Review of "Distinct secondary ice production processes observed in radar Doppler spectra: insights from a case study" by Anne-Claire Billault-Roux et al.

Overview

This study is aimed at identifying SIP events based on the analysis of ground-based remote sensing and in-situ observations. The presented measurements were collected from a deep frontal cloud system during the ICE GENESIS campaign. The remote sensing identification of SIP events was performed based on the analysis of primarily W-band radar Doppler spectra and SLDR. In my opinion, identification of SIP mechanisms is hindered by the large spatial separation between the observation point of secondary ice particles and the location of their origin. For this reason, the identification of the SIP mechanisms presented here is not convincing and can even be misleading (major comment #1). However, I see great value in this study in the collected data set combining remote sensing and in-situ measurements and a comprehensive analysis of the radar data. The employed remote sensing technique allows for identifying the presence of cloud particles with different properties (i.e. characteristic shape, Doppler velocity) and identifying SIP events with a high degree of confidence. The observational part of the paper undoubtedly deserves publication. However, the portion related to the identification of the SIP mechanisms is very concerning.

Recommendation: I would suggest that the authors to withdraw the paper and rewrite it following the suggestions below. However, if the authors decide to proceed with the existing development, they should address the comments listed below prior to publication.

Major comments

1. The biggest concern in this study is the attempt to identify SIP mechanisms based on the analysis of the remote sensing measurements performed over the ground-based site. The particles which are present in the vertically pointing radar beam at each moment of measurements arrived there from different upstream locations and, therefore, have different ages and have experienced different histories of RH and T. The measured secondary ice particles most likely originate many kilometers away from the location of the measurements. Crucially, this means that the conditions required to initiate the specific SIP mechanism that has created these particles most likely will not be persist at the location of their measurement. For example, the small secondary ice particles, which result from ice-ice collisional breakup between graupel will be spatially separated from graupel by a minute, due to the large difference between their fall velocities. Additionally, the secondary ice particles that may be subsequently transported by horizontal wind to the point of remote sensing and measured by the time when the graupel will precipitate down to the ground. Thus, depending on the vertical profile of RH and T, for secondary ice particles formed at 2km, it could take more than one hour to precipitate down to the ground. Therefore, the location of production and measurement of secondary ice may be separated by many kilometers depending on the horizontal wind speed. Such large spatial separation and variability of environmental parameters hinders accurate identification of SIP mechanisms and may result in misleading conclusions. This is a serious limitation in identification the of the SIP mechanisms from the vertically pointing ground-based radars. Under these circumstances, it would be relevant to reduce or completely remove sections associated with the identification of SIP mechanisms as highly speculative.

2. **Suggestions:** The authors may consider refocusing the interpretation of the observations on the identification of the conditions of the formation of multimodal Doppler spectra. For example, it can be speculated that the source of secondary ice particles required to produce bimodal Doppler spectra as in Figs.1,5,7 should have a large horizontal and small vertical extensions (pancake type source). It is unlikely that production of secondary ice particles that extended over a large vertical distance (e.g., >1km) would produce patterns like in Figs.1,5,7. The authors may consider replacing the attempts to identify SIP mechanisms by a simulation study to reproduce the radar measurements. I believe you will discover much more interesting things along this line of inquiry. The simulation study will undoubtedly delay the publication. However, in my opinion, this will be worth the effort and increase the scientific value of this study.

3. In-situ measurements obtained in the frame of this study contain plenty of information, which could be used for a scrupulous interpretation and calibration of the radar techniques. Unfortunately, the in-situ data was used in a qualitative way. One of the options, which the authors may consider is to calculate the Doppler spectrum and SLDR based on the analysis of the particle probe measurements and particle image recognition. This would be a solid basis for interpretation of the radar outputs.

4. In Section 5.2.1 the estimated liquid water content (0.9<LWC<1.4g/m3) appears to be overly high for the stratiform region in a cold season frontal cloud system. I do not see a reasonable explanation for the formation of such high LWC for this specific case. The problem is aggravated by presence of ice particles, which are expected to rapidly deplete liquid water through the WBF and riming processes. I also suspect that the high value of the measured LWP >800g/m2 may be contributed by the melting layer, which is clearly seen in Fig.4c.

5. Another point of concern is related to instant transition of disk-like particles to columnar ice at approximately 1.9km as shown in Fig.8. An instant transition of particle habits does not sound physically possible. The disk-like and columnar particles are expected to coexist at some range of altitudes like in Fig.7. Could you explore in more detail the retrievals behind this and attempt to explain this case?

6. Recent laboratory studies by Hartmann et al. suggest that the role of the HM-process is overestimated. The authors may consider adding a disclaimer regarding the role of the HM-process. It is worth noting that some past laboratory studies also did not observ SIP during riming (e.g., Hobbs and Burrows, 1966; Aufrermaur and Johnson, 1972).

Aufdermaur, A. N. and Johnson D. A.: Charge separation due to riming in an electric field, Q. J. Roy. Meteor. Soc., 98, 369–382, https://doi.org/10.1002/qj.49709841609, 1972.
Hartmann, S., Seidel, J., Keinert, A., Kiselev, A., Leisner, T., and Stratmann, F.: Secondary ice production - No evidence of a productive rime-splintering mechanisms during dry and wet growth, EGU General Assembly 2023, Vienna, Austria, 24–28 Apr 2023, EGU23-11199, https://doi.org/10.5194/egusphere-egu23-11199 , 2023.
Hobbs, P. V and Burrows, D. A.: The Electrification of an Ice Sphere Moving through Natural Clouds, J. Atmos.Sci., 23, 757–763, https://doi.org/10.1175/1520-0469(1966)023<0757:TEOAIS>2.0.CO;2, 1966.

7. Ground based observation of ice habits and time series of temperature would be a valuable addition to the radar measurements and may facilitate their interpretation.

Minor comments

1. Lines 40-41, "*The so-called Hallett-Mossop (HM) rime splintering mechanism (Hallett and Mossop, 1974) occurs as supercooled cloud droplets or drizzle/rain drops rime onto ice particles, generating ice splinters in the process*…" Following the original definition, "*drizzle/rain drops rime*" is not included in the HM process. In fact, riming of drizzle/rain drops on ice is in a gray zone in terms of the type of SIP mechanism it may initiate. Thus, impact of drizzle/rain drops with a much smaller in size ice crystal will result in the initiation of the droplet shattering mechanism rather than HM mechanism.

2. Caption to Figure 1: What is the definition of "fragile aggregates"? I am wondering if the HVPS low resolution imagery is sufficient for identification of mechanical properties of ice particles.

3. Lines 265-268: "*the detection of columnar crystals, at first in a restricted altitude range around 1.5 km…"* and *"a disk-like mode is identified either in restricted altitude ranges…*" Clarify "*restricted altitude range*".

4. Figure 3: What is the dotted line in the vicinity of 4km?

5. Figure 6c: Identification of HVPS images in three green boxes (two top image frames in Fig.6c) is quite ambiguous and not convincing. The resolution and quality of the images are not sufficient to draw such a conclusion. Why don't you use 2DS with a higher pixel image resolution to defend your statement?

6. Figure 6: The circular images in blue boxes do not contain information about the thermodynamic state of these particles. Therefore, their identification as "liquid droplets/drops" in the figure caption is an overstatement.

7. Figure 6: Fix overlayed text in the HVPS titles.