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

General comments

This article puts forward the interesting idea that wind shear could "enhance interaction between ice particles", particularly through shear-induced turbulence, and promote secondary ice production. There are other interesting ideas also, for example that secondary ice production strength or dependencies could be hidden behind unrealistic hydrometeor growth rates. It is challenging and important to compare high-resolution model output and ground-based observations of microphysical variables, and I appreciate that effort here.

Despite these positive aspects, there is significant work to be done on this manuscript before it is in publishable form. The discussion of results was quite difficult to follow, in part because the figures do not seem to be ordered logically and in part because the writing is often convoluted. Points 3 and 4 below are two suggestions to improve this in my opinion. Before the results are restructured a bit, it is hard for me to know if I am convinced by them.

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.

2. The two prior studies mentioned (Grazioli et al. 2015a and 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. 2017 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.")

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?
    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"?

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 (~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.

Specific comments

Line 78-79 Again, a definition of K_{dp} 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.

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.

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.

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

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.

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.

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.

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

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. 2021b (your Equation 3) is not an important factor in this case?

Figure 6 Which simulation is this figure from?

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.

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.

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?

<u>Small editorial stuff</u>
Line 53 prevelent → prevalent

Line 79 e.g. removed. 252 K = -21° C; it's not an example.

Line 82 number high → high number. Also, "as ***large*** K_{DP} is an indicator of.." Only large values of K_{dp} indicate high number concentrations of oblate hydrometeors.

Line 154 recoreded → recorded

Figures 2/3/7 caption Hofmoller → Hovmöller*

Line 359 specie → species

<u>References</u>
P. Field et al. (2017) Secondary ice production: Current state of the Science and Recommendations for the Future. *Meteor. Monog.* **58** 10.1175/AMSMONOGRAPHS-D-16-0014.1

E. P. Luke et al. (2021) New insights into ice multiplication using remote-sensing observations of slightly supercooled mixed-phase clouds in the Arctic. *Proc. Nat. Acad. Sci.* **118** (13) e2021387118.

V. T. J. Phillips et al. (2017) Ice multiplication by breakup in ice-ice collisions. Part I: Theoretical Formulation. *J. Atm. Sci.* **74** (6) 10.1175/JAS-D-16-0224.1