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Atmospheric Measurement Techniques Discussions

Interactive comment on "Bulk density and its connection to other microphysical properties of snow as observed in Southern Finland" *by* Jussi Tiira et al.

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The authors greatly appreciate the effort of Dr. Andrew Heymsfield in carefully reviewing our manuscript. We have addressed each of your concerns in our response below.

A marked-up version of the manuscript indicating the changes we have made is attached as a supplement to this response. All page and line numbers in our comments are in reference to that document.

1 General comments

Q1. *My most major concern has to do with the near-absence of a discussion of what areas would most benefit from the results presented here. It is mentioned that the properties of ice particles are an area of continuing interest for ground, airborne, and satellite remote sensing retrievals. For example, which disciplines are interested in using the "volume equivalent diameter", or "the population-mean density"? I don't see these parameters being useful for weather forecast or climate modeling because a size-dependent density is needed for these studies. To address this question, I suggest that the authors look at the citations for the Brandes article and determine what type of studies are needing these parameters.*

Response. Great suggestion. We have done some reference analysis on the Brandes article and found that especially their ρ-D_0 relation is used in characterizing winter precipitation microphysics using radar and NWP parametrization. Further discussion and references were added in the introduction.

Changes. Added a paragraph starting p.3 l.8.

Q2. *More could have been done to estimate the density of individual particles, at least as a reality check on the ensemble-bulk densities that they do derive. Crude estimates could have been made by using particle terminal velocities and approximate cross-sectional areas (viewed from the side) and Best Number-Reynolds Number relationships that are referred to (Bohm, etc). Also, would it have been possible to derive the fractal dimension of the particles from their cross-sectional areas and terminal velocities, such that the exponent in the mass-dimensional relationship could be estimate?*

Response. Yes, we completely agree and the proposed studies are in the progress. Since there are many details associated with each of these studies, we have decided

to keep them separately and not to include in this manuscript. We have not studied whether it is possible to derive the fractal dimension.

***Q3**. If there are collocated radar observations, it would be interesting to use the PSDs and estimate densities to forward-model the radar reflectivity and compare against the meausurements.*

Response. We are working on this as well. Our plan is to have a number of methods/estimates of snow microphysical properties, in order to have independent ways of estimating density and mass-dimensional parameters.

2 Specific and technical comments

***Q1**. Page 5, line 17: What was the motivation for using the equivalent area diameter?*

Response. Equivalent area diameter is the particle dimension used in the output of PIP software. We are converting it to volume equivalent diameter as it enables the retrieval of volume flux weighted snow bulk density.

***Q2**. Page 5, line 22: what is the approximate ratio of particle height to maximum particle width from your data set? It would be useful to show.*

Response. We have added two new plots, the histograms of aspect ratio and area ratio of the particles observed during 8 snow events, Fig.3. These are derived by fitting an ellipse utilizing the orientation of the particle with measures of maximum width and height of the bounding box. From the fitted ellipse we retrieve major and minor axis to calculate the aspect ratio and maximum diameter for defining the circumscribing sphere. The area ratio is defined as total area of shadowed pixels in respect to area of

the circumscribing disk. As can be seen from the these new images the aspect ratio median is 0.72 and the area ratio median is 0.65.

Changes. Changes in text have been made to section 2.2. to describe the output of the instrument (p.4 l.25 onwards) and to section 3.1. to justify the chosen correction factor assuming spheroid of axis ratio of 0.6 (p.6 l.15 onwards).

***Q3**. Page 7, Eq. (8). It would be good to mention here that rho is the population-mean average.*

Response. Changed to "–, where $\bar{\rho}$ is the volume flux weighted population mean snow density"

***Q4**. Page 7, line 6: "snow bulk density" to "mean snow bulk density" or "volume flux-weighted snow density"*

Response. Changed to "volume flux weighted snow density".

***Q5**. Page 7, line 15: "used diameter" to "diameter used"*

Response. Corrected.

***Q6**. Page 8, line 8: From the definition of D, what would this relationship be used for? Likewise, Page 9, Eqs. 11-13.*

Response. The relationship currently on p.10 l.29 is used in this paper for density retrieval by substituting the fitted v-D relations to Eq. (9). Considering the definition of D, the formulation of (11)...(13) should be useful for analysis of radar observations and their comparison to ground observations, see for example Kneifel et al. (2015). These points are now noted in sections 3.5. (p.10 l.28) and 4.2 (p.12, end of the page).

Q7. *Page 10, line 12: 5. . . 6 needs to be corrected.*

Response. Replaced with "value rarely exceeds 5mm".

Q8. *Page 11, line 6. "induced" to "derived"*

Response. Corrected.

Q9. *Page 11, Eq. (19): Eq. (19) assumes that the PSD goes from 0 to infinity. Will this assumption induce some error?*

Response. Yes, it will result in overestimation of the ensemble mean density. This overestimation is not very large, considering over uncertainties. We have added new error analysis and discussion in the text.

Changes. Added a new Chapter 3.4 (starting p.9) and further discussion on p.14 l.17 onwards.

Q10. *Page 12, line 20: consecutive winters.*

Response. Corrected.

Q11. *Although you show the temperatures of the observations in Table 1, it would be good to me*

Response. Unfortunately, your question was truncated. Our understanding is that you would like to see the derived parameters, i.e. PSD parameters and snow density, to be expressed as a function of temperature. This approach is followed in Brandes et al. and in a number of other studies. We have decided not to do this, because in our opinion the correlation between N_w and D_0 reported in this manuscript adds something new. Also there is a demand for other retrieved snow microphysical properties, which can

be used to improve our understanding of a connection between snow microphysics and radar multi-frequency observations as was presented in Kneifel et al. 2015, for example.