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

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We sincerely thank Reviewer 1 for the constructive feedback. The suggestions and comments 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.

General comments

1. I felt that a small, general overview of the use of remote sensing to study secondary ice production in general, or ice-ice collisional breakup in particular, was missing from the Introduction. For example, I think the recent study of Luke et al. (2021) using longer-term ground-based remote sensing to infer secondary ice production strength would be worth mentioning.

   We agree with the suggestion of the reviewer and with the fact that an overview of the use of remote sensing to study SIP is needed. We expanded the introduction to include an overview of the subject. We refer now to Zawadzki et al. (2001), Oue et al. (2015), and Luke et al. (2021) (highlighting however that their approach made use of Doppler spectra, not available to us, rather than dual-polarization bulk measurements. For this second (our) approach we refer to Hogan et al. (2002); Andrić et al. (2013); Sinclair et al. (2016); Kumjian and Lombardo (2017). This part of the Introduction in the revised manuscript now reads:

   Remote sensing from weather radars has been used to study snowfall microphysics and hydrometeors' habit (e.g., shape, phase or hydrometeor type). Although radar observations do not provide a direct information on SIP, a few studies leveraged the Doppler and/or dual-polarization capabilities of weather radars to identify the occurrence of SIP and to speculate case-by-case on the possible mechanisms behind its origin. Two non mutually exclusive approaches can be found in the literature. Zawadzki et al. (2001); Oue et al. (2015); Luke et al. (2021) exploited Doppler spectra collected by vertically-pointing radars to identify the appearance of secondary populations of particles at given altitudes or tem-

perature levels. Other approaches (Hogan et al., 2002; Andrić et al., 2013; Sinclair et al., 2016; Kumjian and Lombardo, 2017) focused on the interpretation of the signature of dual-polarization variables and their respective evolution over the vertical column of precipitation. This second approach, used in this study, leverages the fact that dual-polarization variables are complementary and affected in a different way by changes in number, shape, size and density of hydrometeors.

2. The two prior studies mentioned (Grazioli et al., 2015b; von Terzi et al., 2022) are also employing specific differential phase shift, which is not defined in terms of its information content. Only differential and horizontal reflectivity are defined. I think the definitions in Section 4 of Field et al. (2016) are quite nice; perhaps some variant of those could be included here. ("By transmitting horizontally and vertically polarized waves and looking at the differences in power and phase between the echoes in each polarization, information about the orientation and/or phase of the hydrometeors being probed can be obtained." "Just as the backscatter is different for horizontal and vertical polarizations in the presence of oriented ice crystals, so too is the speed at which the radar wave propagates through the cloud. This leads to a small phase shift between the horizontal and vertical polarized echoes.")

   The reviewer has a valid point here. K_{dp} was not properly defined and the overall introduction of the polarimetric variables was not adapted to a broad audience (as we would like this manuscript to be understandable both for experts of the numerical model and remote sensing communities). This part of the Introduction section is rewritten in these terms:

   Not only the backscattered power is different for horizontal and vertical polarizations in the presence of anisotropic particles, but also the propagation speed of the waves. The rate of change in phase shift between the horizontal and vertical polarized echoes is expressed by the specific differential phase shift K_{dp} [$^{\circ}\mathrm{km}^{-1}$]. This variable is complementary and not redundant: it is in fact not affected by the absolute calibration of a radar and is less affected than Z_{DR} by eventual presence of large isotropic particles within the sampling volume. Local K_{dp} enhancements in snowfall have been documented (Schneebeli et al., 2013; Bechini et al., 2013) and in some cases associated to SIP (e.g., Andrić et al., 2013; Grazioli et al., 2015a; Sinclair et al., 2016).

3. I had some difficulty to follow the arguments in Section 3.1.1. I am thinking that the RS process is muted in the simulation because there are insufficient droplets of the correct size. Then depositional growth or aggregation dominates growth and broadens the ice crystal size distributions, promoting the size sorting? But a droplet limitation is not explicitly mentioned, and to me the chain of events in the model is "limited riming + strong aggregation / depositional growth -> broad ice size distributions -> size sorting", whereas the size sorting is described first in the text before its causes. Could the ideas be rearranged in this subsection to follow the argument?

   Yes, we agree with the chain of events you have pointed out and adapted the section with the following text:

   Z_{H} was significantly overestimated by the RS simulation between 13:00 and 17:30 UTC which most likely was a result of the following chain of events. 1) Insufficient droplets of size 25 μm (Fig. 5d), within the narrow temperature range

($-3 \geq T \geq -8\,°C$), led to a limitation in ice particle growth by riming and therefore limited rime splintering (Fig. 6f). 2) Because rime splintering was not that active, typical for wintertime MPCs (e.g., Henneberg et al., 2017; Dedekind et al., 2021), the ice and snow crystals grew mainly by depositional growth and aggregation. 3) The ice and snow crystal size distributions widened substantially (Figs. 6a, b and S2a, b). These categories both had number concentrations less than 100 L^{-1} with particle diameters of up to 0.8 and 5.1 mm, respectively, at 15:30 UTC. 4) The larger ice and snow crystal diameters resulted in enhanced Z_{H}. These observation is consistent with other times during the day which showed even larger sized ice and snow crystals of 0.9 and 5.2 mm, respectively (Figs. S3a and S4a) as well as higher rain mass mixing ratios (e.g. Fig. 5a). There were single grid points where snow crystal even reached diameters of 13 to 17 mm during the latter part of the day (not shown here). Additionally, excessive size sorting in the model most likely contributed to the overestimation in Z_{H}. Size sorting typically occurs within the sedimentation parameterization of 2M schemes in regions of vertical wind shear or updraft cores (Milbrandt and McTaggart-Cowan, 2010; Kumjian and Ryzhkov, 2012). All these factors contributed to the RS simulation overestimating Z_{H} by at least 8 dBz throughout the vertical profile compared to the observations (Fig. 7d).

(a) As a sub-comment here, I am still confused by the contradicting reflectivity (Fig 2) and ice crystal number (Fig 4) results. How can the reflectivity from the RS simulation be so off when its crystal number and IWC are reasonably accurate? This is really just the product of a “shortcoming in the derivation of NICE from the radar obs”?

There are a several reasons why the reflectivity in the RS simulation is overestimated: 1) Rime splintering was very low, which is typical for wintertime MPCs (Fig. 7f here and e.g., Henneberg et al., 2017; Dedekind et al., 2021), the ice and snow crystals grew mainly by depositional growth and aggregation to much larger sizes compared to the collisional breakup simulations (Fig. 6c). The snow crystals, especially, were double the size (Fig. 6b, over 2 mm in diameter). Below we show Hovmöller plots (Figs. 1 and 2) of the diameters for ice crystals, snow, and graupel from 15:00 to 17:30 UTC showing the snow and graupel particles reaching diameters of over 4 mm and 8 mm, respectively. 2) The rain mass and number concentrations were also substantially higher in the RS simulation. All these factors may have contributed to the large overestimation of Z_{H} apart from the issues that arise from size sorting which is discussed in the manuscript. The adapted manuscript now includes Figure 3 showing K_{dp}, Z_{DR} and Z_{H} over the entire period. The large K_{dp} values (see the reference values reply on the Specific Comments section point 1) implies large number concentrations of ice particles which is not consistent with the RS simulation (e.g., at 15:30 UTC as shown in Fig. 6). The observed ice crystal number concentrations, collected at the surface by the disdrometer, are also not in agreement with the RS simulation. These reasons indicate that the reflectivity will be overestimated in the RS simulation. We now discuss to the main assumptions and limitations which can cause shortcomings in the derivation of the IWC and NICE from the radar observations in Section 2.1.

4. Again, in the ordering of results, it would have made more sense to me to show Figure 7 and some of the results in Section 3.2 prior to any cloud fields. It is a bit hard to tell from the colorbar in Figure 7, but it seems to me that midlevel

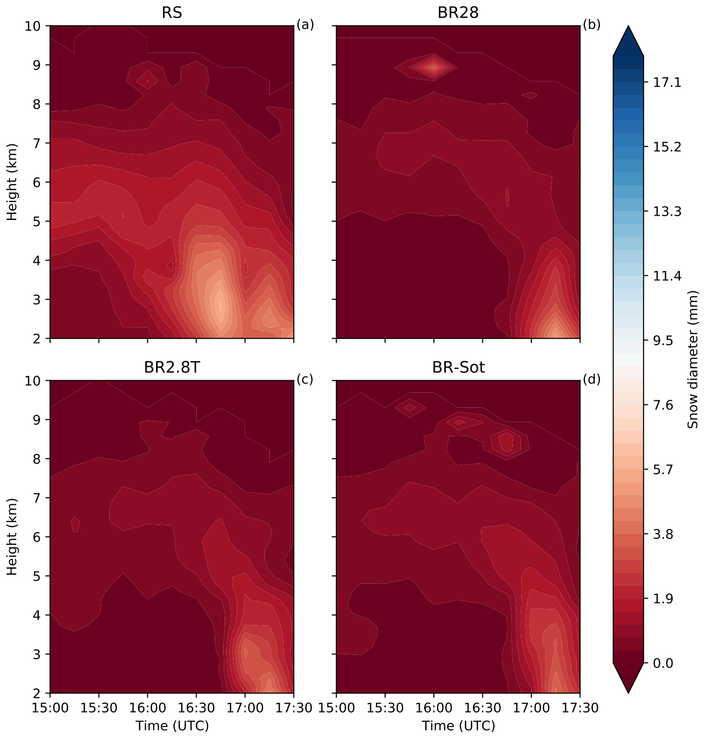

Figure 1. Hovmöller diagrams of snow diameters for panels **(a)** RS , **(b)** BR28, **(c)** BR2.8T and **(d)** BR-Sot between 15:00 and 17:30 UTC.

(3-5 km) wind speeds are being overestimated pretty much by the model. If there are strong biases in the wind field, then we cannot expect agreement in the cloud (microphysical) fields.

We agree in presenting these results earlier in the manuscript. Concerning the wind biases, there certainly appear to be stronger biases in the wind fields and the reviewer is certainly correct in raising the issue that expecting agreement in the microphysical fields is futile if the wind fields do not compare well. However, the interpretation of the model data should be interpreted in the following light: 1) the model output is used to generate three cross-sections (the entire width of the three cross-sections is ≈3 km wide) which are interpolated along the direction of the RHI of the Doppler radar (Fig. 1). The mean of the three cross-sections yields the mean cross-section for each simulation. Using this method may generate biases; 2) Comparing results close to the surface in valleys and on the peaks of mountains over complex terrain is challenging because of the differences in the model topography and the actual topography (e.g., Goger et al., 2016). E.g., the altitude of the Doppler radar location (9.843°E, 46.789°N) is 2132.5 m compared to the closest model grid point (9.837°E, 46.79°N) of 1729 m. Additionally, the Doppler velocity also includes the falling z-oriented component of the hydrometeors which can generate more uncertainty in the comparison to the simulations.

Specific comments

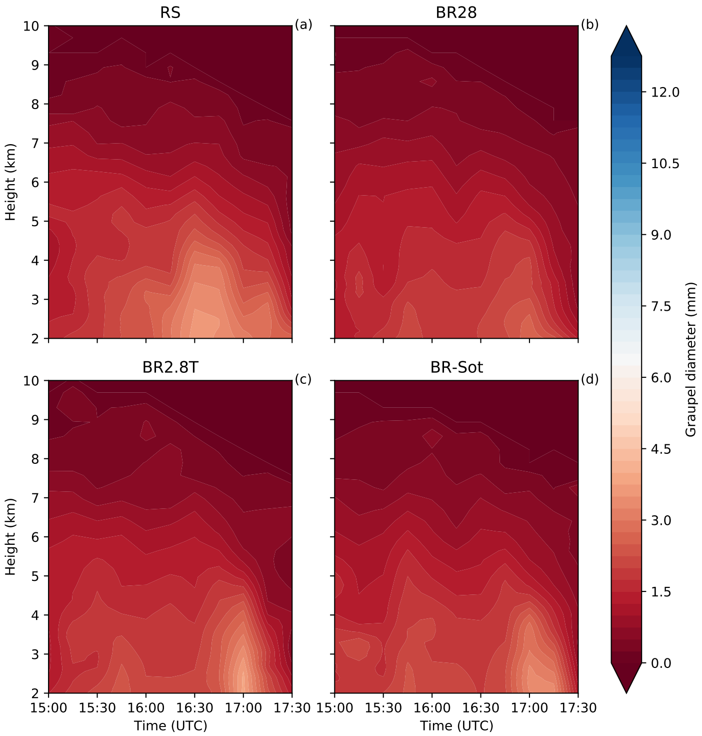

Figure 2. The same as Figure 1, but for graupel.

1. Line 78-79 Again, a definition of Kdp and a basis for comparison for values of 1.5° and 2° km^{-1} would be helpful for readers who are non-experts in radar.

   We decided to provide some reference values to put these number into context. We now cite the work of Schneebeli et al. (2013), who compiled a statistical analysis of polarimetric variables for this exact location and this radar system for a time span which includes the current case study.

   K_{dp} scales with radar frequency. A statistical analysis of K_{dp} in snowfall conducted with this radar and in this location over a long observation period (Schneebeli et al., 2013), showed that the 80% quantile of K_{dp} at every height level is lower than 0.5 $^{\circ}\,\mathrm{km}^{-1}$. Considering that the distribution of K_{dp} is very skewed, values above 1 $^{\circ}\,\mathrm{km}^{-1}$ in snow can be considered as unusually large.

2. Line 98-99 Would it be too cumbersome to include the formulations (in some condensed form) of Murphy et al. (2020) to convert Z_H, Z_{DR}, and K_{dp} to microphysical quantities here? It would give the reader a better idea of how the measurements are being used.

We agree with the reviewer and we decided to provide the explicit formulation of the variables as well as a more thorough discussion of the (significant) sources of uncertainty for these relations. Eqs. 1, 2 and 3 in the revised manuscript present the mathematical formulation, while the discussion on the underlying assumptions and limitations reads:

More details about the derivation of the equation can be found in Ryzhkov and Zrnic (2019); Murphy et al. (2020) but it is worth to focus on the main assumptions and limitations. The main assumptions are:

- *The equations are derived assuming to be in the Rayleigh regime (which may not be fulfilled) for the X-band radar for large hydrometeors.*
- *The density and the size of the hydrometeors are assumed to be inversely proportional.*

The retrievals have shown to be most reliable at $T < -10\,^{\circ}C$*, for low riming degrees and in regions where the* K_{dp} *and* Z_{DR} *signals are not close to 0. As recognized by Murphy et al. (2020), the errors may be large and in situ validation efforts are needed to refine these techniques. As a final caveat, the equations developed on theoretical basis are in practice very sensitive to the accuracy of the polarimetric variables, which can be very noisy.* K_{dp} *in particular is an estimated variable affected by mean errors on the order of 30% (Grazioli et al., 2014).*

3. Line 118 It seems to me that the motivation for including only ice-graupel collisions (not, for example, snow-ice collisions) is the theoretical constraint from Phillips et al. (2017) for a sufficient collision kinetic energy. Perhaps this should be explicitly mentioned.

   This is not the case. It is rather a theoretical constraint from Sullivan et al. (2018). The parameterization is based of the ice fractures that are generated during the collisions of large rimed ice particles (e.g., graupel or hail) in the study of Takahashi et al. (1995). We added the following text to make the limitation explicit.

   Because the parameterization from Sullivan et al. (2018) is based on experimental results by Takahashi et al. (1995), it is constrained to only ice-graupel collisions and may not be adequate when studying ice-ice collisions in winter-time MPCs consisting of mainly ice and snow crystals. The amount of fractures that can be generated in snow-ice collisions might, on the contrary, not be significant because of the low collision kinetic energy between unrimed particles. Experimental studies are thus required to show evidence for generated ice fractures between unrimed ice particles.

4. Lines 139 Is the temperature threshold correct here? Normally, a threshold freezing of cloud droplets occurs at -37°C not -50°C.

   Correct. We offer the following explanation to make our statement clear. The temperature threshold at which most cloud droplets freeze is $\approx -38\,^{\circ}$C. In COSMO, there is an additional step stating that all cloud drops should freeze at $\approx -50\,^{\circ}$C. We adapted the manuscript:

   Homogeneous freezing of cloud droplets, parameterized from the homogeneous freezing rates of Cotton and Field (2002), is calculated for $0 > T \geq -50\,^{\circ}C$*. At* $-38\,^{\circ}C$ *most cloud droplets will freeze given the enhanced homogeneous nucleation rates at colder temperatures. As a lower bound the homogeneous freezing of all cloud droplets occurs at* $T = -50\,^{\circ}C$*.*

5. Lines 144-145 Confusing wording here. How about "As for many other numerical weather models, rime splintering is the only secondary ice production process included in the standard version of COSMO." Also, I assume the RS formulation uses the general 350 fragments per milligram rime value, but I would mention this value here.

   Yes, that is correct. We adapted the manuscript as follows:

   A default value of 350 fragments per milligram of rime is used in the rime splintering parameterization.

6. Lines 169-170 Could you say something more precise about what you mean by early graupel formation? As it stands, you say that it is "promoted when ice crystals or snow are converted to graupel" which is a bit of a tautology.

   We will clarify the meaning of early graupel formation with the following adaption to the manuscript:

   In eq. (70) of Seifert and Beheng (2006), they specify that ice and snow crystals can only be converted to graupel once they reach $\bar{D}_{\mathrm{i,s}} \geq 500\,\mu m$. However, in the current version of the 2M scheme (as used in this study), ice and snow crystals are converted to graupel already once they exceed $\bar{D}_{\mathrm{i,s}} \geq 200\,\mu m$. Therefore, earlier graupel formation is promoted in the current version which should lead to enhanced SIP though ice-graupel collisions. To test the model's sensitivity to these different thresholds for graupel formation, we set-up sensitivity studies with graupel formation at $\bar{D}_{\mathrm{i,s}} \geq 300$, 400 and 500 μm, respectively, to understand how the conversion rate impacts SIP processes.

7. Lines 190-191 "The vertical evolution of K_{dp} and Z_{DR} is similar, with a peak observed about 4 km amsl.." Here you are already looking at Figure 5, right? Please cite the figure.

   Correct. We adapted the reference.

8. Line 208-209 Unless I am missing something, I would remove the sentence that IWC and NICE "fall within the 10 and 90^{th} percentiles range of the observations." This does not really indicate any agreement to me.

   We removed this entire paragraph because we discuss the limitations of the retrievals in the methods sections.

9. Line 248-250 Given that no simulation performs best on all metrics, is it a fair conclusion that the size scaling from Sotiropoulou et al. (2020) (your Equation 3) is not an important factor in this case?

   No, this would not be a fair conclusion. In Eqs. 1 and 2 (now 4 and 5) a scaling parameter is applied to compensate for the discrepancy between ice particle size vs velocity used in Takahashi et al. (1995). Sotiropoulou et al. (2020) instead applied a direct size scaling in terms of the large ice particles that was used in Takahashi et al. (1995). We believe that the parameterization from Sotiropoulou et al. (2020) is most likely the better parameterization to use, but it needs to be tested in more cases. One caveat is in how the scaling factor is applied. The scaling factor has to be used with caution because very small graupel might not cause ice fractures in collisions with ice.

10. Figure 6 Which simulation is this figure from?

    In Figure 6 (now Figure 8) we used the BR2.8T simulation. We do mention it in the text, however, we now added it to the figures (8, 10 and 11) to make it clearer. Thank you.

11. Line 283 I would remove the “not surprisingly” here. There has been significant discussion of how updraft modulates SIP rates but not shear, so it is indeed surprising that longitudinal wind shear is the “most important determinant” here.

    This is true, we removed "not surprisingly"

12. Line 349 I would write “Both shortcomings could be explained by omission of ice-graupel collisions.” There are also other processes that could explain an overestimation of Z_H and underestimation of NICE.

    Thank you. We have adapted the manuscript.

13. Line 296-297 “The higher MI values for V-wind shear with SIP is most likely why the Wind shear had larger and significant MI values with SIP.” I do not understand this; to me, it sounds like you are saying the values are larger because they are larger. Could you please reword or remove?

    We removed this statement.

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