Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-272-RC1, 2020

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 #1

Received and published: 18 August 2020

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

Authors: Mróz, Kamil Battaglia, Alessandro Kneifel, Stefan von Terzi, Leonie Karrer, Markus Ori, Davide

DOI: 10.5194/amt-2020-272

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

Printer-friendly version

Discussion paper

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.

Line 54: Please change “asymmetric” to “nonspherical”.

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.

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.

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.

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 compared 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.

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.

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?

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?

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.

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

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

Technical Corrections:

Line 16: Remove comma after "that".

Line 36: I don’t believe “mid-latitudes” or “tropics” needs to be capitalized.

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

Lines 51, 176, Table 1, and elsewhere: Change “msˆ-1” to “m sˆ-1”.

Line 77: Add “the” before “DSD” and “PSD”.

Line 97: Remove “a” before “X-”.

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

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

Line 336: Change “undergone” to “underwent”

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

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

Line 462: Change “constrain” to “constraint”

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-272, 2020.