Anonymous Referee #1

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Title: Linking rain into ice microphysics across the melting layer in stratiform rain: a closure study.

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Decision: Accept with minor revisions

General Comments: This preprint uses multi-frequency radar data and forward scattering calculations to investigate the validity of the often-used assumption of one raindrop corresponding to one snowflake, or "melting-only steady-state" (MOSS). The approach is extremely innovative, well-described, and supported, and should be of significant interest to the community given the ubiquity of the examined assumption and need for better microphysical insight in ice regions and the melting layer. The manuscript is very strong, with a logical and thorough flow, and is also quite well-written, with only minor corrections and clarifications needed. Pending the following comments and technical corrections, I believe the manuscript will be ready for publication.

Specific Comments:

Line 50: Please add just a brief statement about why it is more valuable in rain than ice for readers less familiar with Doppler spectra techniques.

The sentence was modified to accommodate some explanation:
"While this information is more valuable in rain than in ice, since the velocity of raindrops is unambiguously related to their mass and size (which is not true of snow), Doppler spectra allow to detect the presence of riming…"

Line 54: Please change "asymmetric" to "nonspherical".

Done

Line 103: By "re-sampled" here, I assume the authors mean "interpolated" and not a true re-sampling process (e.g., bootstrapping)? If so, please clarify.

Changed to "interpolated"

Line 113: Because it forms the basis for sampling "above" and "below" the ML, please provide just a very brief description of what this approach entails.

The following sentence was added:
"This approach is based on a very strong bright band signature in the LDR data in

correspondence to the melting regardless of the rainfall intensity. In this study, the inflection points around the LDR peak are used as the top and the bottom of the melting zone."

Lines 197-199: If the lidar data indicates supersaturation due to the inferred presence of liquid clouds and thus little risk of evaporation, should that not imply that condensation will occur on melting ice particles (assuming their surface temperatures remains near 0C during melting, or at least colder than the environment) and thus violate the assumption of mass conservation? (In addition to the collision/coalescence of said liquid cloud droplets). I certainly understand this is being presented as a known simplifying assumption (as stated on lines 213-214), but what is written (i.e., that the assumed presence of supersaturated conditions from the lidar data supports the notion of mass conservation in the ML) may not be strictly true.

Thank you for spotting this. Indeed, the discussion that was pointed out was not strictly true therefore it was modified to:
"In order to connect properties of ice with the properties of rain, several assumptions are made. Firstly, processes across the melting layer are assumed to be in steady-state. Secondly, effects of condensation or evaporation are neglected. The radiosonde launched at 9:00 UTC showed RH values exceeding 90% in the proximity to the freezing level which effectively excludes the possibility of evaporation. However, the possibility of condensation on melting ice particles and collision/coalescence with cloud droplets cannot be ruled out as the RH measurements are likely to be underestimated due to the saturation problem of the GRAW humidity sensor. Signatures of liquid clouds are present in the lidar data for altitudes within the melting zone after 8:15 UTC (Fig.1d) that indicates water vapour supersaturation conditions. Despite these potential discrepancies with the real world, neglecting condensation and evaporation is used as a simplifying hypothesis. These assumptions imply the flux of mass through the melting zone is conserved."

Line 347-349: If the ratio Vr/Vs increases with size, and Ns (compared to Nr) scales with this ratio, shouldn't this result in a relatively larger number of large particles com- pared to small ones (compared to what is measured in rain), rather than the other way around? Such an understanding would also seem to correspond with the subsequent statement of Dm decreasing during melting due to this shift.

That is true, the number concentration of large particles is increased compared to the small ones and it was modified in the text.

Line 359: This sentence stating the mean Doppler velocity is equal to the adjustment factor for the dielectric constant between ice and rain confused me. Is this a typo, or a reference to the idea that the change in dielectric constant is often approximately offset by the change in fall velocity, as noted in Drummond? Please introduce the factor mu separately. Also, after reviewing Zawadzki et al. (2005) it is my impression, perhaps wrongly, that this relation is strictly only true if the density of snow is assumed to be independent of its size, otherwise a size-dependent value of |Ks| would be needed. If that is the case, it should be explained and added to the list of qualifications for when the relation is valid. In general, given the importance of this value and formulation, a bit more explanation of its origins may be helpful to readers.

The sentence you are referring to is a typo. It has been corrected. We meant that the reflectivity flux ratio is equal to μ = 0.23. Regarding the other point, the factor μ in the study of Zawadzki et al. (2005) was derived assuming constant ice density. Nevertheless, their derivation is based on the formula of Debye that relates the dielectric constant to density of ice which effectively implies that the ice particles of the same mass, regardless of their density, correspond to the same reflectivity under Rayleigh scattering assumption. I add this comment in the manuscript:

"According to the ``reflectivity flux'' method proposed by Drummond et al. (1996); Zawadzki et al. (2005), the ratio of the reflectivity fluxes in snow and rain,

$\gamma = Z_s V_{D,s} /(Z_r V_{D,r})$

is equal to $\mu \equiv (\rho_w/\rho_i)^2(|K_i|/|K_w|)^2 = 0.23$, where the mean Doppler velocity is denoted by V_D, and the subscripts *s* and *r* indicate sampling in snow and rain, respectively, whereas the subscript *i* indicates ice. The relation is only valid for Rayleigh targets (which should hold for our X-band data) and under the ``MOSS'' assumption. Although, the factor μ was computed assuming constant ice density, the derivation is based on the formula of Debye ($|K_s|/\rho_s$ = const) which implies the reflectivity of the ice particles depends only on their mass not density. Therefore, the value of μ is independent on the snow morphology."

Figure 7: Does this analysis account for the residence time of particles within the ML, or is it a direct one-to-one matching of above/below the ML at a single point in time? If so, could the authors state this and speak to what impacts, if any, trying to better account for this offset in matching time might have?

The following text added to clarify it:
"In order to match the data below and above the melting layer more precisely, for each 15 minutes time window the optimal time lag that maximizes the correlation between the X-band reflectivity in ice and rain is derived. All the results that follows use this optimal matching in time."

Figure 7: How was the terminal velocity of snow determined here given the different possible models? The retrieval of the dominant snow type is explored in the subsequent section, but it isn't clear to me if that was applied to this analysis or if something constant was assumed?

The analysis of Drummond et al. (1996) does not use snow models explicitly because the velocity of particles is assumed to be the one measured by the radar. Therefore, the measured mean Doppler velocity is used to derive the reflectivity fluxes in rain and ice (formula 6).

Line 373: By "largest deviation" do the authors mean most consistently large deviation, given the larger magnitude (in dB-space) dips during the riming period? Please clarify.

Yes, we meant the consistent period, therefore we modified this sentence as follows:
"The most consistent deviation from the uncertainty limits is reported during the period when large snow aggregates…"

Line 405: What is meant here by "mapping the continuous into the dashed black line"?

To avoid any confusion, we modified this sentence:
"For a qualitative comparison, γ_n is used as a correction factor to the number concentration that makes triple-frequency measurements consistent with the ``MOSS'' simulations. This is done by reducing the measured reflectivities by γ_n derived for the X-band ($Z^X_{\gamma_n \equiv 0} = Z^X - \gamma_n$; $Z^{Ka}_{\gamma_n \equiv 0} = Z^{Ka} - \gamma_n$; $Z^W_{\gamma_n \equiv 0} = Z^W - \gamma_n$). The result of this correction is shown as the dashed black line in Fig.8 panels a-c."

Line 492: By "inter-model extend", are the authors referring to the range of simulated radar reflectivity values? Please clarify.

The sentence was modified:
"This range of simulated radar reflectivity values mainly reflects differences in the terminal velocities for different models and is comparable to the uncertainty of the ``MOSS'' hypothesis…"

Technical Corrections:

Line 16: Remove comma after "that".

done

Line 36: I don't believe "mid-latitudes" or "tropics" needs to be capitalized.

corrected

Lines 37, 40-41, and elsewhere: Remove parentheses around reference year.

done

Lines 51, 176, Table 1, and elsewhere: Change "ms^-1" to "m s^-1".

changed

Line 77: Add "the" before "DSD" and "PSD".

added

Line 97: Remove "a" before "X-".

removed

Line 106: Please add “the” before “methodology”.

added

Line 255 and elsewhere: Change “kgmˆ-2” to “kg mˆ-2”.

Changed

Line 336: Change “undergone” to “underwent”

changed

Line 448, 509: Change “approx.” to “approximately”

replaced

Line 460: Change “even tighter relation. . . are expected” to “an even tighter relation. . . is expected”

changed

Line 462: Change “constrain” to “constraint”

changed