Author responses to Reviewer 2

Format: The reviewers' comments are in black 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 2:

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 contains a new global climatology of cirrus clouds derived from satellite observations. It is an impressive work with very extensive analyses of various properties of cirrus, as cloud ice particle number concentration (N_i), effective diameter (D_e), ice water content (IWC), shortwave extinction coefficient (α_{ext}), optical depth (τ), and cloud radiative temperature. The study includes innovative data analyses that lead to new perspectives and a deeper understanding of cirrus clouds. In particular, the observations are analyzed to determine whether the cirrus formed homogeneously or heterogeneously. Further, the fraction of hom-affected cirrus clouds is determined and τ distributions are used to establish a proxy for cloud net radiative effect (CRE) of the hom affected cirrus. Finally, a conceptual model of cirrus cloud characterization is proposed. Altogether, this study has the potential to become a new standard work on cirrus properties.

Unfortunately, however, I have some concerns, which I will list in the following. I know that the authors are experienced scientists with many publications and therefore write their articles the way they like it best. Nevertheless, I would like to add some comments, because I feel that otherwise the extensive and thorough study may not get the attention it deserves.

(G 1) It took me quite a while to work through the long, sometimes complicated text and the equally complicated figures. To my opinion, the interesting, but complex results could be presented more simply and shorter to make them easier for the reader to understand. Otherwise, I fear readers will be discouraged from reading the article.

So, overall, I think it might be good to consider shortening the main part of the paper and only showing the most important figures in that part. Everything else could be moved to the Appendix or Supplementary Material.

Reviewer 1 made a similar recommendation, along with specific instructions. We have followed the advice from Reviewer 1 and Reviewer 2 to simplify this paper, reducing the number of figures in the main part of the paper from 27 to 20, greatly reducing the number of panels is some of the figures, and changing the labeling convention in some figures.

(G 2) I have some suggestions for simplifications - but not for the text, for that I can only ask the authors to go through the manuscript again and simplify and shorten the descriptions.

For example, I recommend

(a) to introduce a small table with abbreviations, which contains for example:

T_r: cloud layer radiative temperature, approximately in the middle between T_{top} and T_{bottom}
IAB: CALIOP 532 nm layer integrated attenuated backscatter

IAB < 0.01 sr -1 = ~ 0.01 < τ < ~ 0.3 optically thinner cirrus – thin - subvisible cirrus*

IAB > 0.01 sr -1 = ~ 0.3 < τ < ~ 3 optically thicker cirrus – opaque cirrus

* subvisible cirrus (τ < 0.03), thin cirrus (0.03 < τ < 0.3)

(b) to use either the τ ranges throughout the manuscript to identify the cirrus type, or (I think even better) the name (subvisible cirrus, thin cirrus, opaque cirrus), IAB doesn't give an intuitive impression on the type.

We appreciate the need to provide a more intuitive impression for cloud type, and a new table (Table 1) has been added in Sect. 2.1 which contains the information mentioned above by Reviewer 2. These descriptions of cirrus clouds are then used throughout the article (i.e., thin, thick, and all cirrus clouds).

(c) to simplify the figures:

I strongly recommend revising all figures so that the recurring headings above each panel be incorporated into a general figure title, so that only the specific information appear above the panels (in the current version it is hard to find out the differences between the panels). Further, I would also include information that is now somehow hidden in figure captions in the Figure title.

As an example of the simplification of the figures here **Figure 18 (**I modified the figure for my own understanding): <not shown>

This has now been done for all of the applicable figures.

(G 3) Retrieval of liquid origin cirrus

Line 110 ff: … *cirrus clouds with T_{base} warmer than 235 K (and T_r colder than 235K), hereafter called liquid origin cirrus. …*

This method is an approximation that may underestimate liquid origin cirrus clouds

somewhat (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.'
This method sorts not only liquid origin as in-situ origin cirrus, but likely also in-situ origin as liquid origin, as explained in the following: Warm conveyer belts (but also convective systems) consist from bottom to top of layers of liquid, mixed-phase and cirrus clouds. The mixed-phase clouds appear in the cirrus region as liquid origin clouds, but above these, in-situ cirrus usually also form due to the lifting of the air masses. An example is shown by Luebke et al. (2016) (see Figure below, top panel). The vertical structure of liquid origin and in-situ origin cirrus is clearly recognizable.

If these clouds were classified as described in this paper (i.e. if the clouds reach down to temperatures warmer than -38C they are liquid origin cirrus), the whole in-situ origin cirrus umbrella would be misclassified as liquid origin cirrus.

I would recommend doing some case studies to test the classification. A trajectory analysis, as done for example by Luebke et al. (2016), would be best suited for this. This is the most reliable method to classify cirrus of in-situ and liquid origin. I think it is crucial to check the classification method, as all results on in-situ origin and liquid origin cirrus depend on the correctness of the sorting - and my concern is that many of the in-situ cirrus at the top of WCBs, MCS or convective cells will be classified as liquid origin.

We have changed our cirrus cloud classification wording to describe only what the CALIOP lidar measures, with clouds having a radiative temperature $T_r \leq 235$ K and a cloud base temperature $T_{base} > 235$ K classified as "warm-base cirrus clouds", or WBC clouds. The "liquid origin cirrus" classification is no longer used. We provide evidence showing that WBC and liquid origin cirrus are not equivalent but also provide evidence showing that WBC may be used as an approximation or proxy for liquid origin cirrus as done in Gasparini et al. (2018, J. Climate).

If there is an in situ origin cirrus umbrella with a clear layer between this "umbrella" and a WBC cloud below, then the lidar will likely detect two layers and the scene will not be included in our sampling because only single layer clouds are sampled. But if an in situ cirrus cloud and a WBC cloud somehow become connected and "bridge" so that the lidar detects only one layer, then this method will classify this vertically continuous cloud layer as a WBC cloud. Our methodology is not capable of determining such bridging phenomenon.

(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* α_{ext}*) where* $N_i < 30\ L^{-1}$*, apparently corresponding to het only. To the right of this region is a gradient of increasing* N_i*, culminating in values of* $N_i > 2000\ L^{-1}$*. 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 Ni 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.

The region with low α_{ext} and N_i 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 N_i (and thus α_{ex}) 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 α_{ext} and N_i are in a subsaturated environment** is also indicated by the decreasing D_e to the left of D_{max} (Figures 15 and **16), because under this condition, the thinner the cirrus clouds and the lower the N_i,** 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 D_e, but an increase, maybe with a change of the slope during the transition from one to the other regime.

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.

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.

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 N_i 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 (Krämer et al., 2016), hom tends to produce much higher N_i (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.

Specific comments:

(S 1) Line 35f: *… liquid origin cirrus associated with cloudy air advected from lower levels (T>235 K) that often contains liquid cloud droplets.*

I don't think cirrus of liquid origin often contains liquid droplets - they are usually completely glaciated by the Wegener-Bergeron-Findeisen process when they enter the cirrus temperature regime (< -38C; see Costa et al., 2017, their Figure 15). Only at high vertical velocities (as in convective cirrus), or in the absence of INPs (e.g. in the Arctic), liquid droplets can rise to temperatures as low as -38 C, where they freeze homogeneously.

Completely agree. Earlier text has been removed and replaced with new text as follows: "*with liquid origin cirrus associated with cloudy air advected from lower levels ($T > 235$ K) that is typically near ice saturation and completely glaciated by the Wegener-Bergeron-Findeisen process when this air enters the cirrus temperature regime ($T \leq 235$ K; Luebke et al., 2016; Costa et al., 2017; Avery et al., 2020; Mitchell and d'Entremont, 2012). However, at high vertical velocities liquid cloud droplets may be advected into the cirrus regime where they immediately freeze homogeneously (e.g., Rosenfeld and Woodley, 2000).*"

(S 2) Line 51f: *However, hom resulting from cloud droplet freezing dominated ice production in the lower part of cirrus clouds at all latitudes.*

This is not consistent with the measurements of Costa et al. (2017), see point (2).

Yes, we agree this is at variance with observations, but we are just reporting what other studies have done, and the observations cited earlier should alert the reader when discrepancies between predicted and observed features occur. We challenge some of these modeling results in the next paragraph.

(S 3) All kinds of studies are cited in the introduction, but not with a specific focus. Then it is said - even without focus - what is in the paper.

A new sentence has been added to the beginning of the 3rd paragraph of the Introduction, stating: "*Since this study estimates the fraction of cirrus clouds strongly affected by hom, we briefly review similar estimates from modelling and observational studies here*." This provides the rationale for what follows, where various modelling and observational studies are cited and related to the relative roles of het and hom in cirrus formation.

(S 4) Line 130-132: *When only clouds with τ >~0.3 are sampled over oceans* (solid red lines)*, liquid origin cirrus clouds prevail at mid- and high latitude (60% and 70%, respectively), but not in the tropics (32%).*

I think these numbers 60, 70 and 32% can be derived from those in the respective panels by taking the difference to 1, right? That is not easy to understand - please mention it in the text or write both numbers (for in-situ and liquid origin) in the panels.
When all clouds are considered (solid blue lines)*, the percentage of in situ cirrus increases by 18 to 25 % and they always prevail.*
I can't find these numbers in the panels …

Regarding both of these two (S4) comments, this paragraph has been rewritten in response to other review comments, and the relevant (S4) text has been removed. However, the end of the second paragraph in Appendix A now reads "*From Fig. A2, the overall percentage of in situ cirrus for all sampled cirrus over oceans ranges from 86% in the tropics, to 62% in the midlatitudes, and 55% in the high latitudes. These percentages subtracted from 100 yield the percentage of WBC clouds*."

(S 5) Line 132ff: *For these blue curves, it is seen that the liquid origin cirrus prevail at Tr larger than about 227 K, which is ~ 6 K higher than shown in Luebke et al., 2016 (their Fig. 13) and Dekoutsidis et al., 2023 (their Fig. 4). …*
Note that the analysis of Luebke et al. (2016) and Dekoutsidis et al. (2023) are based on the same field experiment (ML-Cirrus) and represent only the meteorological conditions that prevailed during that time. I would recommend to compare the in-situ / liquid origin fractions with the analysis of Wernli et al. (2016), which covers 10 years of ERA5 data.

We have modified adjacent text to indicate both studies are based on the same field campaign: "*In 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.*" The study by Wernli et al. (2016) is based solely on model-generated ERA-Interim ice water content (IWC), liquid water content (LWC), temperature, and wind fields, and the temperature where in situ and liquid origin ice clouds are about equally frequent (based on their Fig. S4) is -34°C (239 K), which is much warmer than the cloud-type transition temperature found in this study and in the two ML-Cirrus studies. The authors admit that IWCs and LWCs are "produced by rather simplistic cloud physics" and that "The thermodynamic cloud phase in ERA-40 and ERA-Interim is parameterized simply as a function of temperature, with pure ice clouds below -23°C and mixed-phase clouds between -23°C and 0°C." These and other limitations preclude the use of this modeling study in our study for comparing in-situ / LOC (or WBC) fractions and transition temperatures.

(S 6) Line 175: *Global maps for each season are shown for median Ni, De, IWC, and Tr using the cloud sampling criteria described in section 2.1 and IAB ≥ 0.01 sr-1 (i.e., ~ 0.3 < τ < ~ 3,* thick cirrus*)* …

Please introduce the cirrus category‚ thick cirrus', also in the Figure captions (or even more simply as the title of the figures: ‚Median Ni / De / IWC / Tr , Tr < 235 K, thick cirrus (~ 0.3 < τ < ~ 3)', then only the time of year appears above the individual panels.

The sentence indicated above has been modified as: "*Global maps … are shown for median N_i, D_e, IWC, and cirrus cloud T_r for thick cirrus (i.e., ~ 0.3 < τ < ~ 3) in Figs. 2, 3, 4, and 5, respectively.*"
The figure titles and captions have been modified as per the suggestions here.

(S 7) Line 193ff (Figure A3):
*** panel b:** I wonder why the in-situ D_e is of comparable size to that of liquid origin?

I think it should be smaller, because in-situ ice particles cannot grow as large as ice particles of liquid origin.

This is observed at a given temperature perhaps because hom activity is comparable for in situ and WBC clouds, as discussed above. In general, liquid origin ice particles will be larger than in situ ice particles since the latter form at lower temperatures as shown in Fig. A2.

*** panel e:** I wonder why the in-situ N_i of thick cirrus (red curves) increases at warm T - could this be a misclassification?
Our results show that hom is more active in thick cirrus and at relatively warm cirrus temperatures for NH midlatitudes over oceans during DJF (corresponding to Fig. 12b and Fig. A3). Panel e in Fig. A3 is consistent with Fig. 12b. But this hom activity varies with season and location, as shown in Fig. 12. New text has been added at the end of Appendix A: "*As shown in Fig. 12, the temperature dependence of the fraction of hom cirrus varies with latitude and season, indicating that results in Fig. A3 may vary somewhat for different latitude zones and seasons*."

In addition, a misclassification does not appear likely due to the consistency of in situ optically thick cirrus N_i in Fig. A3 panel e with other Fig. A3 panels and other figures. For example, the geometric thickness (Fig. A3d) of these optically thick in-situ cirrus decrease as T increases and approaches 235 K while their optical depth is about constant (see Fig. A3g). The increase of N_i appears consistent with this increase in extinction. Extinction is proportional to IWC/D_e, and this in situ D_e is quasi-constant at these warmer temperatures while IWC increases. As shown in Fig. 6 of Sect. 3.1, N_i and IWC tend to track each other in optically thick cirrus and this section also suggests that these cirrus tend to be relatively thin cirrus having relatively high N_i.

*** panel f and h** (IWC and IWP): I wonder why the in-situ IWC and IWP (red curves) is of comparable size to the liquid origin ones? I think they should be smaller. Misclassification?

When all cirrus clouds are considered (blue curves), indeed this is true (i.e., in situ IWC and IWP are lower than corresponding WBC values). But orographic gravity wave (OGW) cirrus may contribute substantially to the optically thick in situ cirrus (red curves), possibly resulting in higher IWCs (for a given Tr) relative to optically thick WBC clouds. And OGW in situ cirrus may tend to be geometrically thicker than typical in situ cirrus (see Fig. A3d), perhaps explaining (along with higher IWCs) why optically thick in situ cirrus have only slightly lower IWPs than optically thick WBC. The difference might also be attenuated because the clouds of optical depth larger than 3 are not sampled.

(S 8) Line 237f: *... two different τ categories: ~ 0.01 < τ < ~ 0.3 (IAB < 0.01 sr-1) and ~0.3< τ <~3 (IAB>0.01sr-1); henceforth categories 1 and 2.*
Instead of categories 1 and 2, you could say 'thin cirrus' and ‚thick cirrus', which is more specific and informative.

Agreed; text has been changed here as suggested above: "*Figures 6 provides a means of investigating this question, evaluating N_i, IWC, and D_e for optically thin (~ 0.01 < τ < ~ 0.3) and thick (~ 0.3 < τ < ~ 3) cirrus clouds.* Only retrievals over ocean are considered since variable land emissivities preclude retrievals over land *for the thin clouds* ....
"*Figure 6a shows that for thick cirrus, the highest N_i (resulting from hom due to its magnitude) is found in relatively geometrically thin clouds (consistent with Fig. 10 in M2018), while Fig. 6d for optically thin cirrus is almost featureless with N_i < 100 L^{-1} in general.*"

(S 9) Line 251f: ··· *highest* (IWC) *values in* (geometrically) *thinner clouds in Category 2 for a given Tr, were not anticipated.*

Possibly the higher IWC, especially at warmer temperatures in the geometrically thinner cirrus clouds, indicates that these are young cirrus which have not yet lost any ice particles through evaporation in temperature fluctuations (see Jensen et al., 2023). During aging ice particles are lost by evaporation and sedimentation, so the geometrical thickness increases and the IWC decreases.

The above sentence has been replaced by these two sentences: "*Jensen et al. (2024) show that N_i and IWC are higher in younger tropical cirrus clouds due to mesoscale temperature fluctuations from gravity waves that act to decrease them over time. This may help explain these results if hom cirrus are associated with young cirrus.*"

(S 10) Line 253ff: *For a given T_r, D_e tends to be quasi-constant, although usually decreasing for the thinnest clouds in both categories, possibly due to entrainment. But this D_e decrease could also be due to hom in Category 2* (thick cirrus)*…*
It could also be that the larger ice crystals in geometrically thicker cirrus clouds, especially in the tropics at warm temperatures, indicate liquid origin cirrus clouds. I think that is more likely than the occurrence of hom.

This speculation has been removed from this sentence, which now reads: "*For a given T_r, D_e tends to be quasi-constant, although decreasing for the thinnest clouds in both τ categories.*" Moreover, as shown in Fig. A3, in situ D_e is of comparable size to that of WBC D_e.

(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.*"

(S 12) Line 476f: *When* D_e *> sensitivity limit, we set the sample as het-only.*
Here I have strong concerns, as outlined in point **G 4.**

"Het-only" in the above sentence has been changed to "het cirrus". New text has been added immediately below the sentence cited above that may address this concern, and is presented here as well: "*Applying this* D_e *sensitivity limit to all cirrus cloud samples over oceans reduces the estimated hom fraction by less than 8 % at* $T_r < 211$ *K. The largest changes are for* T_r *within the 223-235 K interval, where the hom fraction is reduced by 50 % in the tropics, and by 22-25 % in the extra-tropics.*"

(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 source 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 can be 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.*"

(S 14) Line 575, Figure 22: I wonder if it wouldn't be easier to understand if LO and IS were the same color (LO grey, IS red) and hom / het the color shades ? And then to plot LO (het/hom) over IS (het/hom).

In this paper, our objective is to estimate the fraction of cirrus clouds strongly affected by hom and conversely those clouds strongly affected by het. This distinction is more readily apparent in our view by assigning one color type to these het cirrus (grey) and another color type (pink/red) to the hom cirrus, where WB and IS are indicated by color intensity. Therefore, we prefer to keep this figure as it is.

(S 15) Line 604f: *Interpreting these w regimes as het and hom regimes, respectively (which was not done in Kramer et al., 2016) ….*
It has been discussed by Kramer et al. (2016) that het dominates in slow updrafts with low IWCs and hom in fast updrafts with high IWCs, see their Figure 6 and corresponding text.

The text here has been modified as follows: "The slow w regime *where het dominates* was characterized by IWC < median IWC, relatively low N_i, and relatively large mean mass volume radius R_v. Conversely, the fast w regime *where hom dominates* was characterized by IWC > median IWC, relatively high N_i, and relatively small R_v. Interpreting these w regimes as het and hom regimes, respectively, this study reports similar findings in Figs. 9 and 10 *(where α_{ext} correlates strongly with IWC).*"

(S 16) Line 650, Figure 25: Considering point G 4, does this scheme fit?
I'll stop commenting here (but have read the rest of the paper); I think there are enough points to be revised, after which the remaining parts could have changed. So I will wait for the next version of the manuscript.

The relationships shown in Sect. 4 (relating T_r-T_{top} maxima to D_e maxima) have now been evaluated separately for in situ cirrus and WBC clouds. While the cloud thickness differs considerably between these cloud types, the relationships observed for WBC clouds still appear applicable to in situ cirrus, although more explanation is needed to see this. This section has been largely rewritten.

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Jensen, E. J., Karcher, B., Woods, S., Kramer, M., & Ueyama, R. (2024).
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Wernli, H., M. Boettcher, H. Joos, A. K. Miltenberger, and P. Spichtinger (2016),
A trajectory-based classification of ERA-Interim ice clouds in the region of the North Atlantic storm track, Geophys. Res. Lett., 43, 6657–6664,
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Table 1. Notations used for CALIOP retrievals of cirrus clouds.

Notation	Definition or interpretation
T_{top}, T_{base}	Temperature at cloud top, temperature at cloud base
T_r	Cloud layer radiative temperature, on average in the middle between T_{top} and T_{base}.
τ	Visible cloud optical depth
IAB	CALIOP 532 nm layer integrated attenuated backscatter
Thin cirrus	IAB < 0.01 sr^{-1} ~ 0.01 < τ < ~ 0.3 : optically thin cirrus including some subvisible cirrus*
Thick cirrus	IAB >0.01 sr^{-1} ~ 0.3 < τ < ~ 3 : optically thick cirrus, but semi-transparent to the lidar
All cirrus	~ 0.01 < τ < ~ 3 ; includes optically thin and thick cirrus clouds

*subvisible cirrus (τ < 0.03)