Heavy snowfall event over the Swiss Alps: Did wind shear impact secondary ice production?

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We thank both reviewers for the constructive feedback. The additional suggestions and comments improved the readability 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.

Reviewer 1: Major comments

1. Abstract: Can you start with a more general sentence in the abstract? What are you trying to understand or elucidate in this study?

   We agree and have added the following few sentences for a more general audience.

   The change in wind direction and speed with height, referred to as vertical wind shear, causes enhanced turbulence in the atmosphere. As a result, there are enhanced interactions between ice particles that break up during collisions in clouds which could cause heavy snowfall. For example,

2. Lines 65-85 – I appreciate that these conceptual definitions of the remote sensing variables have been added; indeed, I requested that during the first round. However, placed here, it interrupts the flow of the study motivation and approach, and I think it belongs better in Section 2.1.

   We moved the section to Section 2.1.

3. Lines 153-155 – "The model output was interpolated along the mean of three vertical cross-sectional paths, ≈ 3 km wide … The cross-sections of the simulations were averaged to create a mean cross-section." I was a bit confused about this during the first read-through also. Figure 1 shows only two such cross-section paths, as far as I can see. How was

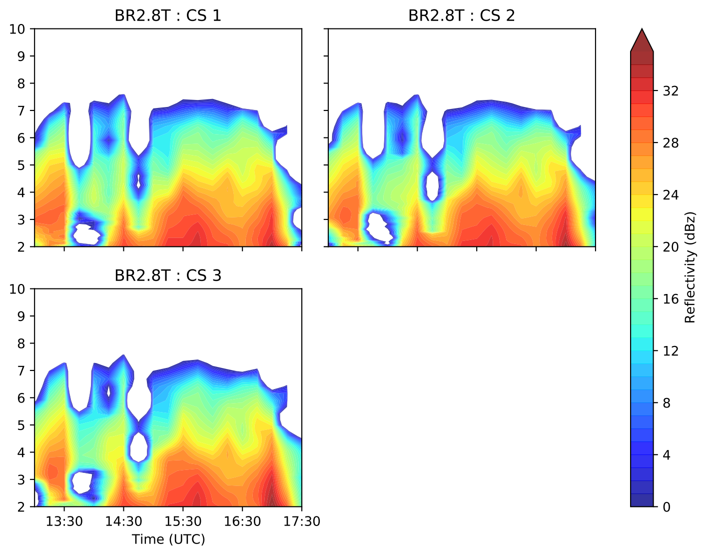

Figure 1. Hovmöller diagrams of the horizontal reflectivity (Z_{H}) from the Doppler radar for the BR2.8T simulation between 13:00 and 17:30 UTC. The variability between the three cross-sections (CS) is illustrated in the panels for CS1, CS2 and CS3. Note that the color bar is different from the figures used in the manuscript to better show differences between the CSs of the BT2.8T simulation.

a separation of 3 km between these paths chosen? And it seems like one of the cross-sections cuts more or less directly through the Doppler radar location; is that right? I am also curious about the variability between the three cross-sections. I guess this is as fine a method as any to compare to radar data, but I would appreciate another sentence or two explaining its setup and uncertainty.

The cross-sections shown in Fig. 1 are the outer limits of the cross-sections. Not shown in Fig. 1 is another cross-section which is between the outer cross-sections. The separation between each cross-section is about 1 km which is similar to the model resolution of 1 km. Yes, one of the cross-sections cuts more or less directly through the Doppler radar location. The variability between the cross-sections is small in this case study (Fig. 1), but a necessary step especially when comparing observations to model simulations in mountainous regions. The section was modified to include more setup details.

To account for the uncertainty associated with simulating atmospheric processes in mountainous terrain, three cross-sections were interpolated from the model output (only the outer two cross-sections are shown in Fig. 1). Each cross-section, of which one cross-section cuts more or less through the location of the radar, is separated by 1 km which is similar to the model resolution. The direction of each of the cross-sections is similar to the direction of the generated RHI cross-section from the weather radar. The three cross-sections are then averaged and compared to the radar data.

4. Lines 183-185 – "During collisional breakup graupel collides with either ice and/or snow particles and fractures." I would start here with a more general definition of collisional breakup. A version between ice and graupel is being employed here, but it refers more generally to the collision of any two frozen hydrometeors in which the density difference (and hence terminal velocity difference) is sufficient that the collisional impact causes shattering.

   Thank you for the suggestion. We have added the following text to the manuscript.

   Generally, collisional breakup refers to the collision of any two frozen hydrometeors with different densities in which the collisional kinetic energy is sufficient that the collisional impact causes shattering. Here, collisional breakup is when either ice and/or snow particles collide with graupel and fracture.

5. Lines 185- 192 – I would refocus / remove these sentences to make two points. First, almost no empirical constraints exist for the efficiency of any form of collisional breakup, and second, from a theoretical standpoint, collisional kinetic energy between the hydrometeors is the key parameter for this efficiency. These points are more fundamental than discussions of ice-graupel, ice-snow, etc. because these distinctions are artificial and will disappear as we transition toward particle properties schemes.

   This is true. We modified this section with the following:

   Almost no empirical constraint, apart from Takahashi et al. (1995) who used collisions between hail-sized particles, exists for the efficiency of any form of collisional breakup. The collisional kinetic energy and the density between hydrometeors are important parameters for this efficiency.

6. Line 211 – This has become quite a long paragraph. I would break after "from Sullivan et al. (2018) where D0 = 0.02m" and add a transition sentence that indicates that "Previous studies indicate that artificial thresholds for hydrometeor numbers or conversion rates may strongly influence the output of secondary ice production parameterization."

   We included the break in the paragraph.

7. Line 230 – As you introduce the case study here, I think it is important to mention some time scales already. You will be looking at about 4 hours of front evolution with high-intensity snowfall concentrated over 1.5 hours during that time.

   It is a good suggestion and we included the timescale in the modified section below.

   A synoptic system passed over Switzerland on 26 March 2010 during which we analyze the evolution of a cold front from 13:00 to 17:30 UTC with intense precipitation from 15:00 to 17:30 UTC. Furthermore, the cold front was associated with a surface temperature drop of $\sim 7\,^{\circ}C$ (Fig. S1), a south-westerly wind flow at higher altitudes, vertical wind shear closer to the surface below 4 km amsl and the development of peculiar polarimetric radar signatures (e.g. an intense K_{dp}).

8. Lines 289-290 – Is this a single, time-averaged observation of ICNC from the disdrometer?
   The green triangle in the figure referenced here indicates the total volumic number concentration from the 2DVD computed at a time scale of 5 minutes (meaning that an observation at 15:30 UTC is representative of data between 15:25 and 15:30 UTC). The temporal resolution is now explicitly mentioned in the text.

9. Lines 345-346 and Lines 349 – I have some reservations here about your wording in regard to the role of V-wind shear. Covariability does not necessarily imply that a factor is a "determinant" or that it "played a major role." I might limit the statement to something like "Environment of high meridional wind shear tend to be the same environments of high secondary ice production rates."
   We agree with the reservation and have modified the manuscript.

Reviewer 1: Minor comments

1. Line 2 – "high precipitation" – high intensity precipitation?
   We modified the text to say high-intensity precipitation.

2. Lines 33-35 – "The enhancement of smaller ice particles triggers an increase in the combined growth rates (riming and deposition) of up to 33% resulting in larger latent heat release. . . When ice-ice collisions occur in wintertime orographic MPCs, the general tendency is for riming to decrease." To me, there are two contradictory statements being made here. Do existing results indicate that riming rates decrease or increase in the presence of secondary ice production? Or is this dependent on the fragment sizes being generated?
   It appears to be contradictory, however, Dedekind et al. (2021) show that the increased combined growth rates of ice particles include a strong reduction in the riming rate overshadowed by an even stronger enhancement in depositional growth rate. The results indicate that riming decreases in the presence of secondary ice production because the fragmented ice particles are smaller limiting their interaction with supercooled liquid water. We adapted the text to make this clearer.

3. Line 53 – new paragraph after "once they reach a size of 200 um"? You are transitioning to discuss remote sensing technique to study these processes.
   Yes, we started a new paragraph.

4. Line 57 – "Two non mutually exclusive approaches can be found in the literature." I appreciate that the authors have added more literature review regarding use of remote sensing to infer secondary ice production. It was not clear to me

why the two approaches were not mutually exclusive?

The approaches are not mutually exclusive meaning that some radar systems have the capability to do both. We rephrased this sentence to remove the phrasing *non mutually exclusive*, as we agree that it is possibly confusing for the reader.

5. Line 63 – "Additional information (in-situ, models, or a combination of more radars)..." in-situ data

   Yes.

6. Line 66 – "The waves interact with precipitation" But also with suspended condensate, right?

   That is correct, we added it.

7. Line 75 – new paragraph after "as is the propagation speed of the waves"? Then you separate discussion of reflectivities from that of differential phase shift.

   A new paragraph was started.

8. Line 170 – "The primary production of ice formation is described by.." Primary ice production occurs via..

   We modified the text accordingly.

9. Lines 178-179 – "Secondary ice production through rime splintering is the only process that is included in the standard version of COSMO which has been used extensively in other numerical weather models" I think you are trying to say that rime splintering implementations are fairly widespread in other numerical weather models than COSMO, but the wording is confusing. I would remove which has been used extensively in other numerical weather models.

   Correct, we modified the text to:

   Secondary ice production through rime splintering, which is widely used in numerical weather prediction models, is the only process...

10. Lines 202-204 – "Only using ice-graupel collisions would limit the full description of SIP as a result of wind shear when graupel formation becomes restricted." I do not understand what this means. Can you clarify somehow?

    Yes. Only using ice-graupel collisions, and not ice-snow and ice-ice collisions, means that SIP might be underestimated in an environment of strong wind shear. However, we removed this sentence because it creates confusion within its context.

11. Lines 224 – “For this purpose a 10 km x 10 km region was selected and masked by the levels in which SIP occurred (T > -21C, blue box in Fig. 1)”
The additional information was added.

12. Figure 2 caption – “The blue and red colors denote wind blowing towards and away from the radar” I don’t see blue and red? I’m understanding the blue-brown shading to be horizontal wind relative to the mean cross-section and the blue-brown contours to be vertical wind. I am also not really extracting “the shaded gray area” which denotes cloud area fraction.
We updated the caption and removed the cloud area fraction from Fig. 2.

13. Line 241 – (Figs. 2**c**-e) Fig. 2b is not a collisional breakup simulation.
We corrected the reference.

14. Line 243 – Missing reference
The link to the reference was corrected.

15. Line 274 – Missing reference
The missing reference was updated as well as other incorrect references to figures in this section.

Reviewer 2: Minor comments

1. I agree with the authors response to my comment 2, however, it is still unclear to me how evidence of SIP was observed. As I commented in the previous review, SIP might be explained as high Kdp collocated with low Zdr, which could be explained by large number concentration of oblate, smaller ice particles (high Kdp) and near-spherical, large snow (low Zdr). This could be observed between 16:21 and 17:21 UTC (Fig.3). However, Fig. 7e showed peak of Zdr is consistent with the peak of Kdp. This could represent slightly-oblate large ice particles, rather than different shape/size of particles.
SIP can be generated by various collisional breakup mechanisms, possibly generating different radar signatures. For example, at around 15:30 UTC collisional breakup was driven by the presence of graupel as opposed to collisional breakup driven by fragile ice aggregates which occurred later during the day. The collisional breakup parameterizations used in this study only included ice-graupel collisions (collisions of particles of different densities). Therefore, the simulations are only able to replicate SIP during the early phase of the event when graupel was present. For this reason, we focussed on the earlier phase of the event. We agree with the reviewer that the second phase of the event more closely resembles the expected SIP signature (high Kdp, low Zdr). At the same time, we believe that this is associated with the breakup

of fragile hydrometeors in the absence of graupel, a mechanism that the model is not able to replicate which should definitely be addressed in future research and compared with these prominent radar signatures (especially K_{dp}). In the conclusion section, this is discussed as follows:

Another time period with high K_{dp}, but low Z_{DR} was observed at 17:00 UTC which was not captured by the breakup simulations because the graupel mixing ratio was depleted. The breakup parameterization does not include ice-ice collisions and relies only on graupel as the collider specie. At this time the radar signatures suggested that collisions of dendrites caused the formation of small oblate particles (increasing K_{dp}) but also the formation of a few, larger, isotropic aggregates (decreasing Z_{DR}).

The presence of dendrites was highly likely given the favorable temperature range (discussed earlier in the manuscript). At 15:30 , the model shows that ice-graupel collision is the dominant mechanism. The radar-based hydrometeor classification suggests the presence of rimed hydrometeors, corroborating this hypothesis, and the 2DVD shows a high number concentration and the absence of a significant number of big particles (see also the PSD spectra later in this document, so we can rule out the possibility of "slightly-oblate large particles" dominating the radar signal). Therefore, in an environment where graupel-ice collision SIP is taking place, we can have the presence of a large number of oblate, possibly rimed, particles (increasing K_{dp}), co-located with positive Z_{DR} if the largest particles (rimed particles and graupel) are oblate. Also, it should be noted that Z_{DR}, at this time step, is positive but not extremely large (around 1 / 1.5 dB). Populations of rimed particles with signatures in this range are documented for example in Grazioli et al. (2015); Besic et al. (2016). In the revised manuscript, while describing the radar measurements (Sec. 3.2.1), we rephrased this as follows:

The vertical evolution of K_{dp} and Z_{DR} is similar, with a peak observed about 4 km amsl, which is 1 km above the peak in Z_{H} (Fig. 7d). The large and colocated values of Z_{DR} and K_{dp} suggest that a large population of oblate and rimed particles, without a significant presence of large isotropic hydrometeors, were present.

2. Why did you select that time (15:30) for Fig.7e?

   As mentioned above, this time step was selected because of the presence of graupel/rimed particles (both in the radar hydrometeor classification and in the model) that could lead to significant breakup-driven SIP. At later time steps, though interesting to study for the strong radar signatures, graupel was depleted and therefore the model, which requires graupel to initiate collisional SIP, cannot reproduce the observations. This discrepancy is in itself a result of this work and highlights the current limitations of the model. Therefore, it was discussed in the conclusions (see also the previous answer).

3. Also, I am wondering if 2DVD observed particle size distributions that represented SIP; for example high number concentration of smaller particles coexisting with large particles compared to non-SIP periods.

   We must be cautious in the interpretation of 2DVD PSD spectra in terms of SIP, given the resolution of the instrument (0.2 mm, which cuts out the most interesting part of the distribution of small particles) and the absence of significant literature/methods for SIP detection using this imager. This is the main reason why we only used the 2DVD to retrieve hydrometeor number concentrations at the ground level. This said it is indeed interesting to look at the spectra of this case study (see Fig. 2 of this document). We compare here a size spectrum collected at 1530 UTC with one collected at 1720 UTC. Both show a very large number of small particles (and we must keep in mind that particles smaller than 0.2 mm are not measured) but also larger particles of a few mm. The spectra collected at 1720 UTC has more large particles, which is compatible with the hypothesized presence of aggregates of dendrites in this time interval. Given that the amount of information that we can extract from the spectra is relatively minor regarding SIP, we decided not to include this type of data in the manuscript.

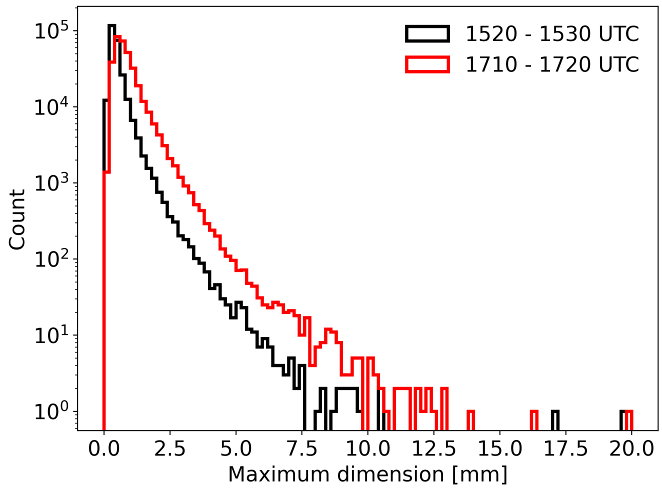

Figure 2. Two spectra of distribution of particle size measured by the 2DVD near the ground at two different time steps, each collected over 10 minutes of measurements. Note that this is a histogram of particle sizes crossing the measurement area in the given time and not strictly speaking a volumic PSD.

4. I do not still know what the numbers mean for the simulation names, i.e. BR28 and BR2.8T. This may be explained in Dedekind et al. (2021), but if it would be also explained in the current text, it would be helpful.
   Table 1 was included in the text which describes the simulations.

Table 1. Sensitivity settings for the collisional breakup (BR) parameterization. The conversion rate (conv) is the size, in µm, at which rimed ice or snow are converted to graupel, α is the scale factor, F_{BR} the fragments generated and γ_{BR} the decay rate of fragment number at warmer temperatures. When $\gamma_{\mathrm{BR}} = 5$, as used in the Takahashi parameterization, then T is included in the simulation name.

conv	α	F_{BR}/α	$\gamma_{\mathrm{BR}} = 5$	$\gamma_{\mathrm{BR}} = 2.5$
200	1	280	BR-Sot	
200	10	28		BR28
200	100	2.8	BR2.8T	
300	100	2.8	BR2.8T_300	
400	100	2.8	BR2.8T_400	
500	100	2.8	BR2.8T_500	

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