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Interactive comment

Interactive comment on "What millimeter-wavelength radar reflectivity reveals about snowfall: An information-centric analysis" *by* Norman B. Wood and Tristan S. L'Ecuyer

Anonymous Referee #3

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This manuscript describes the optimal retrieval process for deriving snowfall from single millimeter-wavelength radar reflectivity observations and represents a derived uncertainty analysis divided into the relevant error sources in the process. The manuscript is logically and well structured, and fluently written. The topic has been walked through by describing the steps nicely and in an informative way, and the manuscript was very nice to read. Although the topic is not necessarily very novel, when more recently the tendency has been on describing the multi-frequency retrieval processes or retrievals combining single-frequency with Doppler-velocity information, the way the authors describe the details in the process, and especially, the factors influencing the uncertainty, the manuscript has a significant contribution and is relevant and interesting for the community. My recommendation is that the manuscript should be published with minor changes and clarifications.

Below I have a few small requests for clarifications for the authors.

Request for clarification:

1. My major concern in the development of the retrieval process is that it is, if I have understood correctly, based on almost single simplified snow model (Wood et al.2015) with single values of α, β, γ and δ with a certain uncertainty stated in Appendix B. Although, it has been shown e.g. in Kulie and Bennartz, 2009 in Figure 1, the huge variety in backscattering coefficients for different particle types and as it is stated also in the manuscript e.g. on line 246 that in Wood et al. 2015 the uncertainties for the described perturbed particle models can be as high as 15 dB. What about the effect of aggregation creating very different particle properties (low area ratio) from single crystals or rimed particles? I wonder could this be the reason for the two outlier cases described in the results in paragraph starting on line 319 that the particle types (m(D) and v(D) were so different from the used parametrization of Mitchel and Heymsfield 2005), and the used particle model were not descriptive for the observed particles, although still dry snow particles and not e.g. melting as proposed in the manuscript on line 403. And therefore, the retrieval failed. Actually, looking at the Figure 7 with all higher snowfall rates (> 0.8 mm), the retrieval seems to underestimate quite systematically. If seen also as relevant, could the authors provide more discussion on this topic to the manuscript although there is already the statement on line 403?

2. As a second point, in my understanding, the Z-S retrieval is less dependent on the intercept parameter in the millimeter wavelength than in the centimeter region where the Rayleigh approximation is applicable (e.g. Rasmussen et al. 2003, Bukovčić et al. 2018). In the abstract, it is stated on line 14, that the PSD intercept is less well constrained by the retrieval, and in Appendix on line 488, that the measurements better sensitive to log(N0) could benefit the retrieval. Could the authors elaborate, whether

these results refer that the dependence on N0 is physically no longer similar significant to the retrieval.

And continuing, would then actually simple averaged Z-S relations provide equally good retrieval results, in case the particle type and shape are correctly modeled? The retrieval results here are compared to the collocated measurements of accumulation with a weather sensor. It would be also interesting to see, how different are the snowfall rates with this presented optimal retrieval method in comparison to these simple Z-S relations presented in the literature such as e.g. Kulie and Bennartz, 2009 or Matrosov, 2007.

Small comments:

In the paragraph starting on line 29, the ground-based radar observations are described. It is slightly striking that only MRR related literature references are stated, even though this paper is describing the retrieval process utilized for the W-band. Was there a certain reason for this choice, why e.g. other studies presenting W-band observations were left out? Although, some of the radars are mentioned on lines 71-73.

Figure 11. Suggestion to use a different color for FD12P to improve readability.

Line 381 Suggestion to use a different term for shape of the size distributions. Although it is clear that the exponential PSD distribution is used here and this term describes the effect of lambda in the metric curves in Figure 13, it still is close to the widely used shape parameter μ of gamma PSD.

Typos:

Line 307 should be Vaisala FD12P (Vaisala Oyj, 2002) instead of (Viasala Oyj, 2002)
Line 336 remove the unit

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Discussion paper

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