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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 deeply appreciate the time and efforts of the Anonymous referee #3 in reviewing the 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.

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Discussion paper

1 General comments

A small correction to your summary of the manuscript: Instead of using a "5-min (or more) temporal resolution", the variable temporal resolution for derivation of mean snow bulk density and PSD parameters could go down to a minimum of 1 minute (in the case that LWE precipitation intensity was higher than 0.1 mm min^{-1}). The shortest integration time, $\tau(t)$, in the analysis of cases in Table 1 was two minutes. The following sentence was added to section 3.2 to clarify this:

Effectively, the temporal resolution of the mean bulk density retrieval is increased with increasing precipitation intensity, and in the analysis of the snowfall events in Table 1, the median $\tau(t)$ was 5 minutes.

***Q1**. The equi-volume diameter of each individual snowflake/particle is estimated by multiplying the PIP equivalent-area diameter by 0.92. This number has been found by simulating the relationship between the PIP diameter of the 2D projection of a rotated spheroid and its equi-volume diameter (see p.5, l.16-21). This equi-volume diameter is central to the study as the PSD is derived from it, but there is no discussion about the uncertainty of this estimation. Real snowflakes are not rotated spheroids, and I am hence wondering what is the spread of the real equi-volume diameter around the 0.92 estimate, and subsequently the uncertainty in the fitted PSD parameters.*

Response. We have added a figure that shows how this correction factor depends on ellipsoid dimensions, assuming that snowflake shape can be approximated as an ellipsoid. We should note that this approximation is only used to capture main effects of viewing geometry on estimated particle size. Typically, the multiplication factor varies between 0.8 and 1. For ice particles with axis ratios smaller than 0.4, i.e. pristine ice crystals, this factor could approach 1.4. From this analysis we can see that the largest expected error is associated with observations of ice crystals. Dimensions of snowflake aggregates and graupel like particles are expected to be captured with a smaller error.

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Changes. Added Fig. 2, discussion starting p.6 l.15.

Q2. *The relationship between D_0, N_w and the density is quantified using linear correlation. This correlation quantifies the co-fluctuation of two random variables, but does not tell anything about causality. I would suggest to investigate the possible link between N_w and density by using multiple or partial correlation, in order to remove the influence of D_0 in the co-fluctuation of N_w and density.*

Response. Thank you for the suggestion. We have performed the partial correlation analysis of the relation between N_w and density while controlling for D_0. It was found that there is a moderate negative partial correlation, -0.33, between N_w and density while controlling for D_0. However, the zero-order correlation between N_w and density is 0.52. The analysis confirms that the observed relation between N_w and density, is due to their relation to D_0. It is not clear, however, what is the meaning of the found negative partial correlation between N_w and density. We have added the corresponding text to the manuscript.

Changes. Added a paragraph starting p.15 l.12.

Q3. *It is more a wish than a request: the PIP provides information about the particle type, and it would be very interesting to conditioned the analysis on particle type as well...*

Response. Indeed, it would be a very interesting study. As there currently exists no reliable automatic classification method for particle type for the PIP data, such analysis is out of the scope of this study. The PIP provides information on particle shape, which is currently not used for automatic classification.

2 Specific and technical comments

***Q1**. Title: I would change the word "snow" into "snowfall", to clearly indicate the difference with studies of snow density on the ground.*

Response. Changed to "falling snow".

***Q2**. P.2, l.8: Garrett et al. (2012) would be a better reference here I think.*

Response. Corrected.

***Q3**. P.6, l.9: a reference about the employed method of moments?*

Response. Added.

***Q4**. P.6, l.26: correspondingly.*

Response. Corrected.

***Q5**. P.6, l.27: How many time steps are filtered here, and how representative is the remaining set?*

Response. The filter conditions are summarized, and the total numbers of time steps included and excluded in the analysis given in the beginning of section 4.3 (p.13 l.9-11).

***Q6**. P.7, Eq.8: a D_{max} is provided as upper integration limit, while it is implicitly assumed to be $+\infty$ in Eq. 2-3. Please clarify.*

Response. We have added an additional error analysis to see how this affects our retrieval. The results of this analysis are summarized in the new Fig. 6.

Changes. Added a new Chapter 3.4 (starting p.9 l.35).

***Q7**. P.7, l.27: the mean/median values are close to the commonly assumed values, but Fig. 2 shows a potentially significant spread around these values. For instance, the mode (most likely value) is around 6-7, and it would be instructive to provide an interquantile range (asymmetric distribution) to quantify this spread.*

Response. We have added interquantile ranges to the Figs. 3 - 5.

***Q8**. P.8, l.3: "the agreement is rather good": please provide quantitative descriptors (correlation, RMSE, bias, etc.) of this agreement.*

Response. We have given quantified measures for the comparison and the text has been altered:

Changes. "It can be seen that the agreement is good, with RMSE of 0.30 cm, linear correlation coefficient of 0.88 and normalized bias as low as -0.06." (p.9 l.32)

***Q9**. P.8, l.10-11: fitting a power law in the log-log space using a linear regression does not provide optimum parameter values in the linear space... It should be mentioned.*

Response. This point is now stated and justified by the fact that we don't want the fits to overly emphasize the large end of PSD.

Changes. At the end of p.10: "It should be noted, that using linear regression in log-log space does not optimally minimize residuals in linear space, but the method is used here as it does not overly emphasize the large end of the size spectrum."

***Q10**. P.8, l. 29: I would move "were recorded" in between "rates" and "on average".*

Response. Corrected.

Q11. *P.9, l.5: any comment on the possible explanations of this variability?*

Response. It is probably associated with riming. higher a_v and b_v values correspond to rimed particles.

Changes. Addition to p.11 l.32: "–, which possibly indicates the onset of riming."

Q12. *P.9, Eq. 11-13: please provide a quantitative descriptor of the goodness-of-fit of these power laws!*

Response. Added values of RMSE.

Changes. Addition to p.12 l.26: "with RMSE values of 0.30 m s^{-1}, 0.30 m s^{-1} and 0.35 m s^{-1}, respectively."

Q13. *P.10, l.3: the term "riming degree" is coming out of the blue here...*

Response. The link between riming and density is now stated in this context.

Changes. Addition to p.12 l.32: "–, which in turn are strongly connected with density (Power et. al. 1964)."

Q14. *P.11, l.5: remove "more" before "colder".*

Response. Corrected.

Q15. *P.11, Eq.17-18: please specify what are the integration limits! In Eq.18, shouldn't it be 0.1^{-3} rather than 0.1^{b_m-3}?*

Response. Integration limits have been added with discussion after the equations. Additionally, there was an error in (19), it should have been 0.1^{-3}, which is now also corrected. While errors were present in the equations in the manuscript, the values have been calculated correctly.

Changes. Corrected equations starting on p.14 l.17 and added supplementary discussion.

Q16. *Fig.4 and 5: it is not easy to spot the a, b, c markers. They should be made more visible (upper part?).*

Response. Thanks for the note and suggestion. Markers have been moved up and the corresponding time intervals have been highlighted with light grey background colour.

Q17. *Fig.12: if the minimum integration limit is $D = 0$, then μ values should be strictly positive (otherwise N(D) is not defined). But there are values down to -2 in Fig. 12. . .*

Response. This is a known problem in the PSD parameter estimation. μ values should be larger than -1. Values below or equal to -1 would result in ill defined total number concentrations, for example, if calculated from such a distribution. However, because of estimation errors and because actual PSD are not necessary following the Gamma functional form, sometimes μ values found to be smaller or equal to -1. This is happening because when we estimate N_w, D_0 and μ, we are looking for a Gamma function that fits the best to an observed PSD. This Gamma function may not be in a strict sense a PSD, because we cannot calculate an N_t from the fitted parameters, for example. As you probably noticed, we are not using derived μ values quantitatively. We just concluded that the observed values are close to 0.