egusphere-2026-2416 | Journal relation: ACP

Origin and evolution of satellite-observed cirrus clouds using Lagrangian microphysical modeling – Part 1: Method and case studies

Athulya Saiprakash, Martina Krämer, Christian Rolf, Patrick Konjari, Jérôme Riedi, and Odran Sourdeval

SUMMARY

This work implements a novel method to perform back trajectories of three different case studies to investigate the microphysical pathways of cirrus clouds following guidance from satellite retrievals. The authors do an excellent job outlining the problem, establishing the methods, discussing the case studies and results. The results are intuitive, simple, and easy to follow without being overwhelming. The paper is structured logically and well written. I have some general remarks and specific comments listed below, but no breaking changes, mostly curiosities and some suggestions to improve robustness in a handful of locations. I am appreciative of the interesting work, and easy read! Based on the comments provided below my suggestions would align with minor revisions.

GENERAL REMARKS

1. As far as I can tell the microphysics scheme used, CLAMS-Ice, does not consider the evolution of ice particle shape. Since the simulations herein are of cirrus clouds, focusing on the evolution of these clouds over time, and since ice particle growth & evolution are quite a critical component of how these clouds evolve, it seems that not considering the shapes & densities of the ice crystals and how they evolve over time *could* result in errors in the overall trajectory microphysics, and even the cloud lifetime in some situations. I think it important that the authors spend some time defending the lack of shape evolution – this can have a significant impact on the depositional growth rates of the particles, the vapor availability, and thus local thermodynamics of the simulated cirrus.
2. Lines 146-148: This is really interesting. I think it would be helpful to expand on what kind of variability in ice processes is captured in these fluctuations. Which processes are represented? What is the range of temperature fluctuation, and what is meant by “small-scale”? From what investigations (simulations? lab studies?) are these fluctuations derived? The preceding paragraph mentions the fluctuations as resulting from dynamic processes (e.g., gravity waves), so is what is described here also a result of dynamic processes? I think you could justify your lack of ice shape parameterization if you can show that the temperature variations at least somewhat capture the variations in latent heating as a result of active shape-aware particle growth, though, no entirely as there are many other ice growth processes that have an effect (e.g. increased fall speed/sedimentation/aggregation rates, etc).
3. Lines 255-256: Regarding the RH values for the members, the line you say here about nucleation and growth events bringing the RH back down is an important one and worth looking into. What can happen a lot of times is that when RH is reported in output files for simulations, it is often the value at the end of the microphysics scheme (even if the time step is small), which means it is corrected for all the condensation/deposition/etc that takes place. Depending on the order of operations in

the microphysics module, your RH is likely higher during critical growth and nucleation periods and then adjusted back down at the end of the module and when the "final" end-of-time-step value is reported. If you output intermediate RH values, I am sure you will reach higher values as you would expect. Further, keep a lookout for any artificial saturation adjustments that are sometimes included to maintain model stability but are not necessarily physical, i.e., condensation of all leftover available vapor, etc, that may be parameterized in the model, as those adjustments would also bring down the RH values.

SPECIFIC COMMENTS

1. Line 198 - TTL not defined.
2. Figure annotations are small. I can see them only when zoomed in. Would not be able to see them in a printed document.
3. Figure 2: purple dots instead of purple crosses? Also the purple text is very difficult to see in the beginning of the trajectory.
4. Line 294: The asynchronous nucleation as evidenced by the spread (in the ensemble members) and also possibly the "sawtooth" trend over phase 2?
5. Lines 297-298: "This clearly indicates…rather than water vapor" – You could test this theory by increasing the initial value of Ni, or by artificially entraining in more INP once it drops to a negligible value.
6. Lines 300-302: I am having trouble following these 2 sentences. I think it would help to identify the hours more explicitly where you are discussing the signatures so it is easier to follow.
7. Fig 3: apologies if I am not interpreting the computation of panels j-l correctly, but can the ensemble members be shown for these too?
8. Line 375: "indicating statistically weaker metrics" is an odd phrasing. Maybe instead "indicating a lower probability for active microphysical processes".
9. Line 383: Here and elsewhere Ni is used in text but Nice is used as panel labels.
10. Line 384: Collection leads to larger crystals. Given the higher RHi here, deposition probably does too. Sedimentation, depending on speed, could have different affects so I would probably wouldn't use this term here.
11. Lines 386-387: Reference to the RH output again here. See #3 above in general comments. A quick and easy check on this would be to rerun the case but change the output location of the RH value to right before the microphysical growth processes that will indicate the actual available moisture leading to particle growth, rather than what is probably the final value in the microphysics scheme *after* growth and possible saturation adjustment, if applicable.
12. Line 392/Figure 4 – Opacity is a difficult metric to use for comparative purposes for this plot. Might I suggest for this panel using unfilled contours and overlaying the three ice origin sources. Their densities would show their relative contributions and you would be able to see where they are co-located where the contours overlap.
13. Figure 4 – Can you please annotate parcels A and B re: the purple dots since they are referenced in the text?
14. Figure 4 – For some of these, panels, particularly b,d, & e, it is difficult to discern the signatures since the contrast is so low given the chosen color bar. It may be more

effective for these to use unfilled contours so that location/color is more clear and density would be a more obvious indicator of magnitude.

15. Figure 5 – A few things: (1) it is difficult to see the background regional contours in a, (2) very difficult to see the green line in a, (2) b & c the tick labels are too small
16. Fig 6 – There appears to be a rogue purple dot that is not referenced in the caption or text.
17. Fig 7 – How are the frequencies computed? All grid points? Ensemble members? Is there any averaging?
18. Line 493 you seem to use ~0.75 as a threshold to indicate cloud age predominance, but then in Line 502 this shifts to over 0.1. It would be a more robust argument to define a threshold (maybe using percentiles if that is more effective) to illustrate cloud age predominance (and other metrics for that matter) than to really on qualitative assessment. Should be pretty easy to do since you have the metrics readily available.
19. Line 509: "homogeneous nucleation slightly dominates" – please quantify this. Hard to eyeball.
20. Line 540: You mention sedimentation as being a factor a couple times throughout the paper, but never quantify it, it is mostly inferred. Would it be possible to quantitatively illustrate the effect of sedimentation in your simulations?
21. Lines 549-556: This paragraph seems to be describing the high updraft case, not the orographic case. I definitely want to see a paragraph on the orographic case which shows a dominance of heterogenous nucleation near cloud top.