**Review of *Role of INP activation schemes and concentrations in controlling ice formation pathways in Warm Conveyor Belts* – Lüttmer et al. (2026)**

## General comments

This manuscript investigates the sensitivity of ice formation pathways in a warm conveyor belt (WCB) to the representation of heterogeneous ice nucleation and secondary ice production. Using ICON with a two-moment microphysics scheme that predicts ice modes, the authors examine the effects of varying ice nucleating particle (INP) concentrations, heterogeneous nucleation parameterizations, and secondary ice mechanisms. Particular attention is given to the contributions of liquid-origin and in-situ ice formation within the WCB and its cirrus outflow.

The topic is appropriate for ACP, and the ability to distinguish the origins of cloud ice offers a useful framework for examining microphysical processes. The experiments reveal substantial changes in the partitioning among formation pathways, particularly between homogeneous and heterogeneous nucleation. The manuscript is generally well organized and contains interesting results.

However, I have concerns about the experimental design, interpretation, and presentation. The relatively coarse grid spacing raises questions about representation of processes central to the analysis. A single WCB event, characterized by strong dynamical forcing and important sedimentation of ice formed above the WCB, limits the broader applicability of the conclusions. The handling of interactions among ice modes also needs clarification, particularly because aggregation transfers mass to snow without retaining its formation-pathway identity. Finally, the figures make it unnecessarily difficult to quantify differences among experiments.

I recommend major revisions.

If the authors have any questions or would like clarification, please do not hesitate to contact me!

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## Major comments

**1. Horizontal grid spacing and microphysical processes:** The simulations use an effective horizontal grid spacing of approximately 6.5 km. While this may capture the broad synoptic structure of the WCB, it is quite coarse for investigating sensitivities in ice formation pathways, particularly secondary ice production and competition between homogeneous and heterogeneous nucleation. These processes depend on local vertical velocity, supersaturation, liquid water content, and hydrometeor interactions, which can vary at much smaller scales.

How are unresolved vertical velocities and supersaturation variability represented, particularly in the homogeneous nucleation treatment? Could the grid spacing suppress localized riming and collisions relevant to secondary ice production? A higher-resolution sensitivity experiment, perhaps using a limited nested domain and selected experiments, would be valuable. If this is not feasible, the authors should substantially expand the discussion of resolution dependence and limit their process-level conclusions accordingly.

**2. Single-case analysis and dynamical forcing:** The study considers one North Atlantic WCB event. The authors conclude that the balance between liquid-origin and in-situ ice is relatively insensitive to microphysical choices and is primarily controlled by dynamics and sedimentation. Yet this case includes substantial ice formed in a cloud layer above the WCB that subsequently sediments into the ascending cloud. This configuration appears central to the results and may not be representative of other WCBs.

A second case with contrasting ascent characteristics, weaker dynamical forcing, or less influence from overlying ice would provide a stronger test. If additional simulations are not feasible, the conclusions should be restricted to this event. Statements that dynamics generally dominate over microphysical assumptions cannot be established from the present design alone.

**3. Interactions among ice modes and formation-pathway bookkeeping:** The manuscript does not clearly explain how particles belonging to different ice modes interact. The modes distinguish initial formation pathways (and this is really cool and I was so happy to see this being used!), but particles originating through different mechanisms may subsequently collide, aggregate, or collect one another. For example, what happens when an immersion-frozen ice particle aggregates with one formed by deposition nucleation? Is the combined mass transferred entirely into snow, and is its original pathway information then lost? Are other cross-mode collisions or collection processes handled differently?

Explicitly tracking every combination of origin pathways would quickly become impractical as particles undergo successive interactions. I am not suggesting that additional mixed-origin prognostic categories are necessarily required. Rather, the authors should clearly document the rules used for cross-mode interactions, where the resulting mass and number are assigned, and how conservation is ensured. This bookkeeping is fundamental to interpreting the reported ice-origin fractions.

**4. Aggregation and the exclusion of snow from ice origin attribution:** As described, the origins of cloud ice are tracked in five modes, whereas mass transferred to the snow category through aggregation no longer retains its original formation pathway identity. Figure 3d indicates that snow accounts for approximately 34–43% of frozen hydrometeor mass in the cirrus regime. Thus, the diagnosed liquid-origin and in-situ fractions apply only to the remaining cloud ice, not to all frozen hydrometeor mass.

This matters if aggregation rates differ among modes or experiments: a change in the composition of the remaining cloud ice could reflect preferential transfer of one pathway into snow, rather than a corresponding change in its contribution to the full ice population. Can the authors quantify mass transferred from the individual modes into snow, or otherwise assess the potential bias? At minimum, the limitation should be made explicit wherever the ice-origin fractions are interpreted, and claims about the origins of all ice should be distinguished from claims about surviving cloud ice.

**5. Presentation and quantification of sensitivities:** The plots provide substantial information about individual simulations but make *differences* among experiments surprisingly difficult to see. Figures 2 and 4 are especially challenging, namely, separate panels with broad percentile bands force readers to compare similar distributions visually. Because experiment-to-experiment sensitivity is the main focus, please consider overlaying median curves on common axes and adding difference plots relative to HA, providing concise quantitative metrics for the largest changes, or presenting the results as differences from the control.

Figure 3 relies heavily on normalized fractions; these can change without corresponding changes in absolute ice mass or number. Adding absolute quantities or differences would help distinguish direct process responses from relative redistribution.

There also appears to be a plotting inconsistency in Fig. 5i, namely, a light-blue region appears inside the dark-blue band near 263–267 K. If these are the 5th–95th and 25th–75th percentile intervals, respectively, the bands must be nested. Please check the percentile calculation and/or rendering, and review the other panels for similar artifacts.

**6. Trajectory comparability and integrated statistics:** Please clarify whether identical initial trajectory locations are used in every experiment, whether the same trajectories satisfy the WCB criteria, and how differences in the selected trajectory populations are handled. The assertion that the WCB dynamics are nearly identical across simulations is not supported by a quantitative comparison. Even modest differences in ascent histories, temperature, or supersaturation could influence the microphysical distributions.

Equation (7) describes volume- and time-integrated fractions, yet the analysis is carried out along trajectories. How is the numerical integration implemented? Are trajectory samples weighted by represented air volume or mass? The relation between trajectory statistics and volume-integrated diagnostics needs to be made explicit.

**7. Evidence for sedimentation and aggregation explanations:** Sedimentation and aggregation are repeatedly invoked to explain the responses, particularly ice formed above the WCB entering the mixed-phase cloud and reduced number concentrations in HA+ near 265 K. These explanations are plausible, but the analysis primarily presents concentrations and fractions rather than the corresponding *process tendencies or fluxes*.

Given their importance to the manuscript's conclusions, selected aggregation budgets, sedimentation fluxes, or ice mode tendencies would strengthen the attribution. If these diagnostics are unavailable, interpretations should be framed more cautiously as hypotheses consistent with the reported distributions.

**8. Interpretation of secondary ice sensitivities:** The RS-only, BR-only, DF-only, and all-mechanisms experiments are useful, but individual mechanism experiments do not necessarily isolate incremental effects under simultaneous operation. The processes can interact through changes in ice number, riming, and collision rates. Please clarify the scope of the attribution and discuss nonadditivity; a limited set of *combined mechanism tests* would help if feasible.

The droplet fragmentation implementation considers freezing raindrops, excluding large cloud droplets or drizzle droplets that may also fragment. Likewise, the collisional breakup implementation has particular collision category and parameterization assumptions. Conclusions about the relative importance of these mechanisms should be expressed as conditional on the implementations tested, rather than general statements about their physical significance in WCBs.

**Specific and technical comments**

1. **Lines 83–90:** Identify the specific ECMWF forcing dataset. Clarify whether diagnostics and trajectory calculations use the native or remapped grid, and whether remapping affects the analyses.
2. **Eq. (4):** Check the notation DF versus DS for droplet fragmentation.
3. **Lines 157–163:** The use of "cirrus" for all pure ice-phase clouds without a temperature threshold is defined, but potentially confusing. Distinguish warm pure ice clouds from cold cirrus consistently.
4. **Eq. (7):** Specify the dimensions, units, weighting, and numerical procedure behind the volume-integrated fractions.
5. **Section 3.1 and relevant figures:** The total cloud ice mass mixing ratio $q_{tot}$ is reported in ppmv. As a condensed water mass mixing ratio, conventional units, such as mg $kg^{-1}$ or g $kg^{-1}$, are preferable. If ppmv is an equivalent molar conversion, define and justify it, ensuring unit consistency.
6. **HA+ interpretation in Section 3.1:** The proposed enhanced aggregation and sedimentation explanation is interesting, but the analysis would be enhanced if tendencies were presented.
7. **Lines 390–394:** Verify the notation for the reported PH number fraction: the text describing HOM dominance appears to label the 0.78 fraction as DEP.
8. **Fig. 5 caption:** Check that the caption correctly describes the secondary ice quantities and panels rather than total cloud ice quantities.
9. **Fig. 5i:** Check the apparently non-nested 25th–75th and 5th–95th percentile shading near 263–267 K.
10. **Lines 470–474:** Verify the figure references (Fig. 2b/f versus Fig. 5b/f).
11. **Table 1 and figures:** Use one experiment label consistently for the simulation with all secondary ice processes turned on (ASEC versus ALLS). Also, consider using SIP over SEC for consistency with the literature.