Reviewer 1

Review of

"Ice formation processes key in determining WCB outflow Cirrus properties"

by Lüttmer, Miltenberger and Spichtinger, EGUsphere-2025-185

General comments and recommendation:

The authors use a newly developed ice-cloud microphysics scheme that allows to treat several groups of ice crystals, that is, to distinguish them. The distinction used for the present purpose is their respective formation origin, in particular, whether their formation is via the freezing of pre-existing water droplets ("liquid origin") or whether they form directly in the ice phase, either heterogeneously (deposition nucleation on solid aerosol) or homogeneously (by freezing of cold solution droplets at high ice-supersaturation, but below water saturation). This new method is applied to the case of a Warm Conveyor Belt (WCB) where moist air is transported within 2 days from the boundary layer to the upper troposphere, which leads to adiabatic cooling, droplet formation, later freezing, and in the outflow region to in-situ ice formation. Simultaneously there is further moist air and cirrus in the same region as the WCB but already initially in the mid to upper troposphere (above 5 km). These airmasses are affected by the strong dynamics below and there is ice formation in these volumes as well. The new method is able to distinguish the formation mechanisms of the ice crystals that are present in Lagrangian air parcels (I assume, that this information is fed in from a basic Eulerian grid).

The obtained results are interesting, in particular, since an alternative distinction method, labelled W16, shows "confusion" once there is sedimentation of ice that is not or cannot be considered in the W16-method. The results show as well, that other simple classification approaches (many small vs. few large crystals) can be misleading, probably again because of the confusing effect of sedimentation.

However, this is not yet a good paper and it needs a thorough revision. The paper has two faces, a good one with good scientific content, and a bad one with many sentences, expressions, etc. that are simply not good enough for a scientific publication. Furthermore there are a lot of grammar errors and sloppyness in type-setting. It is a pity! As a first example, please read the title. To my taste this sounds like slang. Shouldn't it read "processes that are key..."? An example for sloppyness is the lone header B2 in the appendix, which has no content following.

Response: Titles of publications accepted in ACP and other journals in atmospheric science often do not contain verbs or are grammatically correct sentences. See also the ACP guidelines for article titles examples. Due to the comments of Reviewer #2 we changed the title of the manuscript to:

“On the impact of ice formation processes and sedimentation on cirrus origin classification in WCB outflow”

The content of appendix B2 is the Figure B1. The Appendix, Figure B1 and references to it were removed to shorten the overall length of the manuscript.

There are worse examples in the text. One general comment, I would like to make, is that temperatures are never cold. This is popular speech. Air can be cold, and the corresponding temperatures are low. Please avoid such popular errors in serious writing.

Response: We replaced all eight instances of "cold" or "colder" with "low" or "lower", respectively. The only exceptions were references to the *cold sector* of a cyclone.

Summarising: this paper needs a **major revision** mainly of its presentation quality.

In the following, I start with a few science questions before I come to a longer list of textual problems. I leave the grammar errors and typesetting problems away.

Response: We carefully re-read the manuscript and improved the English language quality.

Scientific comments:

Lines 88/89: Please explain the criterion that pressure has to be less than 500 hPa. As a WCB starts in the boundary layer, this is a bit surprising.

Response: This text describes the methods employed by Luebke et al. (2016) and Wolf et al. (2018) for cirrus classification. These studies are not restrict classification of WCB cirrus. The pressure criterion is used in those studies to identify cirrus clouds, which are derived from mixed-phase clouds.

However, there was an error in the sentence, as it should read "more" than 500 hPa (instead of "less"). Then the wording is consistent with other phrases in this manuscript, where we refer to altitude with temperature or pressure as a proxy variable. This is corrected in the new version of the manuscript.

Lines 171 ff: Please justify the assumption of equal shapes and relations for all ice modes.

Response: The shape of an ice crystals depends on temperature and supersaturation, which controls the growth behavior of the crystal by deposition of vapor. The nucleation processes of the ice crystals plays likely only a minor role in predicting the final shape of the crystal. In general, our theoretical knowledge of ice crystals growth is still lacking. Models that resolve the growth of individual crystals are better suited for shape prediction than models that only resolve bulk properties of ice particle ensembles, such as the 2 moment scheme used in this study.

Generally, we keep the approach of the original ice scheme. Cloud ice has one shape property, and splitting up this single class into 5 with the same shape is consistent with the former approach.

We added a sentence in Section 2.2. addressing this point.

Section 2.2: Perhaps this is explained in the other paper (Lüttmer et al.), but it would be convenient for the reader to learn how in a given air parcel with a mixture of, say DEP and HOM nucleation particles, the dicision is taken which of the principally possible mechanisms wins? And how is it done? Do you use a "winner takes it all" principle, or is there a certain partitioning at work.

Response: The microphysics model follows the typical approach for bulk schemes. Hydrometeors are separated into classes (cloud droplets, rain, ice crystals, graupel and more...). Each class has two prognostic moments at each grid point modulating its sizes distribution. The various microphysical classes can coexist. For example liquid water drops of the cloud droplet and rain class can coexist in the standard scheme (and most other bulk schemes).

Following the same principal, ice from different nucleation processes can coexist at the same grid point and time in our model. There are now 5 classes of ice. This is similar (though with a different aim) as for the 2 classes of liquid water droplets. Different ice formation processes can be active within the same microphysical time step in the same grid point, modifying the prognostic moments of mass and number densities of their associated ice classes. Hence, there is no “winning”. The nucleation process just depends on the thermodynamic conditions in the grid cell.

The values of the prognostic moments (mass and number density) of the ice modes are interpolated from the Eulerian model grid the location of the air parcels, same as for all other microphysical and atmospheric model variables.

We added a few sentences to Section 2.2 for clarification.

Eq. 3 and adjacent text: I understand the integrals, but then the integration is done along trajectories, that is, there must be a time dimension. Where is it?

Response: We added the time dimension and limits of integration to the equation and the accompanying text.

Analysis of Fig. 4: I am puzzled by the fact that the figure and thus the entire investigation contains or considers or does not filter out ice that is present above the WCB right from the beginning. I think this ice has nothing to do with the ice formation within the WCB and I am not sure how this distorts the analysis. I stumbled over this in the SEC frame. On a previous page I read that SEC has almost no effect in this study, a statement that I find quite plausible (and I wanted to suggest to leave SEC out of the paper at all). Now I see that SEC ice is the first that appears in the WCB. But obviously it was already there and it was not produced since a WCB appears. Isn't it possible to filter these ice masses out and just to consider what was formed directly by the dynamical triggers caused by the WCB? I understand that later such pre-existing ice may fall into the affected clouds. In this case, it would perhaps be better, to introduce a SED (sedimentation) class instead of the SEC class.

Response: Figure 4 shows an exemplary trajectory of the WCB ascent. This trajectory is one out of over 40.000. Of course there can be other WCB trajectories above this single exemplary trajectory. Section 2.4 stated that secondary ice in this study is only provided from rime splintering and the secondary ice does not reach the **cirrus temperature regime** in a substantial quantity. However, in Sections 3.3 and 3.4 we do not only discuss the evolution and statistical distribution of ice modes in the cirrus temperature regime, but also in the mixed-phase regime. After all these mixed-phase clouds (mostly) transition to cirrus clouds. So in order to investigate the characteristics of the WCB outflow cirrus it is also of interest to the study the mixed-clouds during the WCB ascent.

Albeit WCB clouds are more commonly associated with slow updrafts, the occurrence of embedded convection and therefore of mixed-phase cloud processes typical for larger vertical velocity regimes like riming, which is a prerequisite for secondary ice from rime splintering, has been documented in a range of recent modelling (e.g., Oertel et al., 2023; Schwenk and Miltenberger, 2024) as well as observation studies (e.g. Blanchard et al. 2020).

All ice classes sediment. A Sedimentation ice mode (SED) does not make sense for a bulk scheme as it would not be unique and depend on the location of reference requiring additional diagnostics. Following the sedimentation of individual (ensemble of) particles would only be possible in Lagrangian particle models, not in bulk schemes like we use in the present study.

Line 295: How large is the excluded fraction?

Response: The mass and number fractions of secondary are less than 0.01% for the cirrus temperature regime (see Section 3.4 and Figure 5 e/f).

Lines 319/320: "After ascent...". Please explain this finding. If T and thus saturation pressures are constant (Fig. 3 c), what then reduces RH? Where does the vapour mass go?

Response: Vapor is consumed by ice crystals in depositional growth. The ice crystals sediment out of the WCB air parcels and melt or sublimate elsewhere. Additionally the WCB air mass mixes with dry air from above and around the WCB.

We added a remark in Section 3.2.

Line 404: Shouldn't it read "higher" instead of "lower" supersaturation?

Response: We corrected the mistake.

For section 4: Please add somewhere, best already in section 2, an explanation whether air parcles can change their characteristics over time, e.g. from liq.-origin parcel to in-situ-origin parcel and vice versa. Can that happen at all, in both algorithms, and if so, how often does it happen?

Response: The W16 algorithm classifies entire segments of a trajectory (so along a time dimension) as either in-situ or liquid origin. A segment in W16 is defined as the (total) cloud ice content being continuously larger than a critical value. That critical value is taken from Wernli et al. (2016). Below this critical value the cloud is to thin to be considered a cloud.

The classification of our ice modes scheme is based on the liquid-origin fraction (defined in Section 2.4). If the liquid origin fraction is larger than 0.5 it is liquid origin, or in-situ otherwise. This is a point-wise classification and thus can change with each model time step as the prognostic moments of the ice modes change due to nucleation processes, sedimentation, growth (and more…).

We added some clarifications and remainders to Sections 2.4 and 4.2..

Why do you sometimes use mass mixing ratio and sometimes volume mixing ratio?

Response: The unit kg/kg is common for publications discussing model output, where ppmv is primarily used in publications presenting results of observations since many instruments measure volume ratios. When comparing our results to climatologies we changed the units to ppmv to make it easier for us and the reader to cross-check the modeled cloud properties.
For consistency, we changed all mass mixing ratios from kg/kg to ppmv.

Comments on text and style:

Line 4: "...and ice crystals directly from water vapor". Nucleation directly from the vapour, that is, without suitable nuclei is possible at several 100% RHi, but this is probably not meant here.

Response: This phrasing is often used to express the formation of ice without a stable, intermediate liquid phase in the form of a pure liquid water droplet. The model only "sees" that vapor is directly deposited on a dry ice nucleating particle (deposition nucleation) or the freezing of a solution droplet that exists below saturation wrt pure water (often only labeled as "homogeneous nucleation"). No bulk microphysics model employed in NWP models attempts to resolve (liquid) aerosol particles directly.

We changed the phrasing to make it clearer for readers unfamiliar with this (sometimes misleading) terminology of the microphysics community.

Line 5: It is better to write vapour instead of moisture, since liquid droplets belong to moisture as well.

Response: We agree and changed the term.

Line 9: "...ice at an arbitrary location in the model..." What do you mean with "arbitrary location"? I think, "... ice in the model .." would be sufficient.

Response: We rephrased the sentence. The important part is that our ice modes microphysics schemes provides information on the formation process origin of ice particles directly without the need of additional tools for analysis, e.g., tracking all microphysical process rates (nucleation rates, deposition growth rates, sedimentation rates, collision rates...) at each time step.

Lines 9 ff: The two sentences beginning at "Our analysis..." sound contradictory. Please clarify.

Response: We reworked the abstract in an effort to improve clarity and train of thought. We also addressed the (apparent) contradiction by clearly distinguishing between the nucleation and thermodynamic perspective.

Line 18: "a frequent occurrence"? Better "occurs frequently".

Response: We changed the sentence similar to the suggestion.

Line 29: word missing

Response: We added the missing verb.

Line 46: "since pure water droplets must exist"? Better replace "must exist" with "would otherwise evaporate".

Response: We changed the sentence as suggested.

Line 56: add "of in-situ formed cirrus" after "subcategories".

Response: We added "in-situ cirrus" similar to the suggestion.

Line 65: replace the first instance of "available" with "amount of".

Response: This part of the introduction was removed in the revision of the manuscript.

Line 85: "originating from the geographic information", What do you mean?

Response: Both studies took the geographic coordinates of the in-situ measurements as starting points for the Lagrangian trajectories in the model driven by data from ECMWF reanalysis simulations. We shortened the sentences.

Line 106/7: "...classification methods ... can distort the differences..." What do you mean?

Response: We rephrased the sentence. It now reads " Therefore, classification methods only based on instantaneous data can not distinguish to what degree observed cloud properties are characteristic for the formation process and to what degree they were distorted by other cloud processes (Krämer et al., 2020). "

Line 109: "on the other side". If you use such a phrase, then you should also use the phrase "on the one hand" before (and write hand instead of side).

Response: We removed the phrase.

Line 198: Better write "region" instead of "area" when you are talking about three-dimensional things.

Response: We changed "area" to "region".

Lines 253/254: Bad sentence! It reads essentially like " The evolution of... moved over the North Atlantic". Please reformulate.

Response: We rephrased the sentence.

Line 290: "featured" should be replace by "had experienced".

Response: Rephrased the sentence as suggested.

Line 292: Another example of mixing-up model and nature! ".. tau_600 may impact ice formation ..." How? Does nature first look, which value tau_600 has and then decide?

Response: As stated in the preceding lines, tau_600 describes the (mean) vertical velocity of the ascending air parcels in the WCB. Vertical velocity is a physical quantity linked to adiabatic cooling of air parcels. When the air parcels cool, the supersaturation increases. The rate of increase in supersaturation is proportional to the cooling rate and thus vertical velocity. This directly affects the rate by which ice can grow due to deposition of vapor and also controls which ice formation processes are active, since they are also depended on temperature and supersaturation.

Line 298: Please rewrite this sentence "Forming the tail...".

Response: We rephrased the sentence.

Line 315: "below the cirrus temperature level T_c=235K" although this is formally correct, I suggest a rewording. Such expressions are prone to misunderstanding (similar to expressions involving pressure and lower or higher). It always needs a while to understand whether the "below" refers to altitude or to temperature/pressure.

Response: We rephrased the sentence.

Line 326: Please be consistent with mass mixing ratio vs. mass content, and in line 328 simply "mass". There are more of such instances later.

Response: We replaced the two remaining instances of "mass mixing ratio" in the manuscript with "mass content".

Line 330: please add "in the parcel" after "ice first occurs".

Response: As suggest we added "in the air parcel".

Line 338: replace "temperature regions" with "temperature regimes".

Response: As suggest we replaced “regions” with “regimes”.

Lines 339-343: These explanations are not entirely clear to me. In the last line, "where" should probably read "while".

Response: We removed this part of the text to shorten the length of the manuscript.

Lines 368/9: Incomplete sentence.

Response: We do not see what is missing in the sentence. We slightly rephrased the relative clause.

Line 410: what are "view trajectories"?

Response: We replaced “view” with “few”.

Figure 7, caption: "with colored by" ?

Response: We removed “with”.

Sect. 4, first par: ones -> once (several instances), also on later pages.

Response: We fixed the issue.

Line 483: "spell" is not the correct word here.

Response: We rephrased the sentence.

Line 507: Is "outline" not simply a "line"?

Response: Changed “outline” to “line”.

Line 511: incomplete sentence "thus the statistic...".

Response: We added the missing verb.

Line 524: one instance of "after" should be dropped.

Response: We rephrased the sentence.

Line 559: Sentence cannot be understood.

Response: The sentence was removed when we shortened the manuscript.

Line 632: Drop "remained".

Response: We rephrased the sentence.

Line 666/7: what is "temperature and supersaturation activation"?

Response: We rephrased the sentence.

Reviewer 2

In this manuscript, Lüttmer et al. use a novel multimodal ice microphysical scheme combined with offline trajectories to study the formation mechanisms of cirrus in the outflow of a warm conveyor belt. This allows a much more accurate estimation of microphysical sources compared to previous studies. In particular, they show that most cirrus is formed by in situ ice nucleation at T<-35°C, which differs from the results of a commonly adapted method used in several previous studies.

This is a valuable study that adds an important perspective on the microphysical properties of ice clouds in warm conveyor belts. This will be a valuable contribution to the community. However, I have several points that I would like to see clarified/addressed before acceptance. Many of my general comments relate to the writing. The manuscript, especially the second part, could be tightened up to help the reader. Some more storytelling that would connect the initial results to those presented in later parts of the publication might help, as well as possibly a summary sketch.

General comments:

1. My main takeaways from the paper are the following:

   I. Most cirrus are formed by in situ ice nucleation

   II. Sedimentation of ice crystals is very important and affects the properties of WCB cirrus.

   III. Beware of the W16 classification criterion, microphysically it doesn't make that much sense.

I think point II. could be better clarified in the abstract and the final discussion.

More importantly, while the study makes a detailed comparison with the W16 criterion, there is no discussion about it. The authors give no guidance to the reader about it.

More specifically, suppose we are planning a new field campaign. Should we rely on W16 for our cirrus origin classification? If not, what should we do? Such brief comment would be an important manuscript's message that largely increases the manuscript's relevance.

Response: While the W16 criterion may not be suitable to provide a comprehensive differentiation of WCB outflow cirrus, it may still be useful (and indeed has been shown to be useful in many observational and modeling studies) to classify cirrus from a wider range of phenomena. However, this is difficult to show based solely on the one presented case-study of a WCB, which lacks presence of any “proper” in-situ cirrus, i.e. cirrus clouds whose properties have not been modified by mixed-phase cloud processes. Therefore we tried to avoid to strong statements about the overall validity of the W16 classification scheme. However, we agree with the reviewer that we may have been a bit over-cautious in this respect and leave too much of the interpretation and wider meaning of our results to the reader. Therefore we have added some discussion along these lines in the discussion.

For field campaign planning, the W16 scheme may still provide valuable insight, in particular as it is relatively cheap to obtain (and therefore possible for near-realtime applications), whereas simulations with a modified microphysics scheme are usually too costly and the cost-benefit ratio may not be too good. What the present study suggests, is that one should be careful with over-interpreting the W16 results in the vicinity of deep and complex cloud systems.

2. This study focuses on a single WCB. Can the results be generalized, as is more or less implied in the text? Is the variability between different WCBs and their cirrus formation/properties large? For example, would the cirrus of a very strong WCB be very different from that of a rather weak WCB?

Response: Climatological studies (e.g. Binder et al. 2020) show that the general cloud structure of a WCB is similar in the sense, that there is often a thick mixed-phase cloud in the middle troposphere, a derived cirrus cloud in the upper-part of the troposphere, and a (likely) in-situ formed cirrus shield on top. This overall structure suggests that in most WCBs sedimentation of ice from the in-situ cirrus will impact cloud evolution in the mixed-phase region. However, the details of the cloud microphysical structure will vary depending e.g. on the amount of embedded convection, which alters the vertical velocity structure and therefore downward propagation of in-situ ice crystals as well as the altitude at which mixed-phase derived ice is detrained into the cirrus shield (e.g. Oertel et al. 2023; Schwenk and Miltenberger, 2024). In addition, the importance of sedimentation will vary over the WCB lifecycle as well as the geometrical configuration of WCB ascent and outflow as well as overlying in-situ cirrus. While preliminary results suggest that the amount of embedded convection depends on the zonal position, underlying surface (land vs. ocean) and season, the variations in geometry of WCB configurations are not yet well understood.

In this context, it may be further to point out that the results shown here are based purely on model results with all the known uncertainties in the representation of ice- and mixed-phase cloud physics. Again, we think that the qualitative conclusion about the importance of sedimentation is physically meaningful and not contingent on the particular model used, the quantitative conclusions may be different if other model (versions) are used and therefore not easily generalizable without further studies.

We have clarified the manuscript by highlighting the qualitative conclusion on the role of sedimentation, while also cautioning against directly applying the results in a quantitative manner to other WCB case studies.

3. The title could better indicate the main result. The title alone implies to me a study that compares the importance of ice formation processes with other processes (e.g. large-scale dynamics, turbulence, etc.) in determining cirrus properties. I guess the key finding is that cirrus is formed by in situ ice nucleation. This could go in the title.

Response: We changed the title to: “On the impact of ice formation processes and sedimentation on cirrus origin classification in WCB outflow”.

4. The manuscript could be shortened and streamlined in places to allow the reader to get to the last part of the results, which give unique insights into microphysics that you can't get with a standard microphysical scheme. Mentioning/reminding the reader of this in a few places may help.

Response: Thank you fort he comment, we have carefully re-read the manuscript and have shortened Sections 1., 4.3 and 5. As discussed in another comment, we included a short section highlighting the benefits of the ice mode scheme for investigation of ice formation processes, but otherwise refer to the companion paper Lüttmer et al. (2025a).

Minor: It is more appropriate to use the term "cloud radiative effect" rather than "cloud radiative forcing". CRF has been used a lot in the past, but since the word "forcing" implies a perturbation in the climate system, the community has moved to using CRE instead, since clouds are an internal component of the climate system.

Response: We thank the reviewer for this comment. Indeed, we are only interested in the radiative properties of clouds and not in perturbations of the climate system. We changed all instances of CRF to CRE.

Specific comments:

Line 6: I would suggest simplifying the sentence beginning with "A priori" to something like: The dominant ice formation pathway remains uncertain.

Response: We changed the sentence as suggested.

Lines 10-14: I think these sentences could be improved, the message could be expressed in a more direct, clearer way.

Response: We rephrased the abstract in an effort to improve clarity and train of thought. However, we are limited by the maximum word count for abstracts in ACP.

Line 20: Do we know what fraction of the total mid-latitude cirrus comes from WCB? Have any studies looked at this? If determining radiative forcing is a broader goal here, it would be important to know.

Response: The only study we are aware of is the study by Wernli et al (2016), which however is based on ERA-Interim data. They estimate the directly WCB ascent related cirrus clouds to account for 3% of all cirrus clouds at 300 hPa over the North Atlantic. This reduces relatively rapidly decreases for higher and lower altitudes. However, Wernli et al. (2016) use a relatively strict WCB criterion and more recent studies have shown that WCB related cloud formation occurs in a larger air mass surrounding the core ascent (Heitmann et al. 2024). Therefore the 3% in Wernli et al. (2016) should be viewed as a lower limit.

Lines 28-38: It would be good to add references to some satellite studies on impacts of cirrus on CRE. E.g. Hong et al., 2016 (10.1175/JCLI-D-15-0799.1), or Matus and L'Ecuyer, 2017 (10.1002/2016JD025951).

Response: We added the suggested references.

Line 60: Insolation is at least as important in determining their CRE as their altitude (and thus temperature). Don't forget that the CRE also depends a lot on the surface albedo and the presence/absence of lower lying clouds, e.g. clouds over a snowy landscape will have a very low SW CRE.

Response: We added a remark concerning insolation.

Lines 61-67: Since the intro is already quite long (but nicely written), I would suggest removing this paragraph. It goes way beyond the work presented here.

Response: As suggested we removed the paragraph. A discussion about the importance of ice particle shape is not relevant in this work where the shape of ice crystals is fixed and their radiative properties is not discussed in any detail.

Line 70: Remove the parentheses

Response: We removed the parentheses.

Line 81: What is analysis data? I would just stick to reanalysis there.

Response: As suggested we replaced "(re-)analysis" with "reanalysis".

Line 100: Since Krämer et al. mainly used the previously processed satellite data, it would be more fair to cite the work of Sourdeval et al., 2018 (10.5194/acp-18-14327-2018) and Gryspeerdt et al., 2018 (10.5194/acp-2018-20) here.

Response: We rephrased this part of the introduction to make it clear who processed the satellite data and who made the climatology and cirrus classification by comparison with in-situ measurements.

Line 126: Don't most 3D Eulerian atmospheric models account for sedimentation?

Response: Most microphysics schemes employed in 3D Eulerian atmospheric models include sedimentation. However, the analysis of the microphysical data is often done on air parcels moving along Lagrangian trajectories. Here the usual methods can not decern if the ice inside the parcel stems form sedimentation or formed inside the parcel. Also most microphysical box-models, like e.g. CLaMS-Ice, do not include sedimentation yet.

Beginning of section 2.3: Could the authors explicitly write whether they are using offline or online trajectories (this becomes clear later in the text, but not immediately).

Response: We added a clarification that the trajectories are calculated offline.

Section 2.3: It may be helpful to add some more schematic figure/sketch/illustration of the WCB forward and above WCB backward trajectories. A sketch could also highlight cirrus clouds.

Response: Figures 2 (a,c) already shows exemplary trajectories of the altitude showcasing the flow of the ascending WCB and the air mass above. Especially for the WCB, the increase in altitude also indicates the direction of the flow. Cirrus clouds occur both during the ascent phase and in the outflow. Marking them with highlights would not be very insightful.

Lines 221, 227, 228: "...integrated mass (number) ratios..." implies that mass is synonymous with number. Please rewrite, e.g. mass and number.

Response: We changed the phrasing as suggested.

Line 222: If the ultimate goal is to study radiation effects, some sort of 2D area normalization would also be useful.

Response: This is a good suggestion for a subsequent study that investigates radiation effects more closely. However, in this study we are almost exclusively focusing on the microphysical composition of clouds containing the ice phase.

Line 225: This should come at the beginning of section 2.4, but I would not use the term "completely glaciated" for clouds that can only be composed of ice.

Response: We replaced “completely glaciated” with “pure ice clouds”.

Line 226: "Sub cirrus clouds" sounds a bit too abstract to me. What about fully glaciated mixed phase clouds? That's a term that has already appeared in the literature.

Response: We agree that the term ‘sub cirrus clouds’ is an unusual choice. However, “fully glaciated mixed phase clouds” is problematic since it implies that clouds in this regime formed from a mixed-phase cloud. However, they could have also formed from in-situ nucleation below water saturation or stem from ice that formed in-situ and sedimented from the cirrus regime. We therefore keep our wording.

Line 244: "is being used" => are used

Response: We rephrased the sentence.

Figure 1: I would prefer a perceptually uniform colormap here (which is also an EGU journal requirement). Also, areas with no satellite data could be shaded gray to distinguish them from areas with no clouds, which appear as white.

Response: We changed the colormap to a uniform white-blue color scheme.

Differentiating between areas where no satellite data is unavailable and cloud free areas in gray and white shading, respectively, is a good idea. However, the satellite data files, that we have access to, do not contain a flag for ‘no data available’. It has the same value as cloud free areas. So we removed the mask for the model data.

Figure 2:
I. Is there a reason for a jump in color from blue to yellow at q_tot of 1e-5?
II. The short descriptions under a-d could be more descriptive.
III. When I look at these plots, I think of a WCB breaking out of a prison cell (or a WCB in a prison, for Figure 1). I would slightly reduce the visibility and/or number of gridlines.
IV. Would it make sense to mark t_out could on the plotted trajectories?

Response:

I.: We changed the color map to a uniform green-based colormap without jumps in color (except for q_crit = 1e-7 as values below q_crit are considered as insignificant). We also changed the units to ppmv for panels (b,d).

II.: We expanded the label descriptions and incorporated them into the plots as titles.

III.: We reduced the number of gridlines.

IV. We do not think that such markers would be distinctly visible. We are only plotting 5% of all trajectories (~4000) and they are already overlaying each other and hiding the ones beneath.

Figure 3: Tau was earlier used for cloud optical depth, here for the ascent timescale. I would prefer to call it t_600, also for consistency with t_out.

Response: tau_600 is a common label for the ascent timescale in studies focusing on WCB ascent. While we refer to tau_600 often, the cloud optical depth is only mentioned in section 2.5. Hence, we believe it improves readability to use the same label in this work. We renamed tau to to tau_opt for improved clarity.

Line 278: Why is the intersection with WCB trajectories needed for the selection of "above WCB" trajectories? Wouldn't even those that never intersect WCB be relevant for WCB trajectories and their properties due to ice sedimentation?

Response: This is correct. Even trajectories that do not intersect horizontally with the WCB trajectories can contribute to ice sedimenting into the WCB trajectories. However, finding a suitable selection criterion would be difficult as one would need to estimate the average vertical distance between the trajectories, the sedimentation velocities of ice and co-locate the trajectory positions in time and space. We added a comment that our (simple) selection criterion does not capture all trajectories that might contribute to the formation of ice sedimenting into the WCB, but serves as an indication if that process is important.

Figure 4: Would it make sense to show the same figure but for ICNC and/or ice radius? Perhaps in the appendix?

Response: Such a plot could of course be included in the appendix. However, since the paper is quite long already it is questionable how valuable its inclusion is if it is not discussed in the main text. The purpose of Section 3.3 is to show the vertical layering of the ice modes in an example trajectory and highlight fall streaks of sedimentating ice. For that purpose focusing on mass mixing ratios is sufficient.

Lines 333, 334: I think it's Figure 4c, not 4b, and Figure 4d, not 4c

Response: We corrected the labels in the text and updated Figure 4 with a new uniform colormap and units for mass content consistent with the rest of the manuscript.

Figure 5: Panel a colors are the same as in other panels, but show different quantities. Please change them! Then we will only need 2 legends, not one for each plot.

Response: We changed the colors of panel (a) and removed superfluous legends.

Figure 5: I don't think it's necessary to show illegible very small numbers (e.g. 0.00 or 0.02). Maybe just state that anything smaller than a certain threshold, e.g. 0.05, is not printed in the plot?

Response: As suggested we have hidden labels for values lower than 0.05.

General comment: I would like to make it very clear that DEPO is deposition nucleation at cirrus temperatures. Many microphysical schemes have legacy code that (unjustifiably) uses mixed phase deposition freezing.

Response: Deposition nucleation can occur at temperatures above the cirrus temperature regime (235K). However, it is correct that in the model deposition nucleation can not be triggered in mixed-phase clouds ((qc+qr) > 0). We added a remark in the Section 2.2.

Line 425: I think this can be linked to the results in Fig. 5.

Response: They can be used as an indication to explain the presence of DEP ice in Figures 4. and 5.. We added a sentence in this section to underline this point.

Figure 6d: FRZ only forms at 235 K, right? So the distribution around that temperature is caused by sedimentation and vertical transport, right?

Response: Homogeneous freezing of cloud droplets (FRZ) can occur at temperatures as high as 243K and is based on the homogeneous freezing rate parameterisation of Jeffrey and Austin (1997). At and below 235K the homogeneous freezing rate is so large that (pure) liquid droplets freeze instantaneously. Most of the FRZ ice content shown in Figure 6d is low. It is likely transported there by vertical advection and sedimentation.

Figure 6e, lines 431-432: Why is there no/little change in total cloud ice content at T>230 K? (different from the comment). What should we expect based on the deposition growth equation/theory? At what temperatures is the total cloud ice content consistent with this, and when is it not? If not, is sedimentation the reason for the deviations? Or vertical transport?

Response: Some form of simplified deposition theory to estimate cloud ice mass content can not be employed here. The relaxation time scale of deposition (~0.1s) is much smaller than the physics model time step (40s). The model uses a semi-analytic approach to estimate the depositionial growth rate with an exponential relaxation towards equilibrium (see, e.g., Morrison et al. (2005) and the companion paper). Within the same physics time step sedimentation, transport, mixing and other microphysic processes affect the prognostic moments of the ice modes. It is not possible to see this information in a Figure that includes data from many trajectories sampling the model gridded data for up to 48h over a large model domain.

There is also nothing special about the 230 K threshold in this model. In-situ ice nucleation can occur at higher and lower temperatures. In fact, the heterogeneous ice nucleation scheme used in this study (Hande et al. 2015) nucleates ice from deposition nucleation even at higher temperatures (and compared to other schemes lower supersaturation). The number concentration of nucleated ice crystals at low supersaturation is low. However, for that reason they have enough supersaturation available to grow to large mean ice crystal sizes and sediment quickly. The sensitivity of the model results to the heterogeneous ice nucleation scheme will be investigated in a follow-up study.

Figure 6f: may be more effective with a linear x axis

Response: We changed the x-axis of Figure 6 (f) to a linear scale as suggested.

Line 434: "...ice derived mixed-phase clouds" sounds odd

Response: We agree and rephrased the sentence.

Lines 437-438: I see 6-8 cm/s and 10 cm/s from the plots. Please look at this!

Response: We checked the values and corrected the text.

General comment on section 3.5 and other results: It would be good to better point out what this novel scheme can do that other schemes cannot; what insights we get because of the multiple ice modes?

Response: The novel scheme can distinguish between ice formed via different pathways. Thus, the formation signature is available even long time after the formation process. Although this approach was used for former models (e.g. Spichtinger & Gierens, 2009a), for NWP models this approach is completely new.

We added a short remark to Section 3.3, where the ice modes are first shown in a Figure, illustrating the different applications of the schemes. For a more detailed comparison we refer to the companion paper Lüttmer et al. (2025a) since such comparisons are not the focus of this study.

Line 510: evaporation => sublimation

Response: We corrected the wrong term.

After section 4.2 or so, the reader starts to get saturated. Shortening the text would help, as would turning parts of the text more into a story that refers to not just one but several figures. Examples:

1. Section 4.3 starts with an introductory paragraph describing what will be shown. I don't think that's bad, but I also don't think we need such a long intro paragraph for every results subsection.

2. Section 4.3 is very long, could be shortened.

Response:

1. There is trade off between sections being shorter or more self explanatory. For readers that prefer to read and digest the entire publication in one go, the shorter versions with less repetitions and paragraphs that re-introduce or cross-reference to previous sections are preferable. However, there will also be readers that prefer to read a publication in parts or only read the few sections that might be of interest to them. For those readers these introductory paragraphs are beneficial and add structure to the text.

We shortened the introductory paragraph.

2. We removed and shortened some sentences in Sections 4.2 and 4.3. Section 4.3 got reformatted into subsections to improve readability. We also removed the Figure in Appendix B2 and references to it to shorten the manuscript.

Lines 555-558: So the sedimentation rate of the overlying cirrus (and, I assume, relatively small) ice crystals is greater than the WCB rise. Is this correct? And what temperature range are we talking about here?

Response: As we have discussed in Section 3.5 ice from the overlying cirrus is actually quite large with a median of ice crystal size above 100 µm at temperatures higher than the homogeneous freezing level (235K). Deposition nucleation only nucleates a low number concentration of ice compared to homogeneous nucleation (homogeneous freezing of solution droplets). Hence, for these (few) ice crystals there is sufficient vapor available to grow large and sediment with high terminal velocities. We also saw evidence of these large ice crystals stemming from deposition nucleation in statistics for mixed-phase WCB clouds (see Section 3.4).

Figures 6-8: The first figures show the ice water content in units of kg/kg, here the units are changed to ppmv. It would be good to be consistent, unless there is a strong reason for the unit change (which should be mentioned).

Response: The unit kg/kg is common for publications discussing model output, where ppmv is primarily used in publications presenting results of observations since many instruments measure volume ratios. When comparing our results to climatologies we changed the units to ppmv to make it easier for us and the reader to cross-check the modeled cloud properties.

For consistency, we changed all mass mixing ratios from kg/kg to ppmv.

Section 4.3: Is it really useful to compare a WCB with climatologies?

Response: As discussed in the introduction, characterization of liquid-origin and in-situ cirrus is based on evidence from observations. Statistically, these cirrus types show different microphysical properties with regards to ice mass mixing ratios and number concentrations. We do not expect the results of a case study to adhere to climatologies. But they provide a frame of reference for the evaluation of the classification schemes.

A further study is planned that investigates the liquid-origin and in-situ formation pathways for an ensemble of WCB cases.

Section 4.3: Some sort of summary sketch might help guide readers who are tired at this point in the manuscript.

Response: The reformatting and shortening of Section 4.3 should help the flow of reading.

Line 644: "As discussed above, these particles..." ==> "These particles…"

Response: We shortened the sentence as suggested.

Lines 648-649: Does this make sense overall? How does it compare to the expected sedimentation rates for the ice crystals?

Response: In this work we show several lines of evidence that underline the impact of sedimentation on the microphysical origin of ice in the ascending WCB. It is unclear how sedimentation rates could be estimated for complex simulations like this WCB model study, that include a bulk microphysics scheme in a large model domain. Ice is affected by several microphysical processes, e.g. collisions, vapour deposition and sublimation / melting, while sedimenting. Even for a rough estimation of the individual evolution of ice crystal ensembles, we would need a high interval of model outputs, that would need a unsustainable amount of disk space.

The ice modes scheme allows to investigate sedimentation effects indirectly without having to utilize a costly microphysics scheme like a Lagrangian particle model.

In general, there is no reason why large ice crystals formed in the upper troposphere should not be able to ‘survive’ long enough to reach the WCB clouds ascending from below.

Lines 650-: I'm missing a general message about cirrus classifications. See also General Comment #1.

Response: See our answer above to the general comments.

Section 5: How does section 5 relate to the problems outlined in the Introduction? It would be good to round this up, or simply remove some parts of the long intro, and keep the focus on the main results of this work. Microphysics, cirrus origin, and not radiative relevance. Since there is a lot of discussion about radiative relevance, I would expect this to be shown in the results, or at least speculated in the discussion.

Response: We removed references to radiative properties in Section 5 and shortened the introduction. However, discussion of radiative properties is important for putting the study in the frame of the larger research regarding cirrus formation. This is the reason we still discuss it in the introduction.

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