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.

RC: Reviewer Comment

AR: Author Response

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 $2\,cm^{-3}$ 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 ($\sim 1500\,m$, -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 $10^{-1}\,g\,m^{-3}$ 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 ($\sim 1500\,m$, -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-

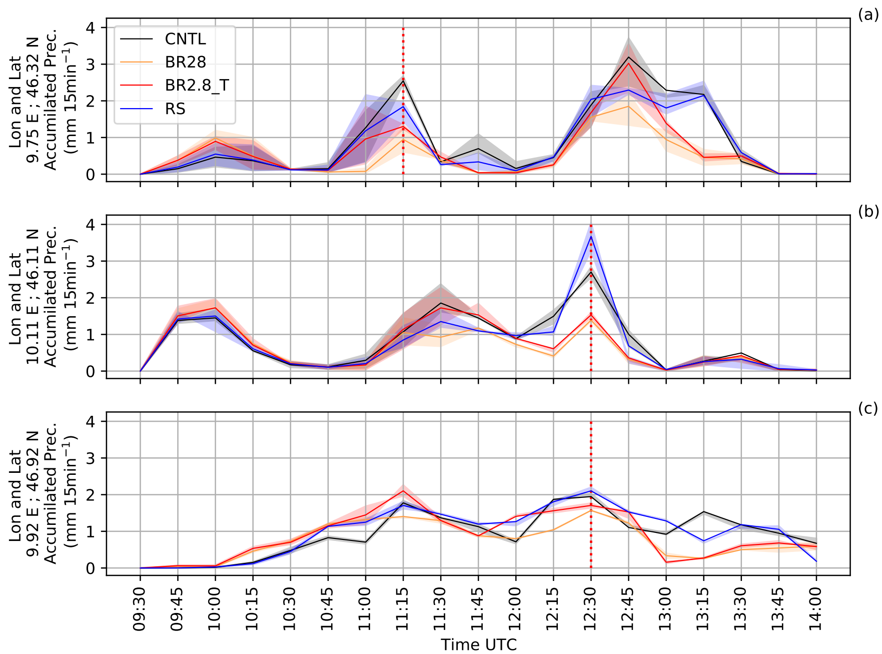

Figure 1. Three localized regions of high precipitation in the CNTL simulation which are suppressed in the collisional breakup simulations. The solid lines are the model mean and the shaded regions are the minimum and maximum values for the four closest model points. The red dotted lines are the selected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

Table 1. Interquartile range (IQR) between the 25th and 75th percentiles and median in $\mathrm{mm\,h^{-1}}$ 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 $\mathrm{mm\,h^{-1}}$ and underestimating the density between 1 and 2 $\mathrm{mm\,h^{-1}}$ 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

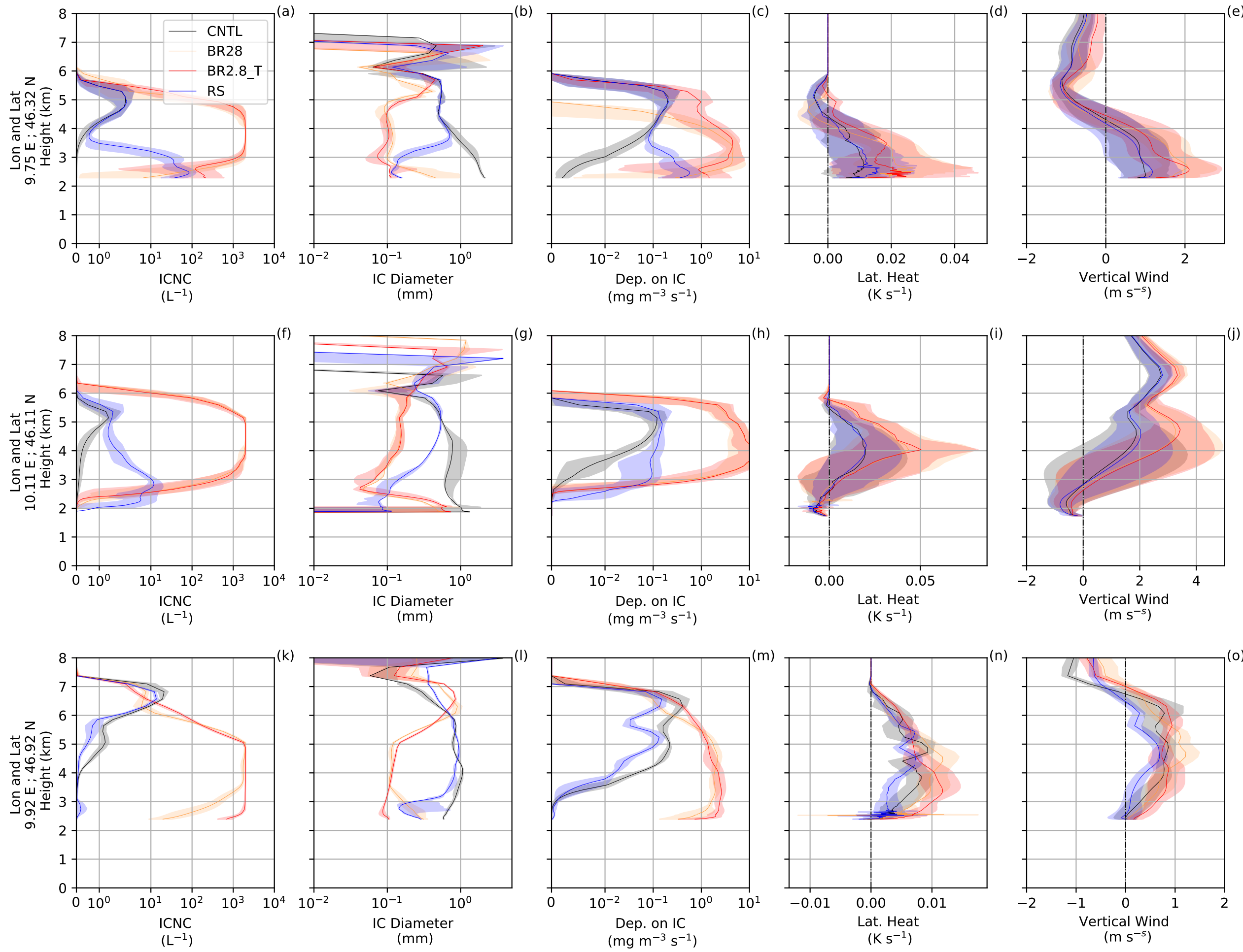

Figure 2. Vertical profiles of the a), f) and k) ice crystal number concentration (ICNC), b), g) and l) ice crystal diameter, c), h) and m) depositional (Dep.) growth of ice crystals, d), l) and n) latent (Lat.) heat release and e), j) and o) vertical wind velocity. The solid lines are the model mean 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 $2\,m\,s^{-1}$ 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 3.5×10^{8} (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 $g\,m^{-2}$ 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 $\sim$*7.6* $m\,s^{-1}$ *is necessary to resuspend snow particles. More recently in the Swiss Alps, Walter et al. (2020) interpolated a threshold value of* $\sim$*7.5* $m\,s^{-1}$ *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* $m\,s^{-1}$ *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* $m\,s^{-1}$ *for the largest part of our analysis period (Fig. 3). However, between 12:45 and 13:30 UTC when winspeeds exceeded 7.5* $m\,s^{-1}$*, 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.

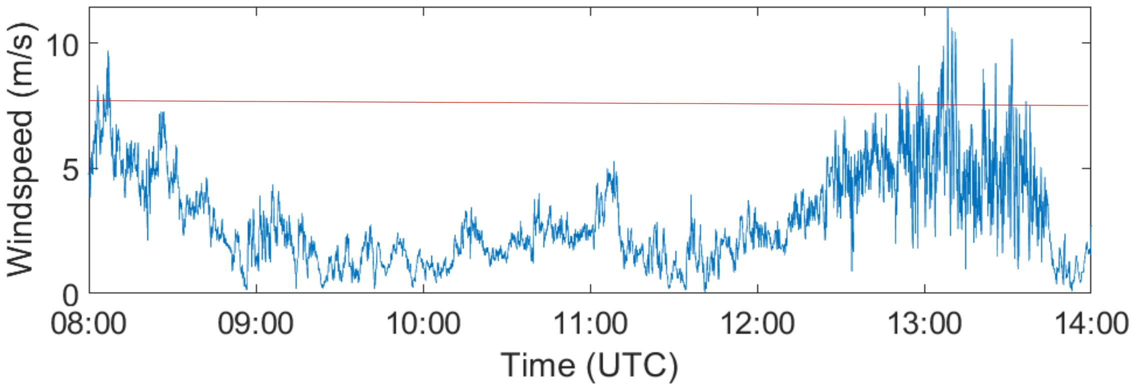

Figure 3. Wind speed recorded by the snow drift station on 7 March 2019. The red line is defined as the threshold to re-suspend snow particles (Walter et al., 2020). Data availability (Walter et al., 2019).

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