Anonymous Referee #2

Received and published: 30 August 2020

Overview:

The authors analyze data from ground-based triple-frequency radar, lidar, surface disdrometers and gauges, as well as environmental observations, with the aim of describing the vertical microphysical structure of liquid and ice-phase precipitation in a single, 6-hour period during which stratiform rain was observed. The data are of high quality and the methods involve optimal estimation of the precipitation PSD's using Doppler spectra and a collection of diverse particle models. The primary question posed is to what extent the flux of stratiform precipitation through the melting layer can be considered a steady, particle-mass-conserving process, and what microphysical mechanisms might lead to deviations from that kind of process?

The work is an original contribution, including the collected datasets which are fairly unique. The data and methods are generally described quite well, with good clarity of language, and the figures appropriately demonstrate the points made in the manuscript. However, there are some questions on the interpretation of the data and especially the "closure" procedure that will require some substantial explanation and/or revision, as detailed in Major Points, below.

The authors would like to thank for all the comments and the suggested corrections. In particular, the comment on the constraining the snow retrieval with the properties of rain made us realize that the validation of the MOSS assumption is not truly independent and, in fact, it is biased toward the mass flux continuity constraint. We have tested a new version of the algorithm were these conditions are removed but we noticed that it leads to much higher uncertainties in the estimates of both the environmental parameters (wind and turbulence) and the PSD itself. Moreover, due to a large number of parameters involved in the retrieval, the algorithm is often trapped in a local minimum of the cost function and the spectra are not well fitted. Such local minima correspond to e.g. only left or right slopes of the spectra being fitted. Therefore, another methodology is currently tested where much lower number of the retrieval parameters is considered. For more detail please see the answers to the specific points.

Major Points:

(1) Section 3.4: Up through section 3.3, the manuscript is of high technical quality, and the authors' approaches and interpretations appear mostly sound. However, section 3.4 describes the optimal estimation (OE) of ice-phase particle properties that utilizes the Doppler spectrum to estimate ice-phase particle PSD's for different assumed ice particle models, selecting the most appropriate particle model based upon which one minimizes the OE's cost function.

In and of itself, the OE is fine. The problem is that the OE is constrained by (a) initial guesses, or priors, of the ice particle PSD's supplied by the Doppler-spectrum-derived rain PSD's which are extended to ice using the "melting only steady state" (MOSS) assumption, as well as (b) a second objective function term that constrains the ice and rain mass fluxes to be closer (a difference fraction standard deviation of 0.33 is assumed). The MOSS assumption, in particular, is used to obtain a prior ice PSD that has the same mass flux as the rain below it. Clearly, the two prior terms (a) and (b), but especially (a), of the OE's objective function will tend to force the estimated mass fluxes of ice and rain to be more similar, regardless of the ice particle model chosen. But the primary purpose of the OE described in section 3.4 is to "assess the validity of the flux continuity assumption" as stated in the last sentence of that section. (The fact that the rain-spectrum-derived constraint is assigned a factor of two error doesn't really allow that much freedom to the OE solution, because as seen in Fig. 2b, e.g., a change of 1 x 10ˆ4 mˆ-4 to 2 x 10ˆ4 mˆ-4 in number density is not that large.)

Clearly, the application of such an OE could result in greater consistency of estimated ice and rain mass fluxes, and so as formulated, the estimated ice-phase precipitation fluxes from this OE can't be used to independently evaluate how much consistency there is between ice and rain fluxes. But that is precisely what is done in section 4.4. Unless I'm missing something, this is circular reasoning and not a scientifically valid approach.

If the authors want to address the ice vs. rain flux continuity issue in a quantitative way, they would need to decouple their rain and ice estimation procedures: What if no priors (referenced in a and b, above) are included in the objective function described in section 3.4, or what if only some simple gamma-fit to the ice particle Doppler spectrum is used as a prior? Either would decouple the rain and ice-phase estimation. If some prior based on rain-related PSD's and the MOSS assumption is required to get a stable estimate of ice PSD's, then one must question the information content of the ice Doppler spectrum and whether there is any way of independently estimating the ice PSD's and mass fluxes directly from their Doppler spectra.

The link between PSDs of ice and DSDs rain imposed in the retrieval was intended to stabilize the retrieval, but as you rightly pointed out, by using it the mass flux continuity conjecture cannot be evaluated independently. Therefore, we tested another version of the ice PSD retrieval where this dependence is removed. As expected, the retrieval became more uncertain which is manifested by much higher variability of the precipitation rate over time as it can be seen in Fig.1. below (left panel). As in the old analysis, the characteristic size of ice during the aggregation period is much larger than in the rain below which suggests break-up during melting. However, during the riming period, the precipitation rate in ice is on average larger than in rain which is not in line with our previous interpretation about the condensation and collision-coalescence with cloud droplets within the melting zone. An inspection of the individual spectra revealed two problems: first, the retrieval that is not constrained by the rain is often trapped in the local minima of the cost function that leads to poor spectra fitting. Second, Doppler spectra are often matched equally well by the two or more snow models by appropriately selecting the vertical wind velocity as it is shown in Fig.2. In order to address these issues, we are currently working on reducing the number of the ice PSD bins retrieved by the algorithm and constraining the wind component. The velocity of the vertical air motion is estimated from the velocity of the slowest detected targets. The final version of the retrieval will be included in the revised version of the manuscript. Figure 9 in the paper will show the spread of the best fitting solutions which will help in quantifying information content of the spectral data.

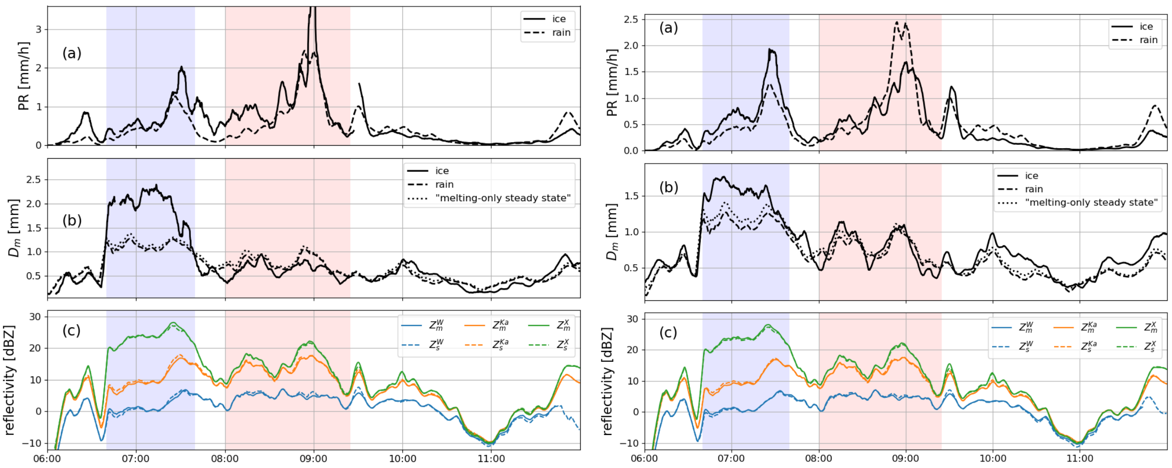

Figure 1. The full Doppler spectra retrievals applied both below and above the melting zone. Left: no dependence between the properties of ice and rain is used; right: PSD of ice constrained by the MOSS assumption

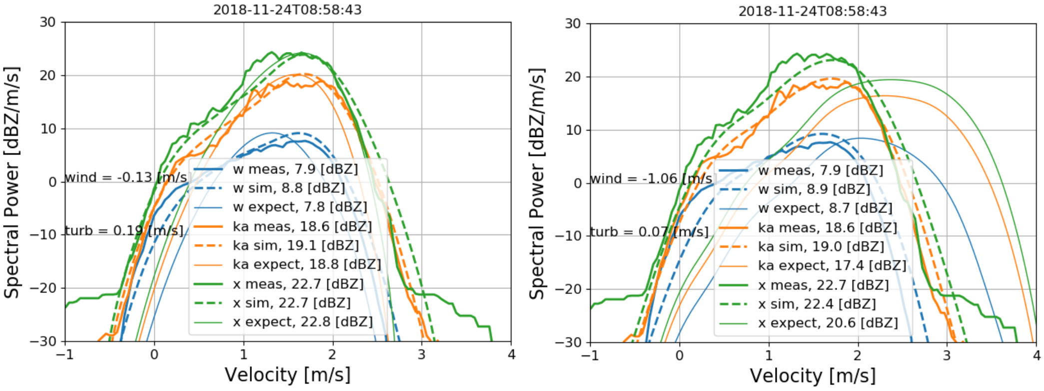

Figure 2. The measured (continuous line) and the simulated spectra (dashed line) for two different snow models (left: L&S15A0.5, right: L&S15A2.0).

(2) p. 18, last paragraph of section 4.3.1, and p. 21 second paragraph: one of the difficulties of interpreting profile-type measurements is that one doesn't get a full 3D picture of the atmosphere, but just a 2D "curtain". Therefore, isn't it just possible that there was some horizontal variability of precipitation during the "aggregation" period and wind components perpendicular to the mean storm motion that could move aggregates of different concentrations into or out of the "curtain", so-to-speak? (At least evidence of vertical wind shear *within* the "curtain" is suggested by the tilted structures of Z and DFR in Figs. 1a and 1b, respectively.) The melting layer during the "aggregation" period had a depth of ~400 m, and so if the particles fell with an average speed of ~2 m/s, then they could potentially move laterally out of the 17 m wide radar beam in the ~200 s it took them to fall through the melting layer. If the precipitation was not strictly horizontally homogeneous, then that could cause difficulties for the authors' microphysical interpretation.

My general point here is that particle breakup in the melting layer is not the only possible explanation for higher ice-phase reflectivity fluxes relative to rain reflectivity fluxes during the “aggregation” period... all it would take is some horizontal variation of aggregates perpendicular to the “curtain” and some vertical variation of the horizontal wind.

Also, although breakup is certainly possible in the melting layer, melting aggregates could self-collect pretty efficiently as well.

The authors fully agree with this comment. This aspect was omitted in the discussion because the presented analysis is based on the 1D assumption due to unavailability of the data about the spatial variability of the system. However, we acknowledge that raising this point is important in the interpretation of the data we present. Therefore, the following discussion was added at the end of section 4.3.1:

One of the difficulties of interpreting profile-type measurements is that they do not provide a full 3D picture of the atmosphere, but just a 2D slice. Therefore, the presented conclusions are based on the assumption that the observed system is locally homogeneous i.e. despite horizontal winds the measurements taken below the melting layer correspond to the evolution of the ice PSD measured aloft. Considering the horizontal wind speed within the bright band (approx. 1.8 m/s during the “aggregation” period according to the ECMWF model) and the time needed for the particles to melt (approx. 3 minutes based on the MDV data) the precipitating system must be uniform over 325 m to meet this criterion. Because, the beam with of the X-band radar at the altitude of the melting zone is only 15 m, it is possible that the higher ice-phase reflectivity flux relative to rain can be a result of a horizontal gradient of the reflectivity that, for example, corresponds to the storm intensification along the wind direction. Note that, for the most part of the aggregation period the precipitation rate increases over time (see Fig. 9a) which supports this alternative interpretation.

Minor Points:

(3) Fig. 2 is a very informative reference, but some of the inset plots are very small and hard to read, particularly the snow spectrum panel above (C). Although these plots are meant to be symbolic, it would be good if they could be read more easily.

Fonts of the labels was increased in the schematic that makes it much clearer.

(4) p. 11, Eqs. (3) and (4), if v is meant to symbolize terminal velocity, shouldn’t the capital V be used, as in Eqs. (1) and (2)? Also, I think w was previously defined in Eq. (1) as “negative upward”. Shouldn’t the w in Eq. (4) be similarly defined?

The velocity symbol, v, in the equations (3) and (4) was capitalized. The vertical wind is consistently defined as “negative upwards”. The formula (4) just indicates that whenever non-zero wind is present the measured spectrum is shifted in the velocity domain. To avoid any confusion, an additional notation is introduced, i.e. $S_{\lambda,w,target}$ denotes the reflectivity spectrum affected by the vertical winds.

(5) p. 15, beginning of first paragraph: when comparing the “aggregate” ice spectra to the “rimed” ice spectra in Fig. 5 (a) and (b) it looks like both the “aggregate” and “rimed” have mean peaks that are pretty steady in velocity up to 1.75 km altitude. The “aggregates” have a deeper structure that is more consistent, while the “rimed” particles peter out above 1.75 km and the peak becomes variable. It’s a very minor point, but I would say the “aggregates” have a consistent spectral peak to 4 km, while the “rimed” particles also show a vertically-coherent peak, but only up to 1.75 km.

The suggested comment was added:

The ``aggregates'' have a consistent spectral peak to 4 km, while the ``rimed'' particles also show a vertically-coherent peak, but only up to 1.75 km in altitude.