (where cloud droplets would freeze if they existed) homogeneous freezing proceeds primarily through the freezing of solution haze droplets. In the revised paper we have now discussed that, if cloud droplet freezing was important, an abrupt increase in median N_i should be evident in Fig. 12 (revised paper) near 235 K, but this is not evident. Using WB cirrus as a proxy for liquid origin cirrus, it is fair to include both in situ cirrus and WB cirrus in Fig. 22 (Fig. 16 in revised version) since hom primarily proceeds through the same mechanism in each cloud type. Moreover, both in situ cirrus and WB cirrus can be modified via CCT for this same reason. New text has been added to indicate this:

Although hom in WBC clouds (and thus liquid origin cirrus) has been predicted to occur mostly through the freezing of cloud droplets (Gasparini et al., 2018), evidence for this was not found in Fig. 12 (as discussed in Sect. 3.4.1). Thus, it appears that hom proceeds through the freezing of solution haze droplets for both hom cirrus categories in Fig. 16, making both in situ hom cirrus and hom WBC clouds susceptible to modification by increasing the concentration of INPs, which is the physical basis of CCT (discussed in Sect. 5).

Section 4:
This is a valuable section, but there's a lot of speculation. I am very intrigued by the results presented in Figure 24. These should be verified in the future with other observational datasets and model studies to confirm or reject the proposed interpretation of cirrus cloud properties relative to updraft.
To increase the relevance of this section, the authors could compare their hypotheses with parcel model studies of cirrus, e.g. work by Bernd Kärcher, if possible.

We looked at some of the recent papers by Kärcher et al. but did not find any compelling reason to cite this work (i.e., it did not add significant value to the paper in our view).

Figure 25:
I assume that the INP number for Figure 25 and its description should be fixed. This should be stated explicitly.
Does the model in Figure 25 hold for all cirrus or only for in situ cirrus?
A caveat to this interpretation is that cirrus data could also be explained as cirrus at different stages of cloud evolution.

As stated previously, our results (and thus the model in Fig. 25 of the preprint) pertain to cirrus formed by the freezing of solution haze droplets for both in situ cirrus and WBC clouds. This figure (new Fig. 18) is now shown at mid-latitude only by separating in situ cirrus and WBC clouds, showing that the correspondence between T_r-T_{top} and maximum D_e is more evident in WBC than in situ cirrus clouds which are geometrically thinner.

Lines 645-6 (in the preprint) have been modified to read:
This proposed explanation of Figs. 17 and 18 is summarized in the schematic in Fig. 19 for the warmest WBC layer, presenting a conceptual model of how cirrus cloud thickness might evolve with increasing cloud updraft and IWC for a fixed INP number concentration.

Section 5:
It seems a bit odd to have a separate section mentioning the otherwise very relevant study by Froyd et al., 2022.

Journal articles often have a section titled something like "Comparison with other studies", and the ACP abstract format specifically asks authors to describe how their work differs from previous work on the topic addressed. Nevertheless, we understand that having this short section dedicated to only one study is odd. The comparison with the work by Froyd et al. (2022) has been shortened and included in Sect. 3.4.1 and Sect. 5 of the pre-print has been removed.

I don't think the Froyd et al., 2022 study presented any ice residual measurements (unlike e.g. Cziczo et al., 2013).

Yes, that is correct. This sentence was removed from the shortened text now in Sect. 3.4.1.

Figures 26 and 27:
Figures 26 and 27 are very useful for the purpose of scientific presentations, but lack some more content to qualify for a proper scientific publication. For example, can we be sure that the clouds shown are at temperatures below the homogeneous freezing temperature of water?
In the best case, one could find a photo of a cirrus cloud with a coincident CALIPSO overpass and the analysis as done for the rest of the paper.

Figures 26 and 27 in the preprint exhibit photos of het and hom cirrus clouds. Since most people are familiar with common het cirrus clouds, Figure 26 has been removed but Fig. 27 (new Fig. 20) showing an example of hom cirrus clouds has been retained. Many scientists consider lenticular or wave cirrus clouds to be very limited in areal coverage (and hence not radiatively important; see Krämer et al., 2016, ACP), but observations by the lead author who lives immediately downwind of the Sierra Nevada Mountain range have consistently shown that this is true only for the relatively low wave cirrus. At higher levels, these wave cirrus exhibit extensive areal coverage as shown in Fig. 27 of the preprint and are thus relevant to radiation (which is why Fig. 27 is retained). These wave cirrus clouds induced by orographic gravity waves (OGWs) have relatively high updrafts and thus are more likely to be hom cirrus clouds (Joos et al., 2008, JGR; Joos et al., 2014, ACP; Barahona et al., 2017, Nature; M2024). They also tend to exhibit higher cloud fractions (see Fig. 4 in Matus and L'Ecuyer, 2017, JGR). These OGW cirrus are characterized by relatively high Ni and, perhaps due to the oscillation of the OGW, are evident far downwind from mountain ranges in North America, Patagonia, and Antarctica during winter (Fig. 2 and S5). The paragraph describing Fig. 27 in the preprint (new Fig. 20) has been revised and moved to the new Sect. 6 (Sect. 7 of the pre-print) after the discussion regarding the importance of including OGWs for ice nucleation in the models:

An example of what OGW cirrus clouds often look like is given in Fig. 20, which are optically thicker than het cirrus clouds. Cirrus clouds induced by OGWs, often called wave cirrus, have relatively high updrafts and thus are more likely to be hom cirrus clouds (Barahona and Nenes, 2008; Joos et al., 2008; Joos et al., 2014; Barahona et al., 2017; Lyu et al., 2023; *M2018). They also tend to exhibit higher cloud fractions (see Fig. 4 in Matus and L'Ecuyer, 2017). These OGW cirrus are characterized by relatively high N_i (M2018, Gryspeerdt et al., 2018) and, perhaps due to the oscillation of the OGW, are evident far downwind from mountain ranges in North America, Patagonia, and Antarctica during winter (Figs. 2 and S5). Note that hom cirrus are not restricted to OGW cirrus and may form under other conditions having relatively high updrafts and/or low INP concentrations.*

Lines 730-731:
Or, more physically, increase the vertical resolution of the model.
Or use some kind of subgrid cloud fraction in the vertical (similar to what is done in the horizontal dimension in coarse GCMs).
In any case, the pre-existing ice formulation tends to be less important when using high resolution models, and may become less important in future cirrus modeling studies.

We agree with these comments and this sentence has been modified as follows:

To address this issue, the model's vertical resolution could be increased, or $q_{i,pre}$ could be attenuated by a factor that best represents q_i near cloud top in the "nucleation zone".

Lines 754-755:
The study by Froyd et al. 2022 used only an idealized model setup (based on dust measurements, but the trajectories were based on reanalysis data).

This sentence has been modified as follows:

From a global observational purview, this was done for the first time by Froyd et al. (2022) who used global measurements of dust concentration from aircraft in the UT to initialize a detailed cirrus cloud formation model that used reanalysis data in the dust trajectory simulations.