Overview

This study evaluated the impacts of various INP activation schemes, INP abundance, and secondary ice production (SIP) on the microphysical structure and evolution of clouds formed in a warm conveyer belt (WCB) case study. The most prominent result is that the sensitivity to INP activation scheme is as large as the sensitivity to INP abundance, which in itself is an important finding, especially within the context of this "real case" simulation. The study focused on the ice microphysical structure in ice-only (called "cirrus" in this study) versus mixed-phase portions of the cloud, in addition to the origins of the ice formation mechanisms using a novel microphysics scheme that tracks the ice formation representation which allowed partitioning between homogeneous and heterogeneous ice formation pathways. Perhaps unsurprisingly, the study demonstrates that increased INP leads to greater heterogeneous freezing. They finally showed that SIP is most important in modulating the mixed-phase portion of the cloud system, which is also expected given the SIP mechanisms that were evaluated. Overall, the study is very well-designed and the evaluation methods are robust. The presentation quality needs some improvement, namely in correcting grammatical errors and clarifying a number of statements. In particular, there are a few high-level concerns that need to be clarified (see Major Comments below); while these comments do not necessitate any substantial corrections to the methodology, they do require clarification and careful consideration/discussion for interpreting the results. The recommendation is therefore to return the manuscript for minor revisions.

Major Comments

1. **Horizontal grid spacing:** 6.5-km horizontal grid spacing seems rather coarse. The domain is large, so it is understood that computational expense must be managed. However, this is firmly within the convective gray zone and only borderline cloud-resolving. In lieu of resolution sensitivity tests, please discuss any implications for how grid spacing may impact the presented results.
2. **Aerosols & INP initializations & mechanics:** It is implied on Lines 225-227 that INP is pseudo-prognostic. However, there is no information on the spatiotemporal structure of INPs or aerosols. Are these considered to be horizontally homogeneous? What about vertically? It is stated that the number concentration of activated INP is relaxed to zero outside of clouds (which is what I mean by pseudo-prognostic), but then how are INPs made available for nucleation in the first place (with no cloud, i.e., during spin-up)? I guess this boils down to whether or not INP is truly prognostic or not. To that end, how are the magnitudes of the INP reservoir guided? Is it idealized or based on climatology? That matters for a cloud system this large. The same question is posed for aerosols, and particularly for the HOM process. What are the assumed chemical compositions and 3D spatial distributions of aerosols? How is cloud activation treated? All of this information should be provided.
3. **"Feeder" effects?**: On lines 235-237, the (much appreciated) transparent introduction of an overlying, precipitating cloud layer is introduced. Feeder clouds can have a strong influence on ice nucleation in the underlying cloud layers, so this seems like a

really important detail of this case. Please discuss how the introduction of ice from above may impact your findings and associated budgets. I *think* this is discussed a bit on Lines 535-339 but I'm not really sure.

4. **On raindrop fragmentation:** On Lines 478-479, you extrapolate that DF is likely weak because raindrops form primarily at temperatures above 270 K where immersion freezing is weak and the Phillips parameterization is also weak. But this made me wonder if the scheme is freezing raindrops only via Bigg (1953) or if there is also a raindrop freezing mechanism upon collisions of raindrops and cloud ice? If not, that may be a substantial gap in quantifying the fraction of ice number that are coming from DF. It has been hypothesized that RM provides such splinters that can interact with the supercooled raindrops in these warm mixed-phase regions and thus form a cascading process. Please clarify if this multiplication pathway is active in your simulations, and if not, what implications that has on your conclusions.

Minor Comments

1. **Section 2.2:** This is the first time that the reader is hearing about WCB trajectories. I would add a brief clause or sentence stating the purpose of doing this (i.e., to evaluate microphysical evolution along the trajectory of a surface-based parcel, or something like that). This was more of a focus in the previous paper by the authors, which is a foundation for this study, but for readers who read this as a standalone paper, a little more context needs to be provided.
2. **Eq. 1:** In the accompanying discussion (Line 114), please make this equation more self-contained. The definition of n has not yet been provided, nor that T is the ambient temperature. While this is intuitive to most readers, it is best practice to define all variables the first time they appear.
3. **Eq 4:** Please define what N_{DS}, beta, and zeta are.
4. **Contact freezing:** Please discuss the reason why contact freezing is not considered in the scheme and any implications this may have for this case.
5. **Units of mass concentrations:** The cloud physics community has not historically used ppmv to describe mass concentrations of hydrometeors. While not incorrect to do so, I do think presenting them with these units (e.g., in Fig. 2) will be a burden to a large fraction of your readers who will generally anticipate and understand mass mixing ratios in units of g/kg (or kg/kg, or mg/g, etc.). This isn't a necessary change, but I would encourage it. There are other portions of the manuscript that use units of kg/kg (e.g., Lines 417-419), which makes this particularly confusing and inconsistent. To that end, threshold values for q_{tot} are first defined in units of mg/kg (Lines 157,170, also q_{liq} on Line 160), but then a value called "q_{crit}" is introduced (but has not been previously defined) on Line 423 in units of kg/kg. Being consistent here is warranted and avoids confusing the reader.

6. **Size sorting (Lines 272-273):** While I don't disagree with the statement here for why q_{tot} decreases more sharply than n_{tot}, I'm a little confused on why this signal would show up given my interpretation that q_{tot} and n_{tot} do not include precipitating ice species. Am I mistaken? Or do these total quantities actually include snow, graupel, and hail? Or is the signal arising because of conversion from ice being tracked in any of these modes to larger, precipitating species? Or does the cloud ice species actually have fallspeeds large enough to warrant sedimentation? Please clarify and potentially address.
7. **Fig 2:** This is purely a suggestion, but it could be worth including a supplemental figure that plots one panel for each row of Fig. 2 but displaying the various experimental medians on the same panel with different line colors. This would make the qualitative comparison more visually instructive for the reader. Alternatively, such a panel could be added as a 6th column to Fig. 2.
8. **PH DEP at 260 K (Lines 292-293):** Fig. 2 does not show any clear evidence of this factor of 2 increase in n_{tot} at 260 K. While the reader should trust your quantitative statement, this would show up much more easily if Fig. 2 had an additional column with all the medians on one panel (see previous comment). Moreover, it is stated that this reflects "the higher freezing temperatures and thus earlier onset freezing associated with this INP activation scheme". But this doesn't look supported by Fig. 1d, where it seems like PH actually has the *lowest* onset freezing temperatures. Am I mistaken here? One more minor thing: This single sentence is currently a standalone paragraph, which I assume was not intended.
9. **Sections 3.2 & 3.3:** These sections seemingly omit any discussion of the additional SEC experiments. This is because you go into these in more detail in Section 4. But since these results are shown in Fig. 3, I would add a short placeholder sentence somewhere in these sections saying that the SEC experiments will be discussed in detail in Section 4.
10. **Figure 5 caption:** This caption does not appear to describe the figure at all. Please address.
11. **On stochastic INP schemes (Lines 544-545):** I appreciate you mentioning these alternative approaches, but I would provide some references as well. You could pull some references from Sun et al. (2026) and use that as a guide for some minor discussion about any expected differences between the singular and stochastic schemes, though that isn't absolutely necessary; it would just strengthen the discussion and interpretation a bit.

Technical/Semantic Corrections

- **Line 5:** I would add a hyphen between "ice" and "modes" here, as you do in the introduction. Additionally, the manner in which this terminology is presented is confusing, because it is not clear here that "ice modes" is the name of the bulk scheme. It

rather seems like a descriptor. Perhaps there is some way to reword this, such as "using the ICON model with a two-moment scheme, called 'ice modes', that…". It would be even better to maybe give it an acronym, like Ice Modes (IM), or anything that makes clear it is the name of the scheme instead of it sounding like ambiguous adjectives.

- **Line 51:** Do you mean limit their application *to* cloud-resolving and sensitivity studies?
- **Line 86:** "…up to using the SLEVE…" – Is something missing here? It sounded like you started saying "…90 vertical layers up to <Insert max altitude> using the SLEVE…" If not, then this sentence is confusing. Please address.
- **Line 87:** "conditions is" should be "conditions are"
- **Line 91:** I think "Louis (1979)" should be the alternative parenthetical here: (Louis, 1979).
- **Line 111:** "There" should be "The", I think, unless you are referring to the possessive of Hallett and Mossop (1974), in which case it should be "Their".
- **Lines 162-163**: It would be good to state here the temperature range that occupies the pure ice category in your dataset.
- **Lines 176-179:** You state here that liquid-origin cirrus is defined as droplets transported to the "cirrus" temperature regime, and then define that regime as $T < T_{hom}$, but you stated earlier that temperature is not used as a discriminator of pure ice clouds (cirrus) and that rather it is only a microphysical definition. It seems like the temperature has no influence on defining the liquid-origin cirrus, which is instead described by the ratio given in Eq. 8. Perhaps you stated the temperature threshold merely as a convenience for description, but as stated it is a little confusing with the definitions given just prior to this introduction.
- **Line 207:** "panel" should be the plural "panels"
- **Line 225:** There should not be a comma between "both" and "IMM"
- **Line 233:** "more than ore than" is a typo. Please address.
- **Line 234:** "Lüttmer et al. (2025a)" should be the alternative parenthetical: (Lüttmer et al., 2025a)
- **Line 235:** "ascend" should be "ascent". You also have duplicated "with" later in the sentence.
- **Line 241:** "hat" should be "that" at the end of this line.
- **Figure 2 caption:** Please state in the caption or a legend what the red lines are on panels (k)-(o) (I assume it is the mean) and the horizontal black line (which is defined in the text as the homogeneous freezing level, but not in the figure caption or a potential legend)
- **Line 253:** There is a stray "0.1ppmv" between the 2 sentences
- **Line 341:** "favor" should be "favors"
- **Line 364:** "…for the cirrus regime of in the WCB…" should be "…for the cirrus regime in the WCB…"

- **Lines 392-394:** This is an incomplete sentence, I think. It is difficult to understand the statement. Please address.
- **Figure 4 caption:** "…evolution o five" should be… "…evolution of five…"
- **Line 403:** "for as a function" should be "as a function"
- **Line 426:** Do you mean sublimate, not evaporate?
- **Line 464:** The variable T_{cir} has not been introduced; I assume this is a typo.
- **Line 467:** "BR is dominates" should be "BR dominates"
- **Line 470:** "This different" should be "These different"
- **Line 471:** Assume you mean Fig. 5f and not Fig. 2f
- **Line 473:** Assume you mean Fig. 5b and not Fig. 2b
- **Lines 472-473:** Regarding the statement " A similar upward shift is not evident in the SEC ice mode mass mixing ratio (Fig. 2b).": While the shift isn't evident in the mean or median, it is evident in the 75th percentile.
- **Line 489:** "parametrization" should be plural
- **Line 490:** Only a suggestion: it's often nice to restate any abbreviations in the Conclusions section of a paper, since a large fraction of readers will not go through the whole paper. You did this for defining the secondary ice processes (Lines 491-492). I would suggest to also expand the acronyms used for the primary ice formation parameterizations on this line.
- **Line 491:** It should be freezing *drop* fragmentation, not *droplet*
- **Line 496:** "ration" should be "ratio". Again on Line 502 & 527.
- **Lines 504-505:** I would add another uncertainty here: the parameterizations themselves. There are only a few available to the community for each mechanism, often using empirical formulations from outdated laboratory studies, and thus mechanism denial experiments like that employed here are only interpretable within the context of that parameterization's construction and associated uncertainties.
- **Line 518:** "occurs" should be "occur"
- **Line 527:** Again, a casual reader will have no idea what g_{dep} and g_{hom} are. I would redefine them or just spell out what they mean here and not use symbols at all.
- **Line 529:** "…study Lüttmer et al…" should be "…study by Lüttmer et al…"
- **Lines 548-550:** I'm a bit confused by this statement. You say that the previous study includes an investigation of ice formation pathways as a function of ascent time—but isn't that also what this study did? Or are you implying that the previous study did it for multiple cases and that's how the results here can be extrapolated from the previous study?
- **Line 543:** I would make clear that you showed the impact of these uncertainties and assumptions re: INP activation and abundance on the dominant ice formation pathways *in WCB clouds*, not generally.

References

Sun, Y., Fridlind, A. M., Silber, I., Riemer, N., and Knopf, D. A.: Prognostic simulations of mixed-phase clouds with model AC-1D v1.0: the impact of aerosol types and freezing parameterizations on ice crystal budgets, Geosci. Model Dev., 19, 1581–1617, https://doi.org/10.5194/gmd-19-1581-2026, 2026.