Authors' answer to the review of Schäfer et al. Simulations of primary and secondary ice production during an Arctic mixed-phase cloud case from the NASCENT campaign **by anonymous referee no. 1**

This is a straightforward and thorough study which compares a series of model simulations to observations of a mixed-phase cloud to determine what changes are necessary to the microphysics scheme in order to reproduce the observed microphysical structure of the cloud. While I think that the discussion at times becomes tedious and the results could be presented more concisely, I can find no fundamental problems with the methodology or the interpretation of the results. The main takeaways of the paper are a number of recommendations to modelers for accurately representing SIP processes in Arctic clouds which I believe could be useful for the modeling community. Below are mainly suggestions for improvement and a few questions about minor points that were unclear.

Abstract: Just a suggestion for revision. Currently the abstract is about 2/3 preamble and 1/3 about the study. I would recommend putting more emphasis on the results of the study and less emphasis on the preamble.

> Thanks for the comment. Based on this and also a comment from reviewer 2, we have added a statement on the need for rime splintering to "kick off" collisional breakup/subsequent SIP to the abstract. We also removed the very first sentence to shorten the preamble part.

Lines 73-80: I think that a more direct statement of this study's objective would be useful for framing the paper. I didn't have a clear sense for where the paper was going after reading the introduction.

> Thanks for the comment. Before the sentence starting with "We are particularly interested in…" we added the sentence "The main objective of this study is to simulate ice production in the model that is in agreement with observations, both in terms of numbers and processes." This makes it clearer.

Line 130: I initially thought that Eq. 1 could be found in the given citation. Then, five lines down, Eq 1 appears awkwardly without context. I recommend moving Eq. 1 to appear immediately after it is mentioned, as is typically done with equations.

> Thanks a lot for this note. The equation is moved to directly where it is mentioned in the text.

Line 190-191: If CCNC is 9/cc for updrafts, downdrafts, and negligible, then is this simply a 1-moment version of the scheme? And if not, then what activation scheme is used if not Cohard and Pinty?

> Thank you for the comment around this potential cause of confusion. You're right, when CCNC is set to 9 cm^{-3}, regardless of the state of vertical motion, there is no activation scheme in use, but a fixed number of droplets is nucleated. However, this does not mean that the whole scheme becomes single-moment for cloud droplets. The droplet number may still be

reduced from 9 cm^{-3} through collisions with snow, graupel, hail and ice particles. These sink terms are tracked and the droplet number may vary according to that. Therefore, the total cloud droplet mass and number are still treated individually and the scheme remains two-moment.

Table 1: Does the CCN type really matter, particularly for MY_{adap}?

Thanks for the comment. Since there is no activation scheme in use in MY_{adap}, you're right, the CCN type does not matter for droplet nucleation. We removed that part from Table 1.

Line 387: "The overview over in which ..." ... please clarify.

The words "The overview over" were simply unnecessary in the beginning of the sentence. We removed them and additionally split the sentence into two sentences to make it less complicated. This part now reads:

"In which altitude region and during which flight the individual conditions and the joint condition are fulfilled is given in Table C1. This overview explains the differences in the simulated impact of the RS process during different flights well."

Authors' answer to the review of Schäfer et al. Simulations of primary and secondary ice production during an Arctic mixed-phase cloud case from the NASCENT campaign **by anonymous referee no. 2**

Summary: This study is built around a modelling case study from a field campaign that investigated secondary ice production in arctic clouds. The authors ran three types of simulations with WRF with two commonly used microphysics schemes: 1) control experiments with unmodified microphysics, 2) experiments with primary ice production modified to be consistent with observations of ice nucleating particles, and 3) experiments with modified primary ice production and secondary ice production mechanisms added (only one of the two microphysics schemes is used for this). The simulations with secondary ice production are most realistic, but only when rime-splintering is modified to make it activate, as it is needed to kick-off the other secondary ice production mechanisms. The study makes excellent use of new field campaign observation and is an asset to the microphysics modelling community. I have many suggestions for improvement as the study was thought-provoking, and I think that this study will make a great contribution to the literature once these comments have been thoroughly addressed.

Major Comments

- **Run without contact nucleation**: The authors should re-run the simulations with contact nucleation turned off. In sentence 193, the authors say "we use the contact freezing parameterization by Young (1974), as no measurements of INPs in the contact freezing mode were conducted." This contradicts an earlier sentence on 116, which says "If the concentration of the newly formed ice crystals is larger than the INPC, it can be concluded that SIP was occurring." In choosing to leave contact nucleation in the simulations, the authors are positing that contact nucleation is occurring in the real atmosphere but is not reflected in the measurements of INPC. Therefore, if there are more ice crystals than measured INPC, it can not in fact be concluded the SIP is occurring; those "extra" ice crystals could have been formed through contact nucleation. It also, in my opinion, contradicts the sentence in lines 137-139 which says that the modification to immersion freezing "permits a correct quantification of heterogeneous cloud particle formation and also ensures than an agreement of the modeled cloud particle concentrations with observations is accomplished through the correct process," because there is still a heterogeneous nucleation process that has not been constrained in any way with observations. I think keeping contact nucleation active somewhat defeats the purpose of having modified the immersion freezing to be more realistic. Fortunately, there is little evidence for contact nucleation actually occurring in the real atmosphere and, for this reason, it is turned off by default in the P3 scheme, which has otherwise similar ice nucleation as Morr. I think the authors can justify turning it off here and then would be more easily able to make the case that they have constrained the heterogeneous ice nucleation with observations to the best of their ability.

- Thanks a lot for pointing to this issue.
  It is indeed a good suggestion to try what happens in the model when no contact INPs are present. We did that for the $Morr_5$ simulation where SIP is most efficient, but found that the ice concentrations are drastically lower than when contact nucleation is active. To be sure that ice production is not hindered by the remaining mixing ratio thresholds for RS, we also performed one additional simulation with the microphysics settings as in $Morr_5$, but without contact freezing and with all mixing ratio thresholds for RS removed. Even in this case, the ICNC is so far from the observations, that we find it more meaningful to keep contact freezing active in the model than turning it off (see Figure below).
  Taking Fig. 8a from the manuscript into account (previously 10a), which shows that contact freezing contributes at a larger range of times and altitudes than immersion freezing (even though at smaller rates), this was not completely surprising. From our observations, we can't tell whether the simulated contact freezing represents what is happening in nature, but support the quest for further observations-based information on contact nucleation. In case contact nucleation is not occurring in nature (as literature suggests), the strength/efficiency of SIP processes needs to be increased.

  Regarding the changes in the manuscript:
  We modified the lines 137-139 you mentioned above (lines 144-146 in revised manuscript) to a slightly more modest statement by replacing "correct" by "better" and starting the second part of the sentence with "guides towards". The revised text now reads:
  "This permits a **better** quantification of heterogeneous cloud particle formation and also **guides towards accomplishing** an agreement of the modeled cloud particle concentrations with observations through the correct processes."
  We added the finding, that ICNC was reduced drastically when disabling contact freezing, in the text in section 3.1 where modifications in the Morr scheme are discussed (line 232-237 in revised manuscript):

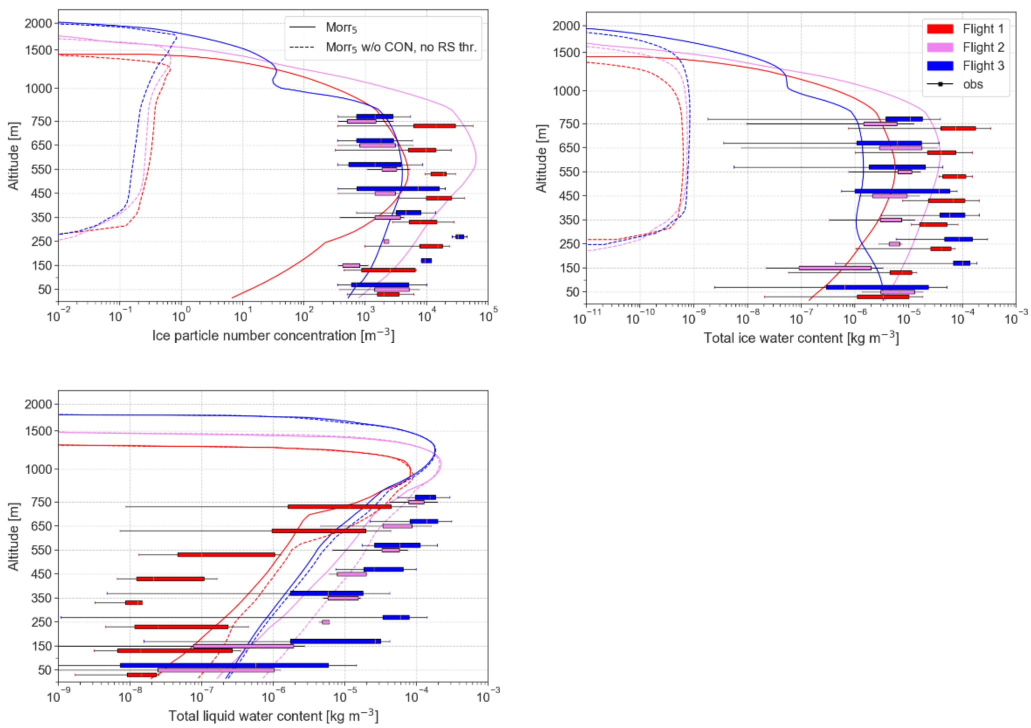
“Even though it is questionable to which extent contact freezing is actually occurring in the atmosphere (Ladino Moreno et al., 2013; Marcolli et al., 2016; Nagare et al., 2016), a sensitivity test with the Morr scheme (not shown) revealed that without contact freezing active and with immersion freezing parametrized after (Pasquier et al., 2022a) as the only heterogeneous ice nucleation process, simulated ice crystal concentrations were far lower than observed. An agreement could not be achieved by adding SIP the way it is done in this study. Therefore, we decided to keep contact freezing active (see also Sect. 4.5).”

At the corresponding place where the treatment of contact freezing in the MY scheme is described, we added a reference to the above note (new text in bold; line 204-205 in revised manuscript):

“For contact freezing, we use the parametrization by Young (1974), as no measurements of INPs in the contact freezing mode were conducted. **For a further discussion of the treatment of contact nucleation, see the following section on the Morr scheme.**”

The conclusion that SIP would need to be even stronger, if contact freezing was omitted, is added in the last paragraph of section 4.5 (line 466-468 in revised manuscript):

“It should also be noted that contact freezing is still active in the simulation. Assuming that this process is not as important in nature as simulated, the strength of SIP would need to be further increased.”



- Clarify the connection to climate: The authors show that the changes to the clouds due to the addition of secondary ice production makes the cloud microphysics more realistic, and changes the precipitation, but does not change the instantaneous radiative effect of the cloud. Might there still be a cloud lifetime affect resulting from differences in precipitation? Also, are the cloud macrophysics constrained so much by the nudging that the microphysics isn't able to affect the cloud macrophysics like it might in the real atmosphere? In particular, nudging to moisture could "bake in" the cloud macrophysics. The authors might consider testing this by running sensitivity simulations with nudging to winds and temperature but not moisture. It is worth mentioning that this finding contradicts that of Young et al. 2019 (https://doi.org/10.1029/2018GL080551) and Atlas et al. 2022 (https://doi.org/10.1029/2021AV000454) and so it is useful to explore and discuss the different possible reasons for that disagreement.

  - Thanks for requesting more information about the connection to climate. We are actually working on a follow-up study investigating the implications of the changes in the microphysics scheme for cloud responses to temperature and aerosols perturbations. These simulations have also convincingly demonstrated that there is nothing in our simulation setup that prevents cloud cover and radiation from changing substantially, given a strong enough perturbation. However, for the scope of the current study, we have also made some modification in this respect: To be able to make more confident statements about the relation between microphysics and radiation we decided to limit the time period used for Fig. 7b (previous 8b) to the time where the direct model evaluation using the observation is performed, i.e. 10-17 UTC. When analysing the simulated radiation for this shorter time period, larger differences between the simulations become evident. However, all simulations slightly underestimate GLW compared to the observations, for reasons that are not entirely clear.
    We made the following changes to the manuscript (new text in bold):
    In Section 4.3, discussing the results from the MY scheme:
    "Downward longwave radiation at the surface **during the balloon flight times (10-17UTC)** is lower than observed in both simulations **and lower in MY_{adap} than in MY_{def}** (Fig. 7b). The **overall** underestimation of downward longwave radiation **may** be explained by a stronger simulated reduction in cloud water content towards the surface and thereby on average a higher and colder cloud base**, but the detailed explanation remains uncertain.**"
    In Section 4.5, discussing the role of secondary ice and the final Morr simulations:
    "Looking at the downward longwave radiation **during the flights**, the variations in the median are small between the simulations **$Morr_1$, $Morr_2$ and $Morr_5$, which all show lower values than $Morr_0$ (Fig. 7b). This hints to that the CDNC modification is influencing the radiation the most in our case. However, the reason for the underestimation compared to observations remains unclear.**"
    In the conclusions (Section 5):
    "The **adapted schemes show a lower** downward longwave radiation than default schemes, **and the changes are likely mainly related to CCNC/CDNC changes. Thus**, the misrepresentation of ice production **might** not lead to a bias in how much heat is trapped between the cloud and the surface, **but due to an overall underestimation compared to observations, conclusions regarding downward longwave radiation remain uncertain.**"

- Emphasize the cascade of secondary ice production: I think the most interesting finding from this study is that you need rime-splintering to "kick off" collisional breakup. I encourage the authors to explain this point in the abstract.

  - This is a good suggestion. We included the following statement in the abstract:

“In our case, rime-splintering is required to kick off collisional break-up. The simulated contribution from collisional break-up is larger than that from droplet shattering.”

- Tighten up the writing: There are many sentences that are unnecessarily verbose. I’ve pointed out a few but not nearly all of them in the minor comments. I recommend that the authors thoroughly edit the writing and remove excess words wherever possible.
    - Thanks for this note. Some of the excess words were meant to make better connections between sentences and paragraphs, but I see that this was often unnecessary and more readable without them. We removed them where you indicated that in the minor comments and a number of additional places.

Medium Comments

- Line 107: Can the authors justify assuming that all particle smaller than 25 microns are liquid?
    - The size threshold of 25 µm for ice crystals is an empirical threshold. Below this size a reliable classification of the particle shape is impossible from the used instrument (see doctoral thesis Annika Lauber https://doi.org/10.3929/ethz-b-000474830, 2020, p. 33). Therefore the existence of ice crystals smaller than 25 µm cannot be ruled out, and the given ICNC can be seen as a lower limit. To reflect this, we extended the sentence. It now reads (added text in bold):
      “Meanwhile, all particles smaller than 25 µm were automatically classified as liquid droplets **as a reliable phase classification based on particle shape from HoloBalloon is limited to particles larger than this threshold (Lauber, 2020). Therefore, the retrieved ICNC is strictly speaking a lower estimate**.”
- Figure 2: The information in this figure could be conveyed in a couple of sentences. I think the authors should either remove this figure or make it more informative to justify it being a figure.
    - The information in this Figure is actually conveyed in the text in Section 3.1 as well. We thought that it would still be beneficial for the reader to be able to see the order of changes applied to the scheme at one glance and hence, have opted to keep the figure in the manuscript.
- Discussion of Figure 3: I think the authors should remind the reader here that the simulations are being nudged to ERA5 reanalysis, to make it clear that this comparison is more of a test of the nudging than of something intrinsic to WRF. Can you mention whether or not these radiosondes are going into the GTS and thus also ERA5?
    - Thanks for pointing that out. The radiosondes do go into GTS and ERA5, thus no large differences should be expected. The text in the beginning of Section 4 is changed to:
      “[…], we verify the performance **of the model nudging** by comparing the simulated meteorological conditions with radiosonde observations (Fig. 3).
      It should be noted that the radiosonde observations are incorporated into the Global Telecommunication System and thus ERA5 data, so no large differences should be expected.”

- Table 1: Listing Pasquier et al. (2022a) under deposition freezing is confusing because it represents immersion freezing. I suggest making another column for immersion freezing and listing “off” for MYdef and Pasquier et al. (2022a) for MYadap, and then listing “off” for deposition freezing for MYadap.
  - Thanks for this note. We agree and changed the Table as you suggested.
- Discussion of Figure 5: There is no mention of the simulated droplet concentrations being 2-3 orders of magnitude too small below 500 m, although a low bias in CDNC is mentioned in line 307. I also suggest putting vertical lines on this figure to show the fixed CDNC values used for the Morr simulations.
  - Thanks for pointing that out. We added a short statement on the underestimation of CDNC in MY_{def} in the previous section (line 310-312 in the revised manuscript; where the underestimation of LWC was mentioned). The sentence now reads (new text bold):

    “However, both MY_{def} and $Morr_0$ are unable to reproduce the LWC below 650m, except for MY_{def} during flight 1 (Fig. 5d, 6c) **while MY_{def} also underestimates CDNC in this altitude region (Fig. 5c)**.”

    A vertical line in panel c of the Figure is added as well.
- Lines 319-322: This sentence is very unclear and I don’t understand the reasons for the increase in ICNC and IWC in MYadap. Through what nucleation process are these extra ice particles forming? How is both the nucleation and particle growth affected by the changes to CCNC in updrafts?’

    The extra ice crystals are graupel, thus they must be formed during collisions and riming during updrafts. We added the following statement after “and found that the change in CCNC during updrafts is the determining factor for the increase in graupel number.” (line 335-336 in revised manuscript):

    “We therefore conclude that the CCNC changes lead to changes in the cloud droplet size distribution that make riming more efficient.”
- Ordering of discussion/figures: Figure 9 is first mentioned well after Figure 10 is mentioned so it seems that they should be switched. I also think it might make sense to have all of the discussion of precipitation and radiation (Figure 8) after the discussion of Figures 5-7, perhaps in its own section, so that the reader doesn’t need to go back and forth so much to tie the discussion to the figures. I also didn’t notice Figure 7 being mentioned in the manuscript at all although when I went back to check, I saw it was referenced one time. The authors might consider moving Figure 7 to the appendix.
  - Thanks for the comments. Figure 9 and 10 are switched. It is a good idea to move Fig. 7 to the appendix. It became Fig. B1 now, while previous Fig. B1 became B2. Regarding precipitation and radiation, we decided to keep the discussion of these with the discussion of the general implications of the different modifications. But with previous Fig. 7 moved to the appendix, there is now less need to jump back and forth for the reader.
- Lines 456-457: I’m confused about the statement that starts the conclusion section because section 4.2 and 4.3 describe several significant biases in both simulations with default microphysics. What does “reasonably” mean here?
  - That is a very good comment. The “reasonable” representation was mostly aiming at the maximum amount, but that information was missing. The start of the conclusions section is now changed to (modifications marked bold):

“This study shows that generalized out-of-the-box cloud microphysics schemes, i.e. MY and Morr, **fail to correctly represent the vertical structure of ice and liquid water content of Arctic mixed-phase clouds. While these schemes do reproduce the observed maximum values reasonably well, we find that** this occurs for the wrong reasons due to compensating errors.”

Minor Comments

- Line 1: I agree that the fact that clouds play a role in Arctic warming is undisputed, but I think the specific role that they play is disputed. I suggest re-phrasing for clarity.
    - That is a valid concern. We removed this sentence from the abstract at the request of the other reviewer, in order to focus more of the abstract on our own findings.
- Line 21: I find the phrasing “...special interest for climate research and particular efforts are made...” to be rather vague
    - We agree that especially “of special interest for climate research” was a rather vague formulation and changed the first sentence to the following:
      “Given the Arctic being the fastest warming region on Earth, understanding the drivers of Arctic climate change and in particular the role of clouds in this warming has been of special interest (e.g. Serreze and Barry, 2011; Wendisch et al., 2017, 2019).”
- Lines 45-46: Could consider adding Järvinen et al. 2022 (https://doi.org/10.1029/2021JD036411) to the reference list
    - Thank you for pointing this out, we have now included this study.
- Lines 48-51: There is an additional proposed mechanism described in Knight 2012 (https://doi.org/10.1175/JAS-D-11-0287.1)
    - Thanks for the comment, we added this as a new sentence. It now reads (added text in bold):
      “These processes include the collisional breakup of ice crystals (BR), rime splintering (RS, also called Hallett-Mossop process), droplet shattering when freezing (DS) and sublimation fragmentation in subsaturated cloud regions (SF) (e.g. Field et al., 2017; Korolev and Leisner 2020). **The existence of additional SIP processes has been proposed but these have yet to be named and confirmed (Knight, 2012)**.”
- Line 143: Suggest changing “accomodate” to “account”
    - Done, thanks.
- Line 151: Suggest changing “whereof” to “of which”
    - Here we actually prefer “whereof” and keep it as both are correct.
- Lines 171-175: The wording “on one hand...on the other hand” implies that there should be a contradiction, which there is not here, so I suggest rephrasing
    - Thanks for pointing that out. We indeed did not intend to state a contradiction and simply removed these words. This is also consistent with your general comment about excessive wording. The sentence now reads:
      “The reasons for nevertheless focusing most of the study on simulations with the Morr scheme were that the MY scheme failed to produce a suitable control simulation due to excessive graupel production when CCN/INP concentrations were adapted to observed values (see Section 4.3), and, more importantly, that we wanted to apply and test the new SIP implementation recently developed for the Morr scheme by Sotiropoulou et al. (2021) and Georgakaki et al. (2022).”
- Line 177: Suggest changing “during” to “when they are”

- We changed “during” to “when there are”. The sentence now reads (changes marked as bold text):
  “The default CCNC **when there are** negligible vertical motions **or** downdrafts is 200 cm^{-3} for continental aerosol and 80 cm^{-3} when maritime aerosol is selected.”
- Line 179: Suggest changing “selected” to “assumed”
  - Good suggestion, that avoids the close repetition of “selected”. The wording is changed.
- Lines 183-185: Suggest changing the two instance of “after” to “from”
  - This is changed.
- Line 204: Suggest changing “predefined to” to “predefined as”
  - Made the formulation even shorter and more concise writing “as the CDNC is a predefined number”.
- Line 237: Suggest changing “not overcome” to “never exceeded”
  - Thanks, this is done.
- Line 250: Suggest rephrasing “we ensure a satisfying model performance” to something like “we test the model’s ability to simulate the observed environment”
  - This is done.
- Line 280: Suggest rephrasing “it is reassuring to see that” to something like “we show that”
  - Thanks, this is done.
- Line 297-298: List the corresponding figure panels just like they are listed in the previous sentence
  - This is done, thanks.
- Line 302: Suggest removing the word “both”
  - This is done.
- Line 303: Suggest changing “evident” to “evidenced”
  - This is changed.
- Figure 5d: The x-axis tick labels are partially covered by the axis label
  - Thanks a lot for the careful look! This is adjusted now.
- Line 310: Suggest changing “little” to “low”
  - This is done (was line 311 in original manuscript).
- Legend of Figure 5: Suggest removing the word “continues” before “logarithmic”
  - This is done.
- Legend of Figure 8: Suggest changing “divided in” to “divided into”
  - This is done.
- Line 327: There are many places in the text where things are stated in an excessively wordy fashion that make the sentences less clear. For example, here, I recommend removing the words “follows” and “now shortly.”
  - This is done.
- Line 352: Suggest removing the word “formed”
  - This is done.
- Line 362: Suggest changing to “We expect that the decrease in ICNC from modifying heterogeneous nucleation will be counteracted by increasing secondary ice production in Morr3 following...”
  - Done, thanks.
- Line 365: Suggest changing “difference” to “differences”
  - Thanks, “a substantial difference” is changed to “substantial differences”.

- Line 380: I think this should say “SIP processes included in the Morr scheme and those by Sotiropoulou et al. 2001”
  - Thanks for the suggestion. The SIP process from the original Morr scheme was (in principle) also active/allowed to happen before, the main message here was to point out that the additional processes did not alter the situation. Therefore, we changed “included” to “added” and the sentence now reads:
    “Contrary to our expectations, activating the SIP processes added in the Morr scheme by Sotiropoulou et al. 2021 did not immediately increase the ICNC.”
- Line 387-389: This sentence is very unclear and “overview over” definitely needs to be rephrased
  - You’re right, both reviewers marked this sentence. The words “The overview over” were simply unnecessary in the beginning of the sentence. We removed them and additionally split the sentence into two sentences to make it less complicated. The revised version reads:
    “In which altitude region and during which flight the individual conditions and the joint condition are fulfilled is given in Table C1. This overview explains the differences in the simulated impact of the RS process during different flights well.”
- Line 408: Suggest removing the word “between”
  - Done, thanks.
- Line 436: Suggest changing “anyway” to “still”
  - This is done.
- Line 442-443: Why is the overestimated precipitation likely due to excessive drizzle? Is it because most of the precipitation in the real atmosphere was observed to be solid?
  - That’s correct, but the phase assessment is qualitative information only and based on the campaign crew’s notes, which do not cover the whole accumulation period. Therefore, we added the following sentence:
    “However, the phase assessment of precipitation from observations is uncertain and of qualitative type only, as it is based on the notes of the campaign crew, which do not cover the whole 24h period.”
- Line 459: Suggest changing “more numerous” to “overly efficient”
  - This is done, good suggestion.
- Line 481: Suggest changing “apart from” to “in addition to the” and removing the word “also”
  - This is done.
- Lines 482-483: The word “changes” appears twice. Also another example of how wordiness can be reduced is by removing “Regarding the cloud’s radiative effect,” at the beginning of the sentence and similar phrases throughout the text.
  - Thanks for pointing out this writing mistake. The beginning of the sentence is removed, as you suggest.
- Line 487-488: Suggest changing “across differences in the microphysical conditions” to “across different microphysical conditions”
  - Thanks, this is changed.

Additional edits:

The reference Motos et al. 2023 was changed from the preprint on EGUsphere to the final published version in Atmospheric Chemistry and Physics (doi: 10.5194/acp-23-13941-2023).