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

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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 Krämer 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 Krämer 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.

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 N i (and thus αex) 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 D max (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 als 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‘ ?).

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 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 (Krämer 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.

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 afactor 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...

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...
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.

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.

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.

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.