Review 2: Heavy snowfall event over the Swiss Alps: Did wind shear impact secondary ice production?

I am grateful to the authors for putting effort into clarifying the manuscript, which I can see in their responses to my first round of comments. I will start by reiterating that I think modulation of secondary ice production rates by wind shear is an interesting idea and extracting such signals from remote sensing data is a worthy effort.

I have to admit, however, that I am still struggling to follow arguments in the results sections. I am wondering if you can still reorganize Section 3.2. Section 3.2.1 is entitled "Model and Doppler radar comparison." I would limit the discussion in this section to exactly that – comparisons of the modeled versus measured Z_H, K_{dp}, and Z_{DR} values – and focus on Figures 3, 4, and what is currently Figure 7d and e. I would extract 7d and 7e into a new, separate figure. Thereafter, perhaps Section 3.2.2 can be "Modeled and measured cloud properties" in which you focus on Figure 7a,b,c, and f *and* Section 3.2.3 "Microphysical explanations" in which you present some potential pathways for the model-measurement differences on the basis of Figures 5 and 6 (e.g. Lines 274-285 could be migrated to such a section.)

Sections 3.3 and 3.4 are somewhat easier to follow, although it seems to me that a better title for Section 3.3 would be something like *Linking Simulated Large-Scale Wind and SIP*. I would also work to end each of the subsections within Section 3 with a "bottom line," i.e. the one or two points you want the reader to take away from the discussion there. This would help readability.

(Relatively More) Major Comments

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

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.

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 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.

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.

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.

Line 211 – This has become quite a long paragraph. I would break after "from Sullivan et al. (2018) where D_0 = 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.*

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.

Lines 289-290 – Is this a single, time-averaged observation of ICNC from the disdrometer?

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."

(Relatively More) Minor Comments

Line 2 – "high precipitation" – *high intensity precipitation*?

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?

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.

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?

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

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

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.

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

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.*

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?

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)*"

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.

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

Line 243 – Missing reference

Line 274 – Missing reference