Answers to the reviewers of the manuscript egusphere-2023-2986

We thank the reviewers for the dedication of their time to review this manuscript. We appreciate their thoughts and willingness to engage in a discussion about the results of this study. We believe the helpful comments have substantially improved our manuscript.

General summary of changes to the manuscript:

Inspired by the comments provided by the reviewers, we decided to make some major changes to the structure of the content and add additional analysis. In the method section we have split the section 2.1 into 2 parts. One now solely includes the simulation description (2.1 Model simulations) and one includes a description of the selected data (2.2. Selected data). We moved the previous section 3.2 “Regimes of phases” into the now added section 2.2 “Selected data”. This enables a clearer overview for the reader and makes them able to more easily find the description of what data is used. Therefore, some parts have be reformulated in the new section 2.2 in contrast to the original section 3.2.

Another change is the restructuring of the results. As the WBF process has received a lot of attention and further analysis was requested, we moved the WBF section “3.4 WBF in mixed-phase clouds” up, so that it is now the second results section 3.2. To add value to the manuscript we included another figure in the WBF section and expanded the discussion accordingly. Further, section 3.1 “dominating processes in low-level clouds” has been improved by splitting the results into different cloud types. For this, figure 1 (now figure 2) was completely revised. It still shows the importance of each process but distinguishes between mixed-phase clouds (MPCs) and pure ice or liquid clouds. The discussion was adjusted to match these changes.

Fig. 5 (previously fig.3) was shortened to only include the figures we perceived as most important and an additional figure in the appendix shows further processes. The WBF process was also added to Fig. 5. The discussion for this section has been adjusted accordingly.

A supplement was added to provide further figures. It includes a flowchart of the wrapper. As the number of mircophysical processes is so large we decided not to show all processes in the results section.

Line numbers refer to the revised manuscript where the changes are not marked.

Reviewer #1 (bold is reviewer comment)

Overview and general recommendation:

This manuscript uses a new microphysics wrapper in the ICON-LEM model to extract microphysical tendencies related to low-level cloud phase in the Arctic. The authors wish to quantify the relative importance of different phase-changing processes. To simplify the analysis, all liquid and ice hydrometeors are binned together, respectively, so that only two categories are considered in the analysis. These tendencies are plotted as a function of temperature, vertical velocity, and ice saturation, and compared between polar day and polar night. The authors find that tendencies involving the vapor phase of water (e.g. evaporation, deposition, sublimation, and condensation) are especially important. The authors interpret the frequent occurrence of evaporation and deposition as evidence of the WBF process.

The approach used in this manuscript is novel and interesting. Instantaneous microphysical tendency fields are critical to mixed-phase cloud processes, but are rarely studied at this scale. The tool described and employed here gives granular insight into these processes in a valuable way. The writing is generally clear and I also find the manuscript to be well-organized. I find the results interesting, but their analysis and presentation leads to conclusions that feel vague and lack clear takeaways. Specifically, conclusions regarding the WBF process should explicitly quantify its importance as a function of other climate fields. I think that with some additional recommended analysis, the results will be more valuable to the broader community. As a result, I recommend major revisions. These concerns along with other more minor comments are included below.

Comments are formatted as:

Line number: "Text"

Specific Comment

1: "either"

Possible change: replace "either" with "both". "either" does not add to the information conveyed by the sentence since any bias must result from an over- or under-estimation of a model field.

Thank you for the hint, this was changed to "both".

6-8: "It was found that… vapour phase dominate."

The first conclusion is vague, what processes specifically vary and how so?

The second conclusion seems more important to the manuscript. Should it be listed first and highlighted?

See answer for next comment, as larger changes were made addressing multiple comments.

9-10: "Going a step further…occurrence."

The meaning and importance of this sentence is unclear to me. Wouldn't we effect the WBF effect to occur in the mixed-phase regime and to be characterized by liquid evaporation and ice deposition? Is new understanding enabled by this perspective?

Answer combined to line 6-8 and 9-10: We agree that the original presentation of the conclusions did not highlight and clarify the main findings enough. We changed this by being more explicit in the formulation as well as moving the sentence on the WBF up before the dependence on the regimes (l. 7-10)

31-32: Citation of McGraw (2023) and Shaw (2022)

I would highlight that these studies adjust both the ice nucleation and WBF processes while investigating the cloud forcing, not just the WBF process.

McGraw shows that agreement with observations can be achieved with multiple nucleation schemes if WBF is adjusted, while Shaw shows that multiple configurations of the WBF process rate and nucleation rates can be used to achieve good cloud phase agreement with observations.

We thank the reviewer for commenting on how the sentences are formulated which include these citations. These citations are aimed at giving some background on the current interest and research in respect to the WBF processes. We added that the ice nucleation as well as the WBF were evaluated. l. 33

33: “new: Classical Nucleation Theory”

This citation format is not familiar to me. Please review.

It is helpful that reviewer #1 pointed out this cause for confusion. We originally had added information on the new setup used in that study but removed this now as it seems to distract from the main message of this sentence. This is meant as an example for a case where a model overestimates the liquid amount.

53: “making the location…clouds.”

Given later comments about the importance of surface type, can you comment on representativeness of Svalbard to the Arctic as a whole?

The representativeness of Svalbard is indeed a topic worth discussing and ongoing work of colleagues at our institute is looking into this. Generally, one can say that these low-level clouds are a feature observed all over the Arctic. The processes occurring in these clouds, e.g. downdrafts/updrafts, composition, liquid amount, etc… have been observed during campaigns in other Arctic locations and are similar to those in Svalbard. A discussion on this is given in Gierens et al. (2020, https://doi.org/10.5194/acp-20-3459-2020). The big difference is here that we are dealing with orography and have little sea ice directly at this location. This impacts the boundary layer structure as for instance the air over the fjord is generally more humid than over the land (this is for instance shown in the cited paper Kiszler et al. 2023). At the same time we are focusing on microphysical processes and are being clear that what we find is valid for the conditions at Ny-Alesund. This is definitely to an extent transferable to other locations although the statistics would probably look different in respect to the lifetime of a cloud. We believe it would be valuable to continue this research and to look at several Arctic sites but that would go beyond our current study.

53: “ICON-LEM”

Please introduce this acronym.

This is now corrected in l: 56

62 (and elsewhere): “approx.”

This does not need to be abbreviated.

This has been changed in all cases.

64-65: “The forcing is…ICON-NWP runs.”

For those unfamiliar with this model: Within the model domain, what boundary conditions are applied vs. determined by the model?

e.g. Greenhouse gas concentrations, surface temperature/type/fluxes, radiation fields at model boundaries.

The ICON-LEM setup runs on a limited area which is forced using boundary and initial conditions from the ICON-NWP simulations (2.4 km resolution). This includes surface variables. Our limited

area ICON-NWP simulations are forced using global operational ICON-NWP simulations from the German Weather Service (13 km resolution). The simulation workflow is further described in the cited paper by Kiszler et al. (2023) where we evaluated the performance of our setup. To clarify the setup without the reader having to look it up, the following reformulation was made in l. 77 cont.:

“The initial and boundary conditions for each ICON-LEM limited area simulation are provided by an ICON-NWP simulation with 2.4~km resolution. This ICON-NWP simulation covers a larger domain and is forced by the operational German weather service global ICON-NWP runs.”

61-72 (entire paragraph)

Has this model/configuration been evaluated in its ability to capture MPCs and their radiative effects before? A simple, general overview of the model's performance would be useful here if available.

Convince the readers that this model is an appropriate tool for this study (e.g. fit for task). If the model has biases or limitations, how may they affect the conclusions of this study?

The reviewer would find a short overview of the model performance beneficial. Such a model performance evaluation has been performed for this location by Schemann and Ebell (2020) and Kiszler et al. (2023). We agree that it would be helpful to summarize their findings. Therefore, we expanded this paragraph and added the following information which also adds to the understanding of why we are so strongly interested in the phase-partitioning:

l. 65 cont. “The general setup follows the papers by Kiszler et al. (2023) and Schemann and Ebell (2020) and a thorough evaluation of the model performance is provided in those studies. While the general performance of the model was found to be very good, there were some short comings. There it is shown that the cloud occurrence matches the observations well but that the occurrence of liquid containing clouds is underestimated by around 30%. ”

75: “10-8 kg/kg”; Later in the manuscript a threshold is described as 10^-18 kg/kg. Are these different thresholds or is it an error in the text?

We thank the reviewer for pointing out this potential for confusion. These are two different thresholds, one for the hydrometeor mass and one for the hydrometeor mass change (process tendencies). To avoid confusions the following sentence was added in l. 137 “This threshold is much lower than the threshold for the hydrometeor mass (10^{-8}~kg\;kg^{-1}) as the microphysical process tendencies, changing the mass, can be very small.”

89-94: Entire paragraph; How is the model vertical coordinate handled when identifying clouds and microphysical tendencies? Are outputs also produced on a vertical grid for cells where a cloud is present?

The tendencies are computed for all grid cells on the same vertical grid as in the model. This is done independently of whether there is a cloud or not. The difference is that if there is no hydrometeor mass yet, and the saturation does not allow the formation of any, then the process tendencies will just be 0 (or very very small which is then the numerical noise which needs to be removed).

As the reviewer indicates an unclear point here the following information was added to the text in l. 122 “and on the same vertical grid as used in the model.”

96-97: “This approach…single processes.”, Are the rates produced by this approach identical to those interactively seen by the model during the run?

The reviewer points towards an important aspect of using a diagnostic tool instead of the direct output. In general the same functions run as in the model, so given that the input to the function is the same in both the model simulations as well as in the wrapper, the same result will be produced. The difference is that in the model simulations also other physics and dynamics run during one timestep. This means that the input to the microphysical functions will be slightly different in the model than in the wrapper as we do not run other processes during our timestep. This does not necessarily affect our results much as we know exactly what input we provide to the microphysical functions and we are not interested in the other dynamical processes during one timestep.

98-101: “Another advantage…schemes have run.”; This is a very important note. So the saved output is not merely a subset for MPCs, it is taken during a different part of the model updating loop? A flowchart figure showing this could be really helpful for a reader!

The reviewer understandably has further questions regarding the way the wrapper works. To improve the clarity we followed the suggestion to add a flowchart. This has been added in the appendix (Appendix A, Fig. A1)

112-114: “In this study…compensate each other.”; Does this simplification create any potential shortcomings in the analysis? E.g. Phase partitioning is also dependent on sources and sinks of hydrometeors, so conversion of cloud ice to graupel/hail could quickly deplete ice and modify the phase partitioning.

We appreciate the reviewer for taking the time to think through our methods and results. We agree that this could be a relevant aspect as an increase in fall velocity would diminish the amount of ice as stated if it precipitates. As we focused on the general ice and liquid mass using this simplification makes it clear how each phase is directly impacted. At the same time we do not explore the precipitation rate here and would rather suggest to create a separate study looking into different microphysical pathways. Such a study would rather take the form of the work by Barrett and Hoose (2023) and potentially one could include actual observations and forward simulations but that is not something we wanted to achieve here.

128: “making the frozen mass increase due to riming smaller.”; meaning of "smaller" is unclear.

We thank the reviewer for making it clear that it is not necessarily understandable what is meant by the enhanced melting. To clarify this we changed the text in l. 155

“If T$>$0\;°C enhanced melting after riming will take place, making the frozen mass increase due to riming less as not all liquid will freeze onto the frozen hydrometeors.”

141-142: One can see…frozen hydrometeors.”; A clear example could make the meaning of this statement more clear.

This part has been changed substantially. The figure which is meant in this sentence has changed and as described in the general summary up top the results section related to this were rewritten. We hope the changes of section 3.1 make the results more clear now.

148-149: During the polar day…less frequent.”

The process rates seem to depend heavily on the mean state of the cloud phase. Does this present a chicken-egg problem? i.e. Is the cloud phase the result of the process rates or are the process rates the result of the cloud phase? This may be more of a question for the discussion, but I think it is important to address.

Also, do the rates generally balance each other? If sublimation decreases during the day does a vapor to condensate or ice transition also decrease to maintain balance?

If equilibrium is not achieved, is this the result of ignoring advection?

I guess what I am asking here is what does the budget for vapor, liquid, and ice look like using this analysis? I think that this would be a helpful way to visualize the balance of processes at play and also to understand processes that may be missing from this analysis (i.e. a not-closed budget implies important roles from advection, etc).

We are grateful that the reviewer has thought about the meaning of these results. As mentioned before, this section has changed quite a bit but these question still remain relevant. In the manuscript l. 129 states that “One must keep in mind that any transportation (advection and precipitation) of hydrometeors cannot be included as the model itself is not run.”. The reviewer therefore assumes correctly that the mass balance cannot be closed here by simply looking at the mircophysical process rates. What we have done to estimate how much advection plays a role is to compare the hydrometeor masses of each timestep between the wrapper and the model run. This showed us that generally below 10% of the mass change cannot be explained by the microphysical processes alone. The following figure 1 shows this as an example for a single case study.

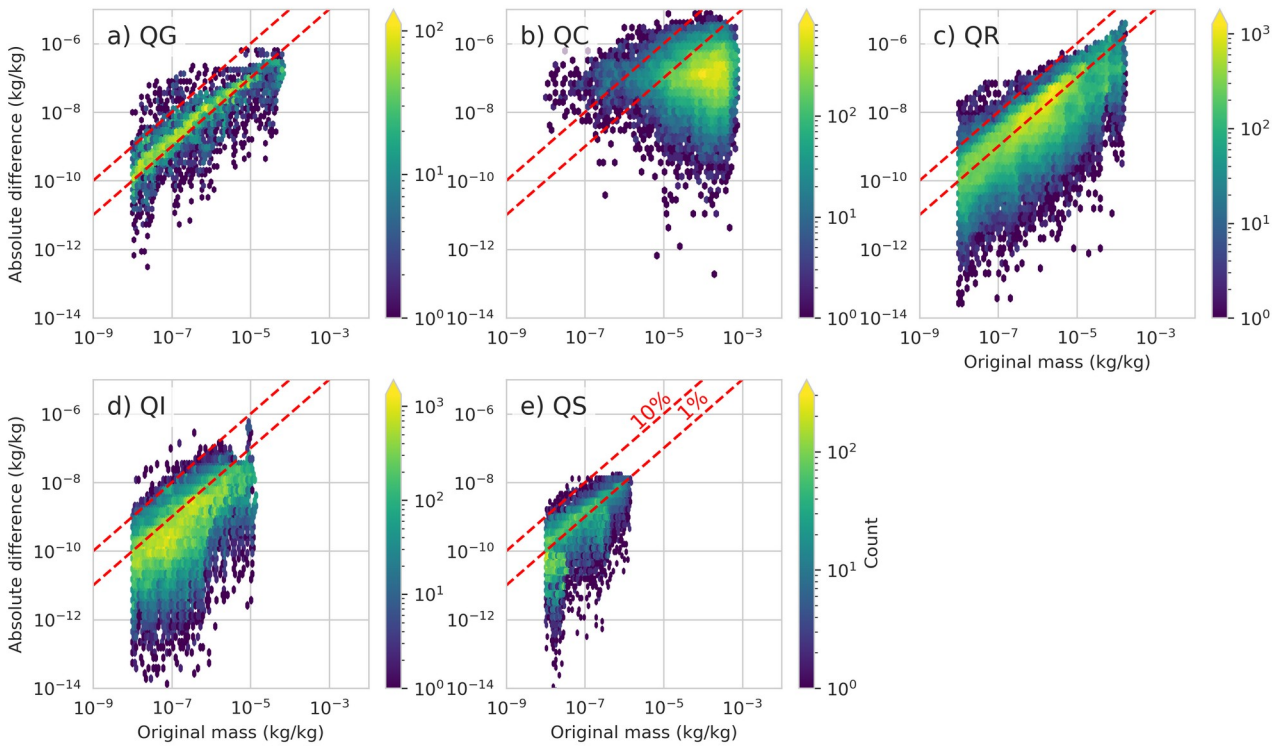

Figure 1: Absolute difference of the wrapper and original simulation plotted against the original mass simulated by ICON-LEM. The hydrometeors are a) graupel, b) cloud droplets, c) rain, d) cloud ice and e) snow. The data includes 3 hours with a timestep of 3 seconds on 150 levels. The number of occurrences per bin is colour-coded, but as the ranges are different, the subplots use different colourbar ranges. The red dashed lines indicate the relative differences of 10% and 1%. (Taken from PhD thesis: Kiszler T. Improving our understanding of cloud phase-partitioning usning long-term cloud-resolving simulations of Svalbard. 2024)

158: This is likely…is used here."

See question about the representativeness of results above. How well can the results be generalized for the entire Arctic if most is ocean? Could you repeat the analysis for an ocean gridcell to see if the conclusions change?

This comment picks up the topic of a question before and we would like to refer to the answer of the question further up.

Would you expect the conclusions to change with a different surface type?

We would expect different conclusions to the point that in other locations the clouds may be in the process of forming in stead of decaying as they are often here. At the same time the physics will be the same and for a given thermodynamic conditions we would expect the same statistical behaviour. This would presumably be independent of the surface type. What may happen is that for instance the temperature, vertical velocity and saturation distribution would look different affecting potentially which processes dominate and therefore changing how fig. 2 would look. This would indeed be worth looking at in a study with a different focus as the interactions of the processes may also change.

162-163: "while processes…not as relevant."

I would consider clarifying where the WBF process falls here. I assume that it is not considered a liquid to frozen process because in the model there is an intermediate vapor phase step. But in some models the WBF process directly shifts liquid water to ice, right?

We appreciate the reviewers interest in understanding more about the WBF process. Therefore, we computed the WBF tendency using the deposition and evaporation and added it to the analysis (Fig. 2 WBF added as process). To improve the analysis we also split the cloud types to indicate what role each process plays in specific cloud types. Further, we expanded the section focusing on the WBF process by adding further analysis.

165-166: To get an…the hydrometeors."

Would you consider also looking at the process rates as a function of the vertical coordinate? E.g. height above cloud base or optical depth?

What about the vertical coordinate? Given the strong vertical phase partitioning of mixed-phase clouds, I might expect a very different balance of process rates between liquid cloudtops and the icier cloud interiors.

We agree that this is an interesting question and from single cases we could see that there is a dependency on the location in the cloud. For instance evaporation occurs more often in lower areas where there is precipitation. As we focused on adding further analysis regarding the WBF and improving/refining the existing results, we have not added more results in this direction.

177-180: "Of the total…31% contained ice."

This is really interesting! Could you include histograms of liquid water content and ice water content for polar day and night as well?

It is good to hear that the reviewer finds our results interesting. Considering that the length of the current revised manuscript already has increased quite a bit we have decided not to add further figures.

201-202: "The homogeneous… further included."

Are the statistics bad? I would be interested in seeing these values plotted if they are interpretable. As you previously noted, the mass tendencies from these processes are small, but the number tendencies are very important.

Nucleation is indeed seldom but we show it now for the temperature dependence in the appendix in Fig. C1. To evaluate these processes it would make more sense to use the number concentrations as metric, which we do not have as a wrapper output currently and it would require a large effort to add this in the implementation of the wrapper and then rerun the processing. It would though, in future work, be worth investigating especially in the context of the aerosol impact.

Figure 3:

Plots in the left column would be easier to read with more standard temperature increments (e.g. 5C) and ticks.

This has been changed in the now figure 5.

220-221: "Reasons for…temperature range."

I think these are good hypotheses, especially given previous arguments that PD/PN differences result in part from differences in temperature and cloud phase. To test this hypothesis, you could plot 2-d histograms of deposition frequency and tendency as a function of both vertical velocity and temperature.

This is a good idea and we explored this, but did not find a clear signal in the 2d histograms we made.

223: "Ny-Å"

Abbreviation is not needed.

This is corrected.

225-226: "For the liquid mass…upward motion."

Could you include these described results as supplementary figures?

We have modified this paragraph and included evaporation and condensation with vertical velocity in the appendix Fig. C2.

240: "The introduction mentions that"

Remove this text.

This paragraph was rephrased and that part removed.

241-242: "As shown in…underestimated."; This evaluation of ICON-LEM should be included in the introduction so readers have an understanding of the performance of the model and if it is fit for task.; Underestimated by how much? Can the model be trusted for this study? See fit-for-task comment in the methods.

This comment connects to an earlier one and the changes mentioned there, are aimed at also addressing this question. To be more specific about the model performance we mentioned some results which we found especially relevant here in l 68

"In Kiszler et al. (2023) it is shown that the cloud occurrence matches the observations well but that the occurrence of liquid containing clouds is underestimated by around 30%."

244-246: "The hypothesis…phase cloud."; The wording here is unclear to me. The WBF process is active when the air is saturated with respect to ice but not water, so condensation does not occur. Evaluating the role of the WBF process requires knowledge of the temperature, right?

We thank the reviewer for indicating potentially confusing wording. We have changed the paragraph to make it more clear what we are trying to look at and what we expect to see. (l. 199 to 204)

Figure 4: Recommendation for readability: Updates x-ticks in subplot (a) to be centered around zero. Matching panel b would be easiest for readers.

The figure (a) is currently centered around 0 and it is due to the way python seaborn boxplot and matplotlib deal with the xticks that it looks different. Both a and b use the same xticks range and

binning so it is unfortunate that the x-axis turns out differently. After some experimenting we decided to leave it as it is even though it is slightly suboptimal.

247-249: "was produced…deposition occurs."; What is the relative frequency of each set? i.e. How often is this simple definition of the WBF process happening when both cloud phases are present?

This is a great question and we have added the occurrence rate of the WBF process in Fig. 2 to identify the relevance of the WBF process. It occurs roughly 42% of the time in MPCs.

261: "For both…statistically significant."; Can you state what method of significance testing was used?

We use the Kruskal-Wallis test as implemented in Python scipy. Information added in l. 221 and the code can be found in the published jupyter notebook: "fig_03_04_wbf.ipynb"

263: "one-order-of-magnitude"; This is a fourfold increase, not quite an order of magnitude.

This was indeed not perfectly formulated and we have changed this in l. 222-223 to:

"When both processes occur at the same time, the average deposition rate experiences a fourfold increase ($9.8 \cdot 10^{-9}$ to $3.9 \cdot 10^{-8}$ kg kg^{-1}), while the average evaporation rate also increases by around one-order-of-magnitude ($2 \cdot 10^{-8}$ to $1.3 \cdot 10^{-7}$ kg kg^{-1})."

263-264: "increase…same order of magnitude"; If the absolute increase in evaporation is not matched by the increase in deposition, are there other important terms in the moisture budget?

Yes, not all vapour may be transferred to the ice state. It could be a question of advection and the saturation adjustment is called before and after the deposition routine while the warm microphysics are only called after the deposition, these can all impact the amount of vapour available for evaporation. Therefore, it is possible that more evaporation takes place than what deposits in that timestep.

240-267: Section on the WBF process.

I think that there is an excellent opportunity here to explicitly quantify the importance of the WBF process and evaluate it as a function of the variables used previously (vertical velocity, temperature, etc). Similar to the methods used to separate the different cases, you could calculate a simple estimate of liquid-to-ice flux due to the WBF process by taking the minimum of the evaporation and deposition tendencies when temperature is between 0 and -38C (or using another approach if one is more valid). This new WBF tendency could be compared with the others (as in figure 1) and also plotted as a function of vertical wind speed, temperature, and saturation. Additionally, you could compare this WBF tendency to the sum of all liquid-to-ice tendencies to see what fraction of liquid-to-ice transitions can be attributed to the WBF process and how that fraction changes as a function of vertical wind speed, temperature, and saturation. I think that this would allow you to quantify your results in a clear way for the audience.

In summary, I recommend:

- **Calculating a new WBF tendency as described above.**
- **Including that tendency in Figure 1.**

- **Plotting both the WBF tendency and the fraction of liquid-to-ice flux it accounts for as a function of temperature, vertical wind speed, and saturation.**

The reviewer shows great interest in the WBF process therefore, we have expanded our analysis. We have computed the WBF tendency as suggested and added it in Fig. 2 (previously Fig. 1). Additionally, we have expanded the analysis to increase the understanding of the WBF process and to quantify better when it occurs and under what circumstances. This analysis could further be expanded but we believe the provided additional results add significant value to this study. Regarding the change of the liquid-to-ice fraction, it would be necessary to compute the ice and liquid mass directly before and after the evaporation and deposition and combine this. This is not so trivial because these functions are called at different times in the code and other processes are called in between. Therefore, one does not have the WBF process directly and cannot directly asses the changes of the mass fractions. This is the reason we added an evaluation of what SLF regimes it occurs in to see whether there was a dependency in this respect. We also added the WBF in Fig. 5 and 7.

Figure 6:

Suggestions:

- Use consistent labelling of the sets in the figure legends.

- Check if colors in panel (a) are colorblind friendly.

- Report the fractional occurrence of each set in the figure label.

We thank the reviewer for the suggestions and have considered them for the now Fig. 3. The labeling is created in a way that it is clear what data is included in the subsets. The reason why we do not just use “no WBF” and “WBF” is because subplot a) shows a different sub-selection than b) and c). This is also indicated by the different color selection. The colors had been put through an online colourblind friendly check prior to submission, and we try to use different symbols, transparencies and line types in all plots where multiple colors are used. The choice may still be faulty as also an online simulator can only go so far. If the reviewer finds the colours hard to distinguish we will change them and would appreciate a hint what colours might be a better choice as the online tool seems not to be sufficient in every case. Regarding the occurrence, this is a good thought and we added it in the text as well as in the figure caption.

276-278: “The results suggest…active enough.”; This conclusion feels pretty vague, could you be more specific? Do the conclusions of this study point to one approach versus another?

This is indeed slightly vague and we have added a sentence in the WBF paragraph highlighting a potential next step to take (l. 346 cont. “Reducing the WBF rate by reducing the deposition tendency may be a way to reduce the underestimation of liquid-containing clouds found in a previous study Kiszler et al. (2023).”). The issue here is that the microphysical processes interact with each other to an extent that it is very difficult to say what will happen if one changes one process equation. We have done some small tests changing the deposition rate and this impacts almost all other process rates as well as the masses in a non-linear way. Therefore, we want to be very careful about being more concrete in this respect.

278-279: “Further…large dataset.”; What does this say about the importance of the mean atmospheric state? How does ICON-LEM's representation of the mean state influence the conclusions of this work?

The representation of the mean atmospheric state as well as boundary layer processes play an important role for the low-level clouds of the Arctic. This was also the reason why this model has been evaluated before being used to go into the details of the clouds.

280-282: “It is worth…process rates.”; So mass tendencies from nucleation processes are small but the number tendencies are important? Could this help guide model development and tuning? For example, could nucleation only influence ice and liquid number tendencies and not mass tendencies without a large effect? (just for low-level mixed-phase clouds?)

We appreciate the thoughts of the reviewer in respect to this topic. To guide model development with respect to nucleation and activation we believe it would be necessary to also look into the number concentrations. The challenge here is that growth processes need nucleated or activated particles to begin with. Looking at this would for example be worthwhile if one can use different aerosol numbers. This would be worth setting up another study though where one can look at this in detail.

305-306: “and the results suggest…frozen phase.”; This language is too vague. How much of the mass tendency occurs via the WBF process?; A simple estimate of the WBF process could be calculated as minimum(liq-to-vap,vap-to-ice) for individual timesteps.

The reviewer highlights that it would be helpful to be more explicit about the results. Therefore, we have rewritten this paragraph to a large extent to match the added results and clearly state the main findings and their potential implications in the summary. (l. 339 to 349)

318-319: “In this study…completely correct.”; I think this should be included in the introduction/model description. Are the aerosols prognostic, do they evolve in time?

The aerosols are described in the model description in l. 74-77 “We use the Segal and Khain (2006) cloud condensation nuclei (CCN) activation with maritime aerosols, as well as the heterogeneous ice nucleation from Phillips et al. (2008) with the maritime aerosol concentrations.”. As we do not look into aerosol effects, this should be sufficient for readers to find more information in case they want to investigate this topic further.

Reviewer #2

This study presents an analysis of phase-change process rates in Arctic clouds above Ny-Alesund motivated by a need to better understand phase-partitioning in mixed-phase clouds. I think that analysis of process rates can be a valuable way to understand clouds and that is something that should be done more often. That said, I have some concerns about the applicability of their results to mixed-phase clouds generally. Even aside from these concerns, I’m not sure what the author’s main conclusions are. The only specific conclusion in the abstract is that “the importance of a process varies for the polar night and polar day … phase changes that involve the vapour phase dominate.” I’d argue that neither of these are particularly novel results. That said, I think that with some additional analysis, the paper could provide more insight than it does in its current form.

Major Comments:

1. **Figure 1 shows that the liquid water budget is clearly not closed. The evaporation and condensation conditionally-averaged rates are essentially equal (my understanding is that averages are taken over all points that meet the minimum rate threshold), but evaporation is 4 – 8 times more frequent. That the budget is not closed is because the authors analyze rates through only a single grid column over Ny-Alesund. The results suggest then that clouds are advected over Ny-Alesund and not generally forming over Ny-Alesund. In short, Ny-Alesund does not capture the full life-cycle of clouds. This is a major limitation of the study, and a limitation that is not discussed by the authors. I find it very difficult to interpret the results without any sense for the underlying distribution of lifecycle stages of the clouds (and I realize that even quantifying the lifecycle is non-trivial). I think that this concern could be partly mitigated by focusing less on the tendencies in the dataset as a whole and more on subsets of data that include only clouds that meet certain criteria. For example, subset by clouds that are growing and clouds that are dissipating, or by clouds that are becoming more glaciated and clouds that are becoming less glaciated.**

We thank the reviewer for thinking through our results and have addressed this by discussing this more in depth in the second paragraph of the results section 3.1. Additionally, in the conclusions we have added this limitation explicitly. l. 370 " Specifically over Ny-Alesund which, as we found, represents more the decaying phase of clouds than the formation phase."

This said, we believe that the chosen data is still very valuable because many campaigns, long-term observations and model studies focus on Ny-Alesund. Additionally, something lacking in most discussions of the low-level clouds in this region are the process rates. This is where we can provide many meaningful insights and quantify the extent to which processes are active in this model. It is true that it would be beneficial to cover a larger spacial area, but as is known from any modeling study, then one cannot look at this high temporal resolution required for the analysis of microphysical process rates. In light of the current work done by others in and around Ny-Alesund, this study adds an important piece to the puzzle. Further, the microphysical wrapper has now been used in a first study and the next study could for instance cover a larger domain while reducing the time coverage.

1. **The authors are motivated by the need to understand phase partitioning, but aside from the WBF analysis toward the end of the manuscript, there is no explicit analysis of phase partitioning. Why not restrict analysis throughout the entire manuscript to mixed-phase clouds? And/or examine, say, tendencies of ice/liquid water fraction and identify the processes that are most important for changing this fraction? These processes may or may not be the same as the processes that are most important for the total liquid change and total ice change, if for example, two processes are well correlated and offsetting one another, or if for example, a process such as condensation is important only when a cloud is predominantly liquid. If the authors were to go down that route, I think it would be useful to additionally include sedimentation fluxes in and out of grid boxes since that is also a process that could change the local partitioning. It could also be interesting to examine the phase partitioning processes as a function of height in the cloud – perhaps phase-partitioning processes important near cloud top are not important in the precipitating regions of the cloud and vice versa.**

We appreciate the critical feedback and have addressed this by adjusting our analysis to directly differentiate between mixed-phase and single-phase clouds from the beginning of the results. One must add that any mixed-phase process such as riming or rain freezing will by default only occur in mixed-phase clouds so already in the first version such analysis was included. We have made this distinction clearer now and have further added insights into the WBF process. This led us to redo fig. 2 and rewrite large parts of the WBF results, adding further findings (also in fig. 5 and 7). We see that the vertical distributions in the clouds are an interesting point but have focused on adding value to the existing results. In fig. 4 one can already get an idea though as we used the temperature distribution to evaluate the WBF in connection to other processes. In respect of the liquid-ice fraction we are limited by the output and microphysical implementation in the ability to exactly say what the change to the fraction was after each process. That is mainly because the saturation adjustment is called twice, once before and once after the warm and cold microphysics. This makes it difficult to retrace at what point the ice-liquid fraction changed as we had to find out while trying to evaluate this. As this would indeed be interesting to see though, we would suggest adding this as output for each process in the wrapper. This could be achieved in a succeeding study.

In short, I think that the analysis could have been more creative to mitigate weaknesses in the data sampling and to provide more insight into phase partitioning. I don't think the authors need to take all of my suggestions, but I think that major revisions could address these weaknesses and produce a study that more directly addresses the gap in knowledge that they identify and that will be ultimately more impactful for the community.

Minor Comments:

Line 45: I was uncertain whether "deposition rate" here referred to the deposition of snow to the ground surface, or the deposition of water vapour to the crystal surface. I think now that it is the former.

In Kalesse et al. (2016) they study the microphysical process of snow deposition rate from the WRF model as the depletion of water vapour can determine the amount of vapour which can form liquid droplets. They compute it "[…] averaged between cloud base and 200 m below cloud base where snow growth rates are largest.".

Line 98-101: I don't understand this advantage. Are you writing the thermodynamic variables as they exist immediately before the call to microphysics? If so, I don't believe that this was stated explicitly.

We thank the reviewer for pointing out that this sentence was confusing. We have removed this and the following sentence because it causes more confusion than explaining anything. We created a flowchart (Appendix A1) which hopefully helps understanding the overall way the wrapper works and what goes in and what comes out.

Line 128: confusing wording, please rephrase

We have changed this in l. 155 to: "In SB in ICON, this also includes the Hallet-Mossop secondary ice production. If T$>$0\;°C enhanced melting after riming will take place, making the frozen mass increase due to riming less as not all liquid will freeze onto the frozen hydrometeors."

Line 195: Is "increases" supposed to be "decreases"?

Yes, this is corrected.

Lines 200-204: Most of these results are not shown, is that correct? If so, please say so explicitly.

These results were indeed not shown but we decided to add them in the appendix (fig. C1) and to refer there so that the reader has the full overview. This section was also changed to incorporate the WBF process in fig. 5. and therefore the paragraph was rewritten in parts.

Lines 234-238: Are any of these results shown?

These results are not shown so we have indicated that in the text.

Line 249: Presumably "no deposition occurs" is the same thing as "sublimation occurs" since we are only examining mixed-phase clouds?

Yes, the formulation is just aimed to make the difference between the two sets clear. This paragraph was rewritten to make the wording and section content clearer.

Additional changes by the authors:

Code and data availability statement:

- Model code: The ICON code has become open source in January 2024, therefore we updated the link and added a short sentence mentioning this.
- Analysis code: The data analysis code which was used for the resubmitted manuscript version can be found under the doi 10.5281/zenodo.10945484
- Analysed data: The data has not changed so the same data set is still valid but an additional data formating script has been added in the code and can be used to store the data selection as netcdf instead of csv.

To be more consistent we now everywhere abbreviate the polar night and polar day with PN and PD, respectively.

The temperature, vertical velocity and saturation are not abbreviated by their symbols anymore (T, w, S_i/S_w), as these symbols where not really used.