Review for Atmospheric Chemistry and Physics (ACP)
Handling Editor: Kara Lamb

Title of EGUsphere preprint: Origin and evolution of satellite-observed cirrus clouds using Lagrangian microphysical modeling - Part 2: Evaluation and sensitivity analysis

Author(s): Athulya Saiprakash, Martina Krämer, Christian Rolf, Jérôme Riedi, and Odran Sourdeval

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General Comments:

It was difficult to evaluate Part 2 of this study without having access to Part 1 (which I was unable to access), so Part 2 is evaluated here as a stand-alone paper. The paper appears well-conceived and organized, although it was ambiguous to me how CLaMS-Ice was initialized from the back-trajectories. Origin based metrics were often mentioned without explaining what they refer to. The results make intuitive sense to me. I recommend publication with minor revisions.

Specific Comments:

1. Figure 1: As noted on page 6 (1st full paragraph), the bimodality for Ni in Fig. 1 for CLaMS-Ice is due to the differing ice nucleation processes (hetero- and homogeneous freezing). This bimodality does not appear for the satellite retrievals due to the assumption of a monomodal PSD, but bimodality is apparent for the in-situ data. However, this Ni bimodality for the in-situ data has a secondary pdf maximum at higher Ni relative to the primary pdf maximum at lower Ni, whereas the opposite occurs for CLaMS-Ice (where the primary pdf maximum occurs at higher Ni relative to the secondary maximum). Noting that homogeneous freezing nucleation tends to produce higher Ni relative to heterogeneous freezing, does this suggest that homogeneous freezing nucleation is more active in CLaMS-Ice relative to the in-situ observations? Moreover, these two pdf maxima in CLaMS-Ice occur at lower Ni values relative to the in-situ Ni observations. Do the authors have an explanation for this?

2. Lines 155-6: What was the average time difference between in situ sampling and the satellite overpass time?

3. A simple formula for estimating the IWC associated with homogeneous freezing nucleation (hom) is provided and validated in Mitchell and Garnier (2025, ACP, Eqn. 4). Since it is merely the difference between water vapor density at the supersaturation threshold for hom and the vapor density corresponding to ice saturation, it may be a lower bound since stronger updrafts may produce vapor densities that exceed this threshold and may provide more time for ice particle growth within a cloud layer. Nonetheless, it may be an interesting benchmark that may impart greater physical understanding of the results shown here in Fig. 1 and elsewhere. This is only a suggestion that the authors may use if they feel this would improve their paper.

4. Lines 265-7: Regarding temperature fluctuations in the high updraft case in Fig. 3, these results appear consistent with the Ni results in Fig. 1, with the primary maximum in CLaMS-Ice associated with hom (higher Ni) becoming more dominant with increasing temperature fluctuations. However, the in-situ measurements indicate that hom produces a secondary pdf maximum (i.e., not a primary maximum as with CLaMS-Ice). Again, does this suggest that hom may be overactive in CLaMS-Ice?

5. Line 341: Was Δt_{traj} discussed in Sect. 3 or elsewhere?

6. Fig. 5. Please provide a reason why the orographic case is almost pure in-situ cirrus, even when f_{sed} = 1 (i.e., no sedimentation). How representative do you think this case is regarding orographic cirrus?

Technical Comments:

1. Abstract: "Nevertheless, it appears from the sensitivity analysis that origin-based metrics proved relatively robust except under most configurations." This sentence is hard to understand since the last part (except under most configurations) appears to contradict the earlier part.

2. Line 143: "interquartile range"; does this mean the 25 to 75 percentile range? If yes, is it commonly understood?

3. Line 182: "observations" => satellite observations?

4. Line 223: "depletes" => enhances? (Fig. 4 and Spichtinger and Gierens, 2009a, ACP)

5. Line 308: "reducing" => increasing?

6. Line 322: “depletes” => increases? (ditto)