Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2022-429-RC2, 2022
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Comment on acp-2022-429

Anonymous Referee #2

Referee comment on "Heavy snowfall event over the Swiss Alps: did wind shear impact secondary ice production?" by Zane Dedekind et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-429-RC2, 2022

General Comment

This study focused on a role of vertical wind shear and turbulence on secondary ice production. The topic is unique and interesting. The hypothesis in this study is reasonable. However, I had difficulty to follow the manuscript probably because of a lack of detailed descriptions of simulations. There is also a lack of observational evidence of secondary ice production. I suggest considering revising the following points before publication.

Specific comments

1. It was difficult follow the entire manuscript, probably because of a lack of descriptions of model simulations (or I cannot find at least). The authors should give detailed, careful explanations of simulations for people who do not have model background. Specifically, I have the following questions:

- For Eqs. 1-3, what does "BR" stand for? What do BR28, BR2.8T, and BR-Sot mean? Because I could not know them, understanding the following descriptions was very difficult for me.
- What does N_BR mean?
- Lines 164-165: To me this sentence does not make sense at all. Need detailed explanations. What is scaled; what are BR, Sot, 2.8T?
- What is RS simulation? I could not easily find the description about the simulation.
- It seems to me that the radar reflectivity from the simulations is very large. For many

regions below 5-6 km, reflectivity attained or exceeded 30 dBZ for all simulations. The authors mentioned the size of simulated particles, but still I think too large for snow scattering at X-band, otherwise it was graupel. Was graupel produced in the entire cloud below 5 km? Please give detailed settings of calculation of reflectivity from the simulation data.

2. I also felt a lack of observational evidence of secondary ice production. The authors need to show observational data and explanations of the secondary ice production. Below are my comments.

- For the case, both KDP and ZDR coincidentally increased at the same altitude. A signature of large KDP and large ZDR does not necessarily represent secondary ice production. Rather, it can be interpreted as size growth of individual particles (without secondary ice production). One of good signatures of secondary ice production is large KDP collocated with small ZDR. This can be seen Fig. S3, but less description about this in the text. In addition to such signature, the previous literature also showed other observational evidence such as in-situ data, Doppler spectra, and/or liquid water path. This manuscript did not show such evidence.
- Because of less figures from the observations (there are only reflectivity and hydrometeor classification plots), it was difficult to follow the first and second paragraphs of Sect. 3.1.1.
- RHI scans were performed from the low to high elevation angles (0-90 degrees). Observed Kdp and Zdr should have strong dependency on elevation angles. Did you correct the values for angles?
- I was not sure how the 2DVD data were used other than number concentration. Did the 2DVD show good evidence of secondary ice production?
- Please explain how to estimate NICE and IWC from observation (e.g. Fig. 5, Fig. S3).
- Did you see shear instability?

Technical comments

- Three digits are needed for latitude/longitude of the instrument locations.
- Line 123 "dual-Doppler radar output": I was confused. Did you perform dual Doppler radar analysis (did you use two Doppler radars)? If so, please provide the second radar information.
- Line 150: The use of consistent unit throughout the manuscript for temperature is better.
- 3.2: What criteria were used for separating the period?