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Atmospheric Measurement Techniques Discussions

Interactive comment on "Linking rain into ice microphysics across the melting layer in stratiform rain: a closure study" *by* Kamil Mróz et al.

Anonymous Referee #2

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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.

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.

(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 $\sim$400 m, and so if the particles fell with an average speed of $\sim$2 m/s, then they could potentially move laterally out of the 17 m wide radar beam in the $\sim$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 possi-

ble 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.

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.

(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?

(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.

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