Author Response to Reviewers for: Sensitivity of precipitation formation to secondary ice production in winter orographic mixed-phase clouds

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We sincerely thank Sylvia Sullivan for the constructive feedback. Her suggestions considerably improved the quality of the manuscript.

Below we present a detailed response with the reviewer comments in black, our responses in blue and additions to the manuscript in blue italics.

1 Reviewer 1: Sylvia Sullivan

Dedekind et al. implement secondary ice parameterization into a nonhydrostatic model and investigate the impact on simulated ice crystal number concentrations and microphysical tendencies, as well as precipitation patterns, over a region with significant topography and relative to a variety of measurements. This study is an important contribution to clarifying the interactions between ice microphysics, particularly poorly constrained secondary production processes, and surface precipitation intensity. The figures are beautifully done. I have some major comments, primarily to improve the readability of the results sections and further justify some statements there. A number of minor comments are also included in an annotated PDF.

Major Comments

1. While the processes (e.g. temperature dependence and mechanism) of rime splintering and collisional breakup are described in lines 48-57, frozen droplet shattering is only mentioned in passing in the introduction (lines 69-72). Even if frozen droplet shattering is not influential in this case, I would still devote another sentence or two to describing it after the other SIP processes.

   AR: Yes indeed. We added the following sentences.

   Another possibility for SIP is droplet shattering upon freezing (e.g., Kolomeychuk et al., 1975; Lauber et al., 2018). If the pressure within a droplet reaches a critical point, the droplet may fragment upon freezing. This process has been observed to happen for droplets larger than about 40 µm in diameter (e.g. Lawson et al., 2015; Korolev et al., 2020). The likelihood of fragmentation upon freezing and the number of produced splinters during the fragmentation increases with droplet size (Kolomeychuk et al., 1975; Lauber et al., 2018, 2021)

2. I had difficulty understanding the aerosol treatment described in lines 137-145. Was a representative aerosol profile derived from the values in different temperature ranges cited (> 261 K, 258-261 K, etc.)?

   AR: We only used an aerosol concentration of $2\,\mathrm{cm}^{-3}$ from the LIDAR as input for the D15 parameterization (DeMott et al., 2015) between temperature of 243 and 258 K. Outside of this temperature range, no INPs were released. This means that the INPs, estimated by the D15 parameterization, are only active between 243 and 258 K. The aerosol concentrations measured at Weissfluhjoch at a temperature of 261 K was only used to validate the LIDAR measurements. If the temperature was colder than 258 K at Weissfluhjoch, we would have used the aerosol concentrations measured at Weissfluhjoch as input to the model, but because of the warmer temperature, the estimated INPs from the D15 parameterization would be less accurate (DeMott et al., 2010). We recognize the confusion generated by the sentence below and adapted it:

   "Because of the temperature mismatch between the observed temperature and the temperature range for which the INP parameterization is accurate ($T < 258$, DeMott et al., 2010) , we used the retrieved aerosol concentration from the upward-pointing LIDAR that was situated at Davos Wolfgang.".

3. I struggled to get the takeaways from Sections 3.1.1 and 3.1.2. Three suggestions in this regard.

   - You start by discussing the ice crystal shape classification in lines 231-235 but then transition to modeled versus measured. I would move anything about the shape classification to where you discuss it further (lines 257-272). Given how little rime splintering changed the simulated ICNC, is it not surprising how large the percentage of observed rimed crystals is in Figure 3b? I felt Fig. 3b warranted more discussion.

     AR: We agree and have moved the shape classification section as suggested and modified the manuscript to fit in with the storyline. There are two things to note here. Firstly, from the observations, the possibility exists that the recirculation of raindrops occurs (Lauber et al., 2021). Ice particles that fall through the melting layer (∼ 1500 m, -1 °C at 1808-1961 m, Fig. 3 caption) and melt to raindrops are lifted back into the MPC by the turbulent mountainous flow. The increased in-situ rain mixing ratio can provide additional rimer and therefore increase the number of rimed ice particles. Secondly, it shows that the model was not able to fully capture the amount of cloud liquid, specifically larger cloud droplets and more raindrops, and thereby reducing the potential for rime splintering to occur. Also, it is essential to note that even though the simulated cloud droplet mixing ratio, at 12:00 and 13:00 UTC, was between 10^{-2} and 10^{-1} $\mathrm{g\,m}^{-3}$ the droplets were less than 15 µm in diameter. In COSMO, droplets larger than 10 µm can collide with ice particles; however, the droplets need to be larger than 25 µm for rime splintering to occur (Hallett and Mossop, 1974). We added the following text to the manuscript:

     Surprisingly, the rime splintering simulation contributed little towards SIP considering the large fraction of observed rimed particles (Fig. 4, 5g and Fig. 3b). There are two possibilities for this discrepancy. Firstly, from the observations, the possibility exists that the recirculation of raindrops occurred, leading to additional breakup (Lauber et al., 2021). Ice particles that fall through the melting layer (∼ 1500 m, -1 °C at 1808-1961 m, Fig. 3 caption) and melt to raindrops are lifted back into the MPC by the turbulent mountainous flow. The increased in-situ rain mixing *ratio can provide additional rimer and, therefore, increase the number of rimed ice particles. Secondly, the low SIP can be attributed to the lack of available liquid water in general, and when cloud droplets were present, they were too small (less than 25 µm) to cause rime splintering to occur (Hallett and Mossop, 1974).*

- It would be easier to make the model-measurement ICNC comparison visually if the values of Figure 3a were actually atop those of Figure 4a.

  AR: It would be. We added it onto Figure 4a and 5a

- It was not clear to me what changed between 12:00 and 13:00 UTC (i.e. Figs 4 and 5). In particular, I thought it was interesting that γ_{BR} controls the vertical structure of ICNC and secondary production rates in Fig. 5 but not Fig. 4. Do you have a hypothesis why this is so?

  AR: We also expected the vertical structure of the ICNC in Fig. 4 to be controlled by γ_{BR}. There is a sharp decrease in the ice and snow mixing ratio, the depositional and riming rate, and the cloud liquid between 2 and 3 km in altitude in the BR2.8_T simulation. It is likely that there was an in-homogeneity in the MPC acting as a sink to the mixing ratios and number concentrations, and affecting the collisional tendencies and secondary ice production rates.

4. Given the discussion of updraft throughout (e.g. in regard to WBF or around lines 305-313), profiles or maps of vertical velocity would be helpful to see whether reduced precipitation in the breakup simulations is due primarily to microphysical or dynamic factors or both.

   AR: We have included two figures that will be included in the supplement. The text in the manuscript was also adapted to refer to these supplementary figures. The precipitation reduction appears to be largely impacted by the cloud microphysics. The three cases that we considered (Fig. 1) indicate that ice-graupel breakup increases ICNC, reducing the ice crystal size and therefore, increase the in-situ residence time of the ice crystals. Depositional growth, as a result of the more numerous and small ice crystals, also increase. Through latent heat release, it consequently strengthens the vertical wind speed(Fig. 2). At these three locations and at these times, there was a reduction in the surface precipitation in the breakup simulations (Fig. 1).

5. I would choose a metric other than the spatial Pearson correlation coefficient for precipitation evaluation; otherwise, the r2 values in Table 3 seem to contradict the statement that “COSMO benefits from the inclusion of collisional breakup processes in simulating precipitation.” You could, for example, calculate the statistical distances between the distributions shown in Figure 8 with the Kullback-Leibler divergence. There is a scipy Python package here: https://docs.scipy.org/doc/scipy/reference/generated/scipy.special.kl_div.html

   That is a good idea. We have updated Table 3 (shown as Table 1 here) with the Kullback-Leibler divergences. We have updated the manuscript in section 3.2 with:
   To assess the skewed precipitation distribution between the CNTL and sensitivity simulations, the Kullback-Leibler divergence that measures the distances between asymmetric distributions was used. All the simulated precipitation distri-

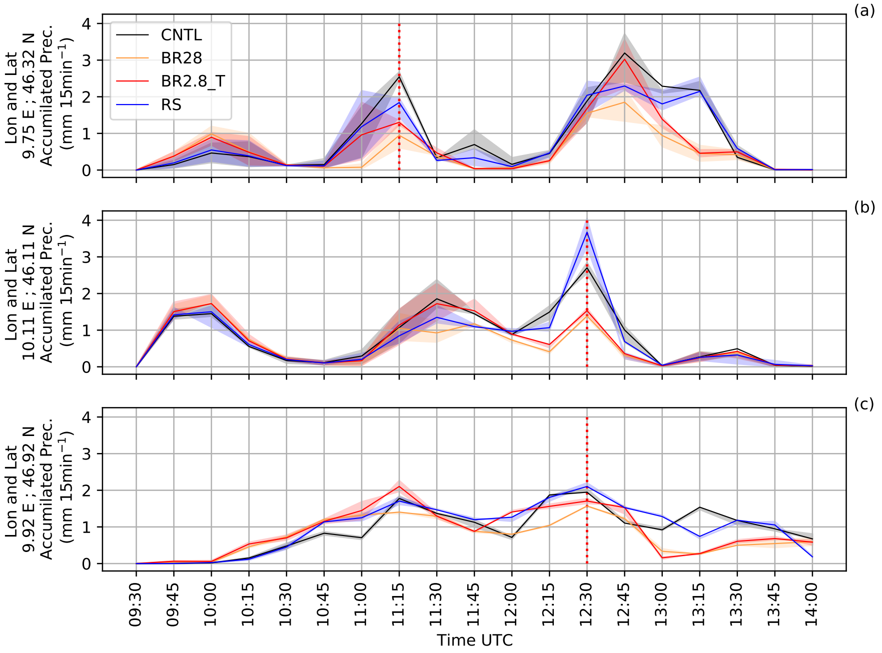

Figure 1. Three localized regions of high precipitation in the CNTL simulation which are suppressed in the collisional breakup simulations. The solid lines are the model mean and the shaded regions are the minimum and maximum values for the four closest model points. The red dotted lines are the selected times for the vertical profiles in Figure 2.

Table 1. Interquartile range (IQR) between the 25th and 75th percentiles and median in mm h^{-1} for CombiPrecip and the sensitivity simulations and the Kullback-Leibler divergences between CombiPrecip and the sensitivity simulation precipitation distributions.

	IQR	25th perc.	75th perc.	Median	KL div. (nats)
CombiPrecip	1.09	0.41	1.50	0.93	
CNTL	1.60	0.35	1.94	1.60	1163
BR28	1.46	0.33	1.79	1.46	930
BR2.8_T	1.51	0.32	1.83	1.51	1035
RS	1.79	0.35	2.14	1.79	1132

butions were more skewed towards the tail, overestimating the density between 2 and 4 mm h^{-1} *and underestimating the density between 1 and 2* mm h^{-1} *over the domain. The collisional breakup simulations diverges least from the observations mainly due the lower and more accurate densities at higher precipitation rates (Table 3 and Fig. 8).*

Minor Comments

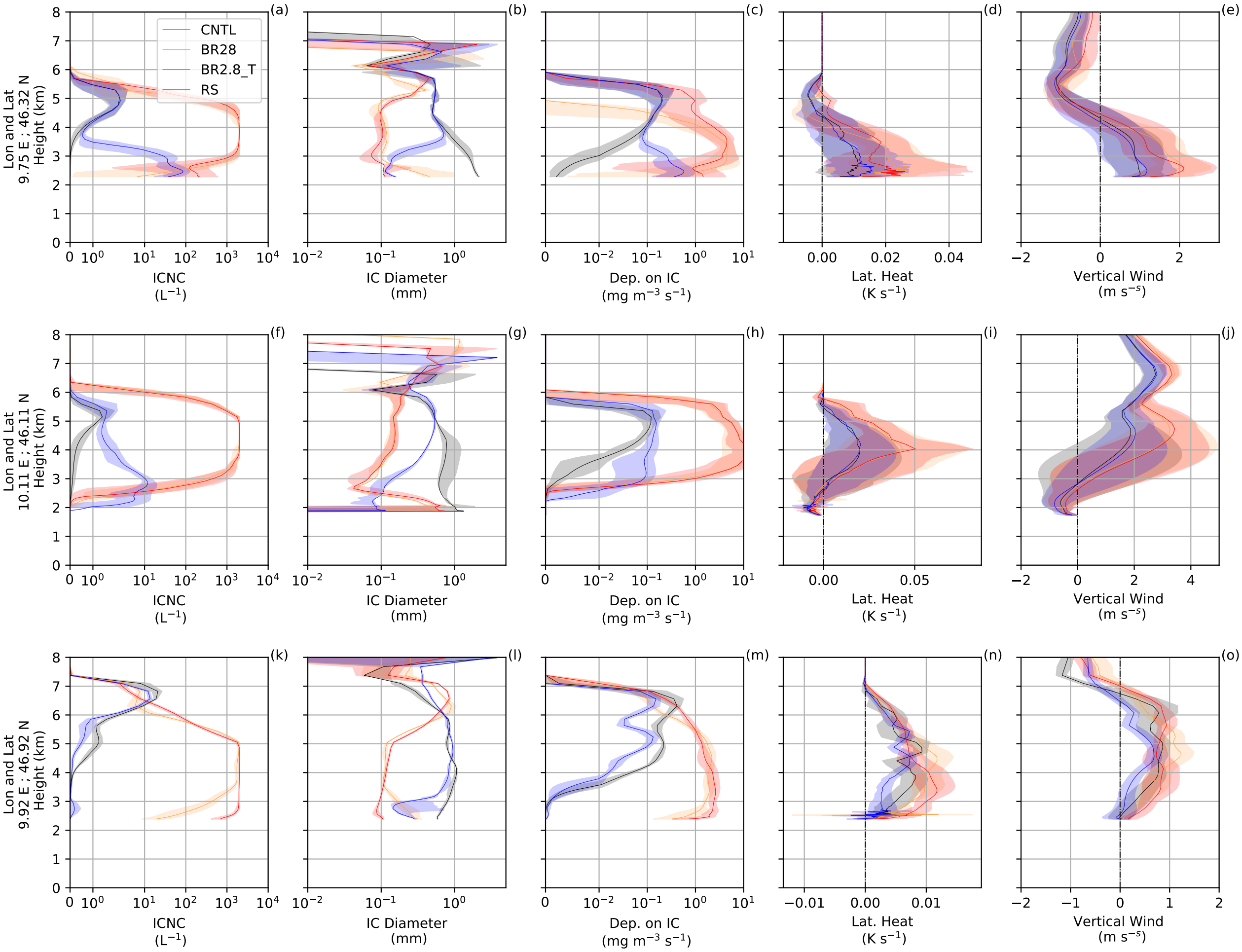

Figure 2. Vertical profiles of the a), f) and k) ice crystal number concentration (ICNC), b), g) and l) ice crystal diameter, c), h) and m) depositional (Dep.) growth of ice crystals, d), l) and n) latent (Lat.) heat release and e), j) and o) vertical wind velocity. The solid lines are the model mean and the shaded regions are the minimum and maximum values for the four closest model points. a) to e), f) to j) and k) to o) corresponds to the localized high precipitation regions of Figure 1 a), b) and c) respectively.

- Line 4: Reword for clarity. "Here, in a region over the Eastern Swiss Alps, we perform sensitivity studies of additional simulated SIP on precipitation formation and surface intensity."

  AR: We modified the manuscript.

- Line 25: Not necessarily, as you point out below. I would add the caveat "in a certain range of updrafts (Korolev 2007)" already here.

AR: We have separated the two sentences to make a distinction between a thermodynamically unstable cloud and the WBF process.

Once ice crystals exist in a supercooled liquid cloud, the cloud becomes thermodynamically unstable. Depending on the ice crystal concentration and their size, an updraft of $2\,\mathrm{m\,s^{-1}}$ *may enable a MPC to sustain the simultaneous growth of ice particles and supercooled cloud droplets (Korolev, 2007). However, for lower updraft velocities the ice crystals grow at the expense of the evaporating cloud droplets.*

- Line 31: I don't understand what this means. In order to prevent glaciation, updrafts must be sufficient to maintain supersaturation wrt liquid? Please clarify.

  AR: Yes, that's what we meant. We reworded the sentence:

  In such environments, supersaturation over liquid water needed so that cloud droplets can grow in the vicinity of ice crystals.

- Line 41: Not necessarily. The INP number must be greater than a threshold value to eliminate the possibility of simultaneous heterogeneous-homogeneous nucleation.

  AR: We believe that heterogeneous-homogeneous nucleation is often associated with cirrus clouds that have very little INPs. In MPC at warmer temperatures than -38 °C this should not occur. We clarified the sentence with:

  The number concentration of INPs can equal the ice crystal number concentration (ICNC) in MPC at temperatures warmer than -38 °C, when no secondary ice production (SIP) process or external ice crystal sources from the surface (e.g. hoar frost or blowing snow) or above (seeder-feeder process) are active.

- Line 55: A bit redundant from earlier. I would state instead "collisional impact" or "collisional kinetic energy"

  AR: We adapted the sentence

- Line 84: This is an incomplete sentence

  AR: We adapted the sentence:

  Secondly, what is the effect of SIP processes on the cloud microphysics and consequently precipitation formation, location and intensity?

- Line 127: What are the time step and output frequency?

  AR: We added the sentence to section 2.2.1:

  The model time step was 4 s and the output frequency every 15 min.

- Line 146: CNDC -> CDNC

  AR: We corrected this

- Line 163: You could put the RS tendency equation here, as you do for the collisional breakup. Otherwise, I would at least cite the fragment number parameter that you use.

  AR: We added the fragment number to section 2.2.3:

  ...whereby the pressure is relieved when the frozen shell cracks and produces $\underline{3.5\times10^8\ (\text{kg rime})^{-1}}$ secondary ice particles (Hallett and Mossop, 1974).

- Line 171: I'd keep in deg C to be consistent with the other temperatures given in this section.

  AR: We adapted the temperature

- Line 173: "raindrop", right?

  AR: Correct.

  ...existing raindrop freezing tendency...

- Line 177: enforced?

  AR: We modified the manuscript:

  ...collided large, 1.8 cm...

- Line 188: "N_BR is the fragments generated" not F_BR

  AR: That's true. We updated section 2.2.3:

  ...F_{BR} is the leading coefficient...

- Line 201: So the other kinds of "inter-frozen-species" collisional breakup given in Table 1 of Sullivan et al. 2018a are not included? I would state this explicitly.

  AR: No, due to the lack of hail in winter orographic MPC we didn't consider hail-graupel/ice breakup.

  In contrast to Sullivan et al. (2018, Table 1), graupel was the only species that could collide with and breakup ice crystals or snow. Hail was not permitted to collide with graupel and so reduce the graupel diameter to smaller sizes which emphasizes the need for α.

- Line 209: I would add a one sentence explanation why droplet shattering is not important in this case. Simply insufficient rain mixing ratio at the appropriate temperatures?

  AR: That's correct. We have added the following sentence to section 2.2.3:

  The low SIP rate by droplet shattering is because of the insufficient rain mixing ratio at the needed temperatures for this process.

- Line 214: I don't understand what this means. Please clarify.

  AR: That is extremely confusing, but what was meant should be more clear in the adapted sentence towards the end of section 2.3.3:

However, the ICNCs were strongly influenced by the upper bound of the ICNC in COSMO, especially between 3 and 5 km.

- Line 229: If you have the corresponding mean/median temperature for each of these three altitude ranges, that would be helpful for generalization purposes.

  AR: 1808-1961 m, 1961-2113 m and 2113-2266 m correspond to -1 °C, -1.5 °C and -2.5 °C respectively. We added it to Figure 3 in the manuscript.

- Line 244: I would start a new paragraph here. (CNTL and RS simulations underestimate ICNC; what about the BR simulations?) I would also restate that BR28_T is the breakup simulation with reduced production at warmer subzero temperatures.

  AR: We have modified section 3.1.1:

  The ICNC in the BR28 simulation, with reduced ice fracture generation at warmer subzero temperatures, simulation was between 1 and 2 L^{-1}*...*

  ... albeit with a high uncertainty below 3 km at 12:00 UTC. Differences in the ICNC profiles and specifically at the surface is to be expected between BR28 and BR2.8_T because γ_{BR} *controls the vertical ICNC profile. The SIP rate of collisional breakup was 2 orders of magnitude larger than rime splintering ...*

- Line 253-255: If the ice crystals are mostly pristine, does this not contradict the statement that "primary ice production rates are nearly non-existent"?

  AR: It is true that the primary ice production in the vertical profile in Fig. 4 and 5 is nearly non-existent. However, these ice particles could have been advected from upstream where they formed. In Fig. 10 we show that primary ice production was occurring upstream and in the vicinity of Gotschnagrat, albeit 4 to 5 order of magnitude smaller than the SIP rate. Additionally, our definition of pristine ice crystals in this context is not correct. We considered pristine ice particles as ice particles that only grew by vapor deposition. Therefore, this could include fractured and pristine ice particles. Nonetheless, it does not take away from the point that the breakup simulations underestimated the liquid in the lower part of the cloud. We changed our definition of pristine.

  This also meant that the growth of ice crystals was mostly through vapor deposition that, in turn, suggests that the ice crystals were mostly unrimed and belonging in the irregular or pristine categories.

- Line 283-284: Unclear wording. Please clarify.

  AR: The sentence was modified to:

  The rime splintering also didn't improve the precipitation rate compared to the observations.

- Line 285-288: The underestimation is not by much. The overestimation of LWP by the CNTL and RS simulations is far larger. I would emphasize this instead.

AR: An underestimation of 150 to 250 $\mathrm{g\,m^{-2}}$ is not insignificant considering LWP satellite retrievals (e.g. Lebsock et al., 2008; Wang et al., 2013), and the LWP was also at or very close to 0. In absolute terms the difference is maximized. Therefore, we only added another sentence on the overestimation of the CNTL and RS.

The overestimation of the liquid water path compared to the microwave radiometer was huge when the breakup of ice particles was excluded from the simulations. In general, including ice-graupel collisions significantly reduced the liquid water path overestimation in the CNTL and rime splintering simulations.

- Figure 6 Caption: How is time treated in this comparison? Are you showing the observations with their original 30-s frequency? And the model output frequency is every 15 min?

  AR: The observations with their 30 s frequency are integrated over 15 min intervals. The model precipitation is accumulated over every 15 min. We added this to the description to Figure 6.

- Line 305-308: I am not convinced. Could it not also be just that you are forming much larger ice crystals when the SIP processes are excluded (Figs. 4e and 5e)?

  AR: Yes, that is true, but the primary reason for the faster growth rates is a result of the higher updraft speed. The localized precipitation maxima appear in the CNTL and persists, but to a lesser extent, in the SIP simulations. Both the dynamics and microphysics play a role for the ice crystal size. We also reiterated your point about the larger ice crystals in the revised manuscript.

- Line 329: How was the width of the cross-section chosen?

  AR: The width of the cross-section is ∼25 km wide to cover a majority of the precipitation along the cross-section as seen in the observations. It was chosen along the average wind direction during the analysis period, wide enough for robust results and at the same time not too wide so that the results become diluted.

- Line 334: "rime splintering", as below the MPC structure does indeed change with the collisional breakup SIP

  AR: This is true. What we meant here is the enhanced SIP from rime splintering. We adapted the manuscript to make it more clear.

  The structure of the MPC did not change significantly due to the enhanced SIP from rime splintering.

- Figure 9: This schematic is beautiful! I feel it should be the final figure of the article though. I would also integrate it further into the Discussion and, if desired, incorporate the changes in cloud layer structure from Fig. 10 (i.e. show a shallower cloud on the right)

  AR: Thank you. We agree and will move this figure into last place. We have considered the cloud layer thickness when creating the schematic. From figure 10 the cloud with ice-graupel collisions is thicker. The mixed-phase part of the cloud is also shown to be thinner than that of the CNTL (indicated by the rain drops), similar to Figure 10. We have integrated it more into the discussion.

  The most likely explanation for the decrease in precipitation is that the smaller ice particles in our collisional breakup *simulations that sediment slower reside longer in the cloud. Longer residence times increase the ability for ice particles to grow by vapor deposition and riming, thereby enhancing the latent heat release and consequently the updraft velocity. The higher updraft speed, in turn, can loft the ice particles to higher altitudes in the glaciated part of the cloud and ultimately alter the intensity and location of the surface precipitation.*

- End of Discussion: It could be worthwhile to think about the relative importance or potential interaction of hoar frost / blowing snow and SIP here, given that other studies have indicated the importance of the former in similar regions (Lloyd et al. ACP 2015). Could their importance be distinguished for example solely by surface wind speeds?

  AR: We have added the following description in the discussion with an additional figure that will be added to the supplementary information.
  The resuspension of snow particles from the surface into the atmosphere where they could potentially interact with cloud particles is dependent on the wind speed. The total mass change of the snow pack is a function of the horizontal distribution of blowing snow, the sublimation rate of blowing snow, the sublimation/evaporation rates, or condensation/deposition rate at the surface, the precipitation rate and the runoff of liquid water that all impact the mass change rate of a snowpack (Armstrong and Brun, 2008). Mahesh et al. (2003) have found that a threshold value for the wind speed above ∼7.6 $\mathrm{m\,s^{-1}}$ is necessary to resuspend snow particles. More recently in the Swiss Alps, Walter et al. (2020) interpolated a threshold value of ∼7.5 $\mathrm{m\,s^{-1}}$ from radar observations which was in agreement with other studies (e.g. Li and Pomeroy, 1997). Wind speeds of larger than 7.5 to 15 $\mathrm{m\,s^{-1}}$ could then transport these snow particles distances of 60 to 240 m. In our case the wind speeds recorded by the snowdrift station used by Walter et al. (e.g. 2020) were mostly below the 7.5 $\mathrm{m\,s^{-1}}$ for the largest part of our analysis period (Fig. 3). However, between 12:45 and 13:30 UTC when winspeeds exceeded 7.5 $\mathrm{m\,s^{-1}}$, a case can be made for blowing snow affecting the ice particle number concentration in the MPC and thereby triggering secondary ice processes.

- Line 404: more precisely "ice-graupel"

  AR: Correct. We modified the manuscript to say ice-graupel collisions for our work.

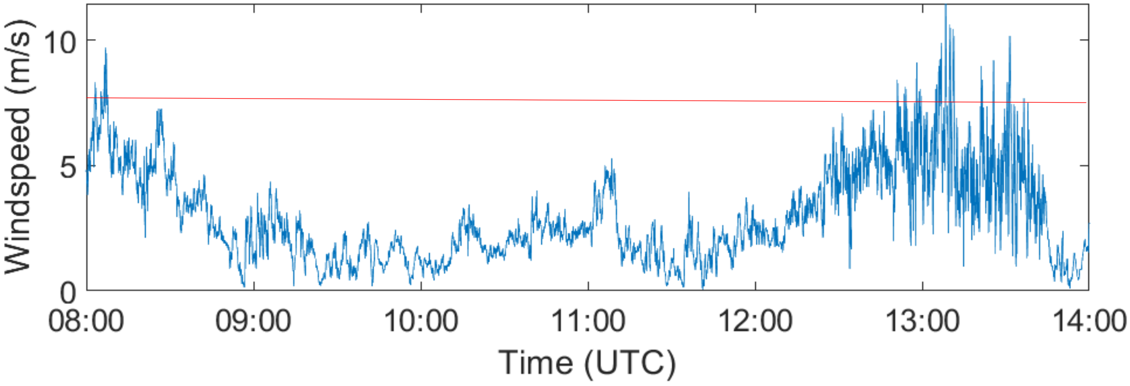

Figure 3. Wind speed recorded by the snow drift station on 7 March 2019. The red line is defined as the threshold to re-suspend snow particles (Walter et al., 2020). Data availability (Walter et al., 2019).

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We thank Jason Milbrandt for the constructive feedback. His suggestions considerably improved the quality of the manuscript.

Below we present a detailed response with the reviewer comments in black, our responses in blue and additions to the manuscript in blue italics.

1 Reviewer 2: Jason Milbrandt

This manuscript describes a numerical modeling study of the impact of including parameterizations for additional (besides Hallet-Mossop) mechanisms for secondary ice production (SIP) in a 2-moment microphysics scheme used in a 1-km mesoscale model simulation of a winter orographic precipitation event. This is a really interesting and timely tudy. SIP is a popular topic in ice microphysics these days, but parameterizations of different SIP mechanisms seem to be still in the exploratory stages, at least in common use in 3D mesoscale model simulations. This is probably related in part to previous limitations in microphysics schemes that limited the relevance in SIP in models; however, this is definitely changing so this topic is becoming increasingly important in atmospheric modeling. Overall, I think this manuscript is very solid. The topic is timely, the study is original, the scientific methodology is sound, and the presentation (writing quality, figures, and organization) is high quality. I do have some concerns about the treatment of graupel in the microphysics scheme used in this study (see comments below). I do not think it is a “deal breaker”, but I think the authors should address in the text (or with sensitivity tests) the implications of certain limitations in the bulk scheme used to their conclusions about parameterizing SIP in bulk microphysics schemes. I am indicating a recommendation of "major revision", but this is only to reflect the importance for which I regard those major comments to be addressed in the paper. Overall, this is a great paper.

Major Comments

1. The choice of microphysics scheme used and the implications in this study need to be discussed on more detail. The risk of not doing so adequately is that readers may dismiss the results as not being sufficiently general, but only relevant to the particular scheme used. The scheme used is the Seifert-Beheng scheme, which is a detailed 2-moment, multi-ice-category, bulk scheme. This has several implications – strengths and limitations – with regards to studying the effects

of SIP. First, it is a bulk scheme, not a bin scheme. In my opinion, this is perfectly fined but it needs to be defended since there is a popular (and incorrect) assumption that bin schemes are inherently more accurate than bulk schemes. This is simply not the case when it comes to ice-phase microphysics, where the results with bin schemes have at least as much, if not more, variability as those with bulk schemes (see Xue et al., 2018, MWR). It will strengthen the paper if the use of a bulk scheme is defended in this regard. Second, S-B is a 2-moment scheme. 2-moment is, of course, necessary (rather than 1-moment) as a minimum in order to properly capture the effects of changes to ICNC and other impacts. Is 3-moment necessary? Probably not for this study, but since 3-moment schemes exists, it is probably worth discussing, given that 3 you are advocating methods to advance the modeling of ice-phase microphysics in models.

2. Following from the above point, there is a major limitation (set of limitations) in the SB scheme which is relevant to this study. The S-B scheme, although 2-moment and detailed, can be regarded as a "traditional" bulk scheme in that it represents ice-phase hydrometeors by partitioning them into representative categories (e.g. "snow", "graupel", etc.) with fixed parameters for bulk physical properties. There is a growing viewpoint in the modeling community that this category-based approach has some inherent weaknesses (in terms of physical realism) and practical limitations (in terms of consistent simulation results). The alternative that some modelers have explored is to focus on the continuous evolution of physical properties (density, etc.) of ice (e.g. Hashino and Tripoli 2007; Morrison and Milbrandt 2015; Jensen al. 2017). In the current study on parameterizing SIP, this has some important implications on interpreting the results. As the authors pointed out, S-B has a fairly crude parameterization of the "conversion" of snow to graupel; this is a non-physical process anyway but it has a large impact on graupel production, and thus collisional breakup. Also, the representation of graupel also has some other relevant limitations; it has a fixed density and fixed fall speed parameters. Thus, even if the parameterization of SIP by mechanical breakup were perfect, the representation of graupel is not – therefore, the representation of the effects of some SIP mechanisms are highly dependent on the treatment of graupel.

   At the very least, these aspects of the baseline microphysics scheme used need to be discussed in the paper in order to provide a broader context to interpret/evaluate the results. Of course, it is not practical to say that the authors should switch to a property based scheme. However, the degree to which the limitations of the category-based scheme could be examined further through a small number of sensitivity tests (e.g. adjusting the rate of conversion from snow-to-graupel; changing the graupel fall speed parameters). I think this could strengthen the paper. But at the least, the aspects/limitations related to the category-based S-B scheme should be included in the discussion.

   AR: We have updated the text in the Methods and in the Discussion sections to address the baseline microphysics and, the strengths and limitations of the two-moment bulk microphysics scheme that we used. We agree that the suggested sensitivity studies are important and should be carried out. Because of their importance and the scope that needs to be covered regarding the limitation of the conversion, density and fall speed of graupel and also including the parameterization for F_{BR} based on the collisional kinetic energy (e.g., Phillips et al., 2017; Sotiropoulou et al., 2020) instead of assuming a constant F_{BR}, such sensitivity studies are beyond the scope of this study and need to form part of a separate study.

We added the text below to the paper.

Methods

The two-moment bulk microphysics scheme has been used extensively to study the evolution, lifetime, persistence and aerosol-cloud interactions of MPCs (Seifert et al., 2006; Baldauf et al., 2011; Possner et al., 2016; Lohmann et al., 2016; Possner et al., 2017; Henneberg, 2017; Glassmeier and Lohmann, 2018; Sullivan et al., 2018; Eirund et al., 2019a, b). We refer to ice particles as any combination of the hail, graupel, snow or ice categories. The size distributions of the hydrometeors, except for the raindrop category which is described by an exponential distribution, are described by a generalized gamma distribution. Cloud droplet activation is based on an empirical activation spectrum which depends on the cloud-base vertical velocityand the prescribed number concentration of cloud condensation nuclei (Seifert and Beheng, 2006). The application is appropriate in atmospheric models with a horizontal grid size and time resolution of $\Delta x \leq 1\,km$ *and* $\Delta t < 10\,s$ *respectively. Condensation and evaporation represented by a saturation adjustment approach is applied after the microphysical conversion rates. The warm-phase autoconversion process from Seifert and Beheng (2001) was updated with the collision efficiencies from Pinsky et al. (2001) and also takes into account the decrease in terminal fall velocity associated with an increase in air density. A better approximation of the collision rate between hydrometeors was also introduced by Seifert and Beheng (2006), which makes use of the root mean square values instead of the absolute mean values of the difference between the fall velocity of the colliding hydrometeors from the Wisner-approximation (Wisner et al., 1972).*

The freezing of all cloud droplets occurs at temperatures colder than 223 K. The homogeneous freezing of cloud droplets which is strongly dependent on temperature, occurs at warmer subzero temperatures. Another pathway is the homogeneous nucleation of solution droplets that are typically associated with cirrus cloud formation. The formulation of homogeneous nucleation of solution droplets follows Kärcher et al. (2006) which determines the number density and size of nucleated ice crystals as a function of vertical wind speed, temperature and pre-existing cloud ice. A critical supersaturation must be reached in which nucleation events can occur if the updraft is stronger than a threshold value determined by the radius and number density of the pre-existing ice crystals (Eq. (19) of Kärcher et al., 2006).

Discussion

The representation of graupel also has other limitations. For instance, graupel has a fixed density and fall speed parameters, which is a less desirable representation of graupel that could substantially impact the collisional tendencies between graupel and other ice particles, affecting SIP rates. It is worth considering the snow-graupel size conversion threshold and the graupel fall speed in further sensitivity studies when SIP processes are used in two-moment bulk microphysics schemes. Another limitation of bulk microphysics schemes is that the variability of observed drop size distributions in different regions within the same cloud or between different clouds is approximated by a gamma distribution with a constant shape parameters (Costa et al., 2000; Geoffroy et al., 2010; Khain et al., 2015, and references therein).

A further consideration is to switch to a property-based microphysics scheme which focuses on the continuous evolution of physical properties (e.g., density) of the ice particles (e.g. Morrison and Milbrandt, 2015; Milbrandt and Morrison, 2016; Jensen et al., 2017; Phillips et al., 2017; Sotiropoulou et al., 2020) which can support a more realistic representation of collision breakup in orographic MPCs. A property based scheme would also eliminate the limitation of two-moment bulk microphysics category-based hydrometeors. In the case of studying collisional breakup in summertime MPCs, the use of a three-moment bulk microphysics scheme can be beneficial, albeit much more computationally expensive, for simulating large hail (Milbrandt and Yau, 2006; Milbrandt and McTaggart-Cowan, 2010; Loftus and Cotton, 2014). These researchers showed that accurately reproducing the tail of a hail particle size distribution, which two-moment bulk microphysics schemes cannot replicate, is required for simulating large hail.

A popular assumption exists that bulk microphysics schemes (e.g., the Seifert and Beheng (2006) scheme used in this work) are inherently less accurate than spectral bin microphysics schemes, which is not always the case. The main advantage of bulk microphysics schemes is their computational efficiency, being a factor of ~ 30 computationally less expensive and making them much more appealing than spectral bin microphysics schemes. When ice-phase microphysics is considered, Seifert et al. (2006) showed the strength of the spectral bin scheme in simulating a squall line. Still, when the nucleation treatment of the spectral bin scheme was diagnosed with a "bulk cloud condensation nuclei" scheme, similar results were obtained in the accumulated precipitation to that of the bulk microphysics scheme. In addition, Xue et al. (2017) showed that spectral bin schemes, designed to be conceptually more realistic and therefore more accurate than bulk microphysics schemes, are quantitatively similar to the spread of bulk microphysics schemes when simulating the microphysical, thermodynamic, and dynamic characteristics of a squall line. Khain et al. (2015) came to a similar conclusion. Therefore, it is within reason to expect that it is sufficient to study SIP with a two-moment bulk microphysics scheme.

Minor Comments

- I suggest that the author make a clearer distinction in the text when referring to natural ice-phase hydrometeors vs. categories in the bulk scheme. This is a minor but important point, in my opinion. Bulk schemes do not have ice crystals; they have bulk categories with prognostic bulk quantities. Not making the clear distinction in the wording can invite readers (and researchers) to make inappropriate comparisons (e.g. between observed ice crystals and model “ice”, “snow”, or “graupel”.). In several places in the text, the authors refer to model “ice crystals” and it is difficult to tell if the mean ice-phase hydrometeors (of any category) or specifically the “ice” category.

  AR: We agree. When we refer to the ice category from the bulk scheme we mention ice instead of ice crystals. In addition to these changes we have added the following sentence in the Methods section:

  We refer to the ice particles as any combination of the hail, graupel, snow or ice categories.

- The writing is very good in general. Just two suggestions: 1) In a few places it says something like "All of the simulations do not capture…" – it would sound more natural to write this as "None of the simulations capture…". [e.g. line 416, 419, and elsewhere] 2). There a few cases of contractions (e.g. "can't") where as it should read (e.g.) "cannot". [e.g. line 234]

  AR: We corrected these grammatical issues.

- Line 121: Suggest "grid spacing" instead of "resolution".

  AR: We modified the text.

- Line 153 (and reference list), "Milbrandt and Morrison, 2015" – should be 2016.

  AR: Thanks, I updated my reference list

- Line 324, "Not as obvious, .." This is not actually a sentence.

  AR: I removed those few words and adapted an error in this sentence after:

  The BR2.8_T precipitation intensity lags behind that of the BR28 simulation over the flow-oriented vertical cross-section...

- Line 355, "… we have shown that COSMO benefits…" (and similar statement in the conclusion [line 427]). I suggest changing this to "… we have provided evidence that COSMO may benefit…". There are too many uncertainties in the model and in the S-B scheme itself (not to mention the SIP parameterizations) to make this claim definitively. Models are a system of compensating errors, so making change that results in an improved result may not be systematic and may not be due to genuinely improved representation of physics.

  AR: Yes, we agree and have modified the text accordingly.

- Fig. 9: This is a great figure! 2 minor suggestions: 1) in the text at the top, put "Lower ICNC" first (since the larger diameters are the result, not the cause). 2). The term "sedimentation velocity" is probably not quite correct, given that this arrow refers to the vector sum of sedimentation plus horizontal advection. Perhaps "hydrometeor trajectory" would be better, and explain this point in the caption.

  AR: We have adapted the figure (now figure 11). We changed the arrows to point vertically to show sedimentation velocity.

- Fig. 11: The units (and description) in panels a) and e) do not seem correct. Is this (a)) the column-integrated graupel mass – i.e. "graupel water path"?

  AR: Yes, it is the graupel water path. We do not see the issue with the units in panel e). We adapted panel a) and the description (now figure 10).

- Line 430 (and a couple other places), “secondary ice parameterization”: Should read “secondary ice production [or SIP] parameterization”.

  AR: Yes, we adapted the text.

- Line 422: “represented” should probably be “reproduced”. (Also, line 440, plus one or two other places.)

  AR: We adapted the text with the suggestion.

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