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Interactive comment on "The importance of particle size distribution shape for triple-frequency radar retrievals of the morphology of snow" *by* Shannon L. Mason et al.

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We thank the reviewers for their constructive feedback on the manuscript. A recurring suggestion was that we apply the retrieval to more data than the 25-minute case study from 21 February 2014 originally used. We agree that this is desirable, reiterating that while the colocated remotely-sensed and in situ measurements of snow from BAECC 2014 are extremely valuable and of a high quality, the number of cases are limited. A related comment was that we should more clearly acknowledge the limited measurement period to which our retrieval was applied. In addressing both of these suggestions, we have both included an additional case study and de-emphasised the

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retrieval of the PSD shape parameter in discussing the results.

During the snow experiment intensive observation period of BAECC there were three cases in which all three radars were zenith-pointing during a snow event, and where the snowfall at the surface was not affected by melting (the cases shown in Kneifel et al. 2015). The snowfall at the surface during one of these cases (7 February 2014) was insufficient for the in situ snow retrieval of von Lerber et al. (2015). We have therefore expanded our study to include 60 minutes of snowfall from the 16 February 2014 case. This case also includes riming, but is notable for the presence of secondary ice production due to rime splintering (the Hallett-Mossop process). These secondary needles rapidly aggregate, such that the radar measurements in this case are dominated by large aggregate snowflakes with a very open structure, while the in situ measurements include a mixture of graupel, large aggregates, and needles.

In applying our retrieval to this case, it was evident from PIP measurements that the PSD shape was nearly constant, but that significant changes in the triple-frequency radar signature could be attributed to the presence of large aggregates of needles, consistent with the findings of Leinonen and Moisseev (2015). We have therefore expanded the scope of the study to include the effects of variations in the internal structure of aggregates, which are represented within the SSRGA. We hope the reviewers agree that expanding the study to address both the PSD shape parameter and the internal structure of aggregates strengthens this effort to better understand and interpret the parameters affecting triple-frequency radar measurements.

To summarise, we have made the following changes:

- The title is now, “The importance of particle size distribution and internal structure for triple-frequency radar retrievals of the morphology of snow”
- We added a coauthor, Leonie von Terzi at the University of Cologne.
- We expanded our discussion of the coefficients of the SSRGA, especially those

relating to the internal structure of aggregate particles, in Sections 2.1 and 3. L. von Terzi's contribution to the study was to perform simulations of aggregation of various monomers and their SSRGA coefficients; we use this to identify aggregates of needles as having triple-frequency radar signatures with especially low values of DWR_{35-95} compared to aggregates of other monomers.

- Section 5 now uses the 16 February case study to explore triple-frequency radar measurements and retrievals from a case featuring rime splintering. This is a very distinct situation from the first case study, and combined the two cases cover the wide range of triple-frequency radar measurements from during BAECC 2014.
- The discussion and conclusions (Section 6) have been substantially re-written to be more concise, while addressing the expanded scope of the paper.

General comments

Why do you use the mean Doppler velocity at 35 GHz in the retrieval? I was unable to find why this frequency is optimal for the retrieval methodology. Please add some information.

We now note in both Section 2.2 and in the case study and retrieval (Section 4) that the 95 GHz radar had a mispointing error during BAECC 2014 that makes it difficult to use the mean Doppler velocity. We therefore use the Doppler velocity from the most sensitive available instrument, the 35 GHz. In practice for snow near the surface, the 10 GHz mean Doppler velocity could also have been used.

PIP PSDs and other products are available at 1 min resolution. Why do you choose 5 min? If it is for better statistics through averaging, please make that

point. Also, if you are using these values during the 25 minutes of the event, this essentially leaves 5 points for comparison with the retrieval. Could you expand on why you feel this is enough in situ data for assessment of aspects of the retrieval?

We are not using the PIP data directly, but rather the retrieval of von Lerber et al. (2016), which includes an estimate of the snow bulk density. This method requires a sufficient sample of ice particles, hence the use of a 5 minute resolution. We have now added this explanation to Section 2.2:

> In situ measurements of snow at the surface are provided by the Particle Imaging Package (PIP) video disdrometer (Newman et al. 2009). While the temporal resolution of PIP measurements is 1 minute, estimates of parameters of the PSD and particle properties are made over 5 minute intervals in order to increase the statistical sampling during BAEC while still resolving changes in the properties of snowfall at the surface, as described in von Lerber et al. (2014) (also Moisseev et al. 2017, Tiira et al. 2016. The method of moments is used to estimate the parameters of the Gamma distribution from the measured PSD (Moisseev and Chandrasekar, 2007).

How did the prefrontal versus frontal period get defined? Is it purely from radar features? Maybe something more rigorous or not from the radar would be more appropriate (since you are using the radar to evaluate the method). Is there collocated met equipment that can be used to determine the onset of the front? You have collocated radiosondes – are those used to determine the timing of the front? Or maybe could help justify the time chosen.

We agree that the definition of the front was not clear, and in fact was probably not necessary to make the analysis, which is more focused on comparing the rimed and

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unrimed snow. We now refer to the two regimes in this case as “rimed” and “unrimed”, rather than “prefrontal” and “frontal”, based on the transition in radar and particle imaging measurements (Figs. 6 & 7).

Since one event is being used to test the efficacy of this method, I think this needs to be emphasized. Also, would be good to argue why this one event may be applicable to other similar particles or events in different locations.

We agree. We now more strongly stress that the limited cases from BAECC 2014 help to demonstrate the effects of the PSD shape parameter and internal structure of aggregates on triple-frequency radar measurements, but that longer and more diverse measurements are needed to better understand their relative importance to global snowfall.

Specific comments

Unless responded to directly, we have appreciatively made the following changes.

Page 2, Lines 7 – 11: Split into two sentences

Page 2, Lines 26 – 27: “but it remains to explore...” does not make sense

Page 2, Line 31: Be consistent with your “-“ or not for frequencies (either all should be 35-GHz or should be 35 GHz

Page 3, Line 12: replace the “measurements;” with “measurements.”

Page 3, Line 14: “advantages” should be “advantage”

Page 4, Line 1: Define CAPTIVATE

Page 4, Line 23: Make mass-size equation on own line with equation number

Page 4, Line 27: "AR" should be in parentheses. Also, the author goes between saying AR, axial ratio, and aspect ratio throughout the document. Be consistent (I recommend "AR" since you define it)

Page 5, Line 5: Should measurement vectors be numbered as well?

Page 7, Line 28: aspect ratio "AR"

Page 7, Line 34: "The range of radar signatures is overlaid with the measured triple-frequency radar data from Hyytiälä..." You already talk about the shape of the data (the hook feature) earlier. I think you should introduce the overlaying of this data before getting into the description above.

In response to this and other comments, we have removed the overlaid radar measurements from these figures (as well as adding a second kind of aggregate snowflake model). This makes the main focus of this figure the sensitivity to the different parameters, and also shows more clearly the fit to observations when we show the case studies. The discussion has been modified accordingly, and is hopefully now more linear.

Page 9, Line 2: 3mm for the spheroids but maybe more like 4mm for the fractal particles?

We now just say "around 3mm".

Page 9, Lines 10 - 11: Reference needed.

This was the result shown in the figure. To better link the earlier result, we now write,

> We have shown that, based on simulated radar backscatter cross-sections, the PSD shape parameter has a greater influence on the

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simulated triple-frequency radar signature than the well-known effect of particle density.

Page 9, Line 34: “We may therefore..."

Page 12, Lines 3 - 5: Please add some details about the frontal passage – i.e., met data or observations that are not radar or particle focused. This will justify better your distinction of the two regimes (since you are using radar and in situ particle obs to test and assess the retrieval).

As stated above: we have chosen to de-emphasise the frontal passage in this case, as it is not strictly necessary for interrogating the snow microphysics. We have added a reference to the more detailed dicussion in Kneifel et al. 2015.

Page 12, Lines 20 - 22: Reference needed

Page 14, Lines 2 - 3: “...suggesting that some rimed particles persist after 23:03 UTC.” Could it be the choice of timing of the prefrontal versus frontal is off? Would using collocated met data clarify this?

As discussed earlier, we now simply distinguish between rimed and unrimed snow regimes. In this context, it doesn’t seem especially problematic that there would be a mixture of rimed and unrimed snow in the transition between the regimes.

Page 14, Line 7: "...in situ measurements at the surface.” Please specify what this is from—I assume the PIP measurements?

Page 18, Lines 12 —13: I do not understand “A significant difference between the frontal and the prefrontal profiles is that all retrievals are able to represent the observed profile of mean Doppler velocity below about 1.5 km,” as I am not see this. Could you please add some details as to what you are referring?

We agree that this was unclear. This sentence now reads,

> A significant difference between the profiles is that in the unrimed regime retrievals that do not assimilate Doppler velocity are able to represent the observed profile of mean Doppler velocity: this is because the a priori density factor ($r = 0$) makes a reasonable estimate of the terminal fallspeed of unrimed aggregate particles, provided that their size is well-constrained by dual-frequency radar measurements.

Page 19, Lines 9 – 11: The truncation you refer to here – are you talking about the PIP or the method? Both have lower limits. And technically snow is always dominated by small particles – just less so or more so depending on the shape of the PSD. So I do not think this is the correct sentiment here (i.e., even when a PSD is quite broad with lots of large aggregates, there still tends to be 1 to 2 orders of magnitude more small particles. When the PSD is quite narrow that is more like a factor of 3 to 4 orders of mag... but still lots of small particles in a broad distribution).

We now cite Moisseev and Chandresekhar (2007) regarding the effects of disdrometer truncation on the method of moments.

Pages 19 – 23: This section almost feels a bit out of order. It is like there are conclusions at the beginning and the discussion of application shown in Fig. 9 further into the section. It may help potential readers to move the discussion of the Fig. 9 to earlier in this section and move the verbiage in the beginning of the section to later – as a transition to conclusions.

Thank you for this—we agree, and hope that the discussion and conclusions have benefited from a significant re-write. We have opted to remove Fig.9 after introducing additional figures for the second case study, while still briefly discussing that it is unknown how closely related the PSD shape parameter is with riming.

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Hopefully the flow of the discussion and conclusions is now clearer and more concise.

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