

## ***Interactive comment on “Rapid ice aggregation process revealed through triple-wavelength Doppler spectra radar analysis” by Andrew I. Barrett et al.***

### **Anonymous Referee #1**

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#### General Comments

The paper focuses on the identification of a rapid aggregation layer within an ice cloud. In order to do so, an innovative algorithm for the retrieval of snow particle size distribution (PSD) is developed. The algorithm leverages on the synergies of multi-frequency and Doppler observations from vertically pointing radar systems. The retrieved evolution of snowflake sizes is connected to microphysical processes through a modeling approach. It is concluded that neither depositional growth nor riming can explain alone the rapid increase in snow size and aggregation must play a major role, moreover, the expected sticking efficiency must be larger than what was previously published in

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dedicated laboratory experiments.

The paper puts emphasis on the properties of the rapid aggregation layer and in particular to the value of the aggregation efficiency ( $E_{agg}$ ). This would entitle the paper to be published on ACP given the importance of this process in ice clouds. However I am not sure if the reported conclusions are sufficiently supported by scientific evidence. In particular, I am concerned about the number of strict assumptions that have been made throughout the text, the lack of independent validation of the results and the very short duration of the single event selected to support the conclusions about  $E_{agg}$  value. By contrast the paper propose a very interesting and innovative way to use vertically pointing radar to retrieve the properties of particle size distributions. As best of my knowledge, this is the first retrieval of the size-resolved PSD using radars, which would allow to drop the assumptions about PSD shape that are necessary in bulk approaches. The presented methodology deserves a much more detailed description than the one presented in the text and a profound analysis of the sensitivity of the method to the various assumptions that have been made. After such revisions, I would definitely recommend to publish it, but I would suggest to consider a different journal such as AMT given the shift of the focus of the paper.

Given all of my concerns, I recommend to consider the paper to be published after a major revision.

#### Specific comments

1) Given the centrality of the concept for the entire manuscript I would suggest to give a definition of  $E_{agg}$  in the introduction section. This also to prevent potential confusion, given by the non-unique nomenclature used in this field where different efficiencies might mean different things (e.g. collection efficiency, sticking efficiency). Finally, this would help understanding the reasoning behind the last paragraph of section 6 and Figure 5, where  $E_{agg}$  is inferred from the vertical gradient of the slope parameter of the PSD.

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2) I am not convinced by the statement about Connolly et al. (2012) at lines 31-34 of page 2. By looking at Fig. 14 in the original paper I would agree on the fact that Egg is between 0.4 and 0.9 at -15 C because that is the confidence interval reported in the plot. For the very same reason I would say that it is between 0 and 0.5 for the other temperatures. Claiming that it is always below 0.2 might be an exaggerated statement.

3) In section 2.1 the non-microphysical sources of differential reflectivity (DWR) are accounted for. These source of retrieval error are compensated by making the radar reflectivity to match in the Rayleigh-scattering part of the cloud and applying the same adjustment to the whole profile (lines 4-5 of page 4). This method is strictly valid only for radar miscalibration and attenuation by radome or wet antenna; for height dependent sources of differential attenuation (e.g. atmospheric gas absorption, liquid in the cloud) this method causes an overcompensation of the higher frequency reflectivities at lower level (in particular W-band). Attenuation due to atmospheric gases depends on the density and humidity profile of the atmosphere and can add up to 2.5 dB at the top of the cloud at midlatitude; under this condition the overcompensation caused by the radar cross-calibration at 3-4 km could be easily in the order of 1 dB. I suggest to compensate for atmospheric gases absorption profile before making the radar-cross calibration, or, at least, estimate the W-band absorption profile for the analyzed case by using either a weather model or a radiosonde profile and report it either in the paper or in supplementary material.

4) 35- and 94- GHz radars are declared to be off-zenith by 0.2 and 0.15 deg in opposite directions (lines 15-16 of page 4). This causes a contamination of the doppler signal from horizontal wind component which is then corrected by shifting the spectra by constant values (line 2 page 5). The authors acknowledge the fact that this procedure is imperfect and consider the matching of the resulting spectra to be good in Figure 4. However it is not described how these numbers (mispointing angle and Doppler velocity correction) are obtained. I personally can hardly see how a composite relative shift of just 0.1 m/s would have affected the matching in Figure 4. The comparison in Figure 4

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would have taken benefit from the inclusion of the 3 GHz spectrum which is considered to be well aligned. Also the 'goodness' of the matching is both affected by velocity and power shifts: a small residual differential attenuation would have caused the spectra to look non-aligned; given the fact that there might be still a differential attenuation issue here (see my point number 3) the matching is potentially flawed by this residual error. I suggest to include the source of the mispointing angles and Doppler shifts numbers.

5) The PSD and v-D retrieval method is very roughly explained in pages 6 and 7. After several readings I understood that it assumed a unique relation between sDWR and the snowflake size. This relation is likely to be very specific of the assumed scattering model and mass-size relation. A plot showing this relation for a certain number of scattering models and m-D functions will help the reader understanding the methodology applied and gives an indication of the expected uncertainty due to the related assumptions.

6) Moreover, for the scattering model it is assumed Westbrook (2006, 2008a) since it has been found to closely match observation in the multifrequency space [Stein 2015]. However, the scattering model from Leinonen and Szyrmer (2015) has also been found to match the observation (unpublished on a peer review journal, but included in conference proceedings [http://www.isac.cnr.it/~ipwg/meetings/bologna-2016/Bologna2016\\_Orals/3-8\\_Westbrook.pdf](http://www.isac.cnr.it/~ipwg/meetings/bologna-2016/Bologna2016_Orals/3-8_Westbrook.pdf)) It would be very interesting to see the results from this different scattering model. Being a detailed DDA model one does not have to assume the m-D relation but can simply take particles masses and sizes from the database, achieving a better consistency of the results.

7) The particles that are sampled within each Doppler bin are likely to have different sizes. Is the model considering only one particle size per Doppler bin? This is potentially a significant source of error when large particles are present. Large particles are expected to fall roughly at the same velocity for many different sizes, thus  $dv/dD \ll 1$ , by contrast the backscattering signal given by those particles is very different. Assuming that in fast-falling doppler bins (i.e.  $v > 1\text{m/s}$ ) there are snow particles of just one size is

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not a valid assumption even at for doppler systems with a very high spectral resolution.

8) Considering the number of correction, a sensitivity analysis of the algorithm with respect to the input data is essential. Assuming 1 dB uncertainty in radar reflectivity and 1 or 2 velocity bins uncertainty in the doppler spectra will already give a good indication of the robustness of the algorithm. It will be particularly interesting to see how this translates into uncertainties in the retrieved PSDs (panels c, f and i of figure 4) and the profile of fitted scale parameter  $\lambda$  in figure 5.

9) The result of 'rapid aggregation' is obtained by comparing the relative potential of various snow growth processes, concluding that only aggregation is capable to give such rapid change in PSDs scale parameter  $\lambda$ . I think that the potential given by the PSDs retrieval is here underutilized. Given the full PSD and the m-D and v-D relations one can calculate interesting bulk quantities such as particle number concentration ( $N_t$ ) and ice water content (IWC) and their vertical fluxes (particle flux  $N_f$  and snow rate SR). It will be extremely interesting to see time-height plots of this quantities in connection with the results in figure 4 and 5. For instance, positive variation of  $N_f$  and SR should be seen in connection with the newly developed mode in fig4d. This analysis would also help in the identification of the significant aggregation process. Depositional growth and riming are in fact expected to increase SR leaving  $N_f$  unchanged (unless newly nucleating particles are present). Aggregation has the distinctive effect of decreasing  $N_f$  leaving SR unchanged and this should appear in the suggested plots.

10) At line 27 of page 12 it is mentioned that the methodology described in Mitchell (1988) has been used to model the evolution of  $\lambda$  parameter, however it is not specified the exact model used. It is surprising that the formula for the depositional growth rate from Pruppacher and Klett (1978) has been fully reported and not this. This is potentially a serious issue regarding the reproducibility of the results. Additionally, I think that a better explanation of the model used will give the reader a better understanding of the other variables that influence the PSD evolution due to aggregation such as particle sizes, velocity differences and total number concentration.

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11) The conclusions about the value of the  $E_{agg}$  are supported by only a 2 minute average profile during one event. I would, at least, model the evolution of the  $\lambda$  parameter for other times during the same event, or, even better, model more events.

Technical corrections

12) When presenting the state of the art of Doppler/multi-frequency radar retrievals at line 20-25 of page 2, I suggest to consider some recent studies like Chase et al. (2018) or Leinonen et al. (2018) in the discussion.

13) The choice of the colormap used in figures 2, 3 and 5 is particularly unfortunate. There is an apparent overlap of light-blue colors for different values that makes the interpretation of the figures more difficult than it should be. In figure 4b there are vast areas of the cloud where I cannot say if the DWR is either +1 or -1 dB. In figure 5 the mapping from the colorplot to the profile is made even more difficult by the fact that the profile as been cut from the panel with a white line; here I also suggest to indicate the profile with a thin rectangle around instead of the white line.

14) Figure 4 – Personally I would swap the axis in panels b, e, h. This would put velocity on x-axis, matching the concept on panels a, d and g. Also it appears that DWR is rather a function of velocity and not the opposite (see in particular panel e). That is a personal preference and I would leave to the authors the decision.

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