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Atmospheric Measurement Techniques Discussions

Interactive comment on "Characterization of disdrometer uncertainties and impacts on estimates of snowfall rate and radar reflectivity" *by* N. B. Wood et al.

N. B. Wood et al.

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Thank you for the comments and suggestion. Below, we've reproduced those comments and provided our responses.

1) Maybe it is a good idea to introduce the adjective 'video' in the title.

Yes, that would clarify the focus of the paper, and we would modify the title.

2) I do not find the adjective 'analytic' (as referred to errors) very pertinent. Isn't it better to use the term 'misclassification'?

We chose the term "analytic" because the sources of error include not only misclassification (i.e., an erroneous estimate of particle size from the particle image causing the particle to be counted in the wrong size bin), but also errors in the particle counts and in the calculation of the sample volume, all elements of the analysis of the particle images to obtain the size distribution. An error in the particle size might not cause the particle to be misclassified into the wrong size bin, but would contribute an error in the sample volume calculation, which would propagate into the size distribution calculation. We would prefer to retain the term "analytic".

3) Units are sometimes wrong. dBz units are for reflectivities but uncertainties in reflectivities are measured in dB!

Thanks - these units would be corrected.

4) Page 6330: the "expected observational uncertainties". It is not clear what the authors are referring to (i.e., disdrometers themselves are used to observe snow rates).

This statement would be edited to indicate "expected uncertainties for radar and precipitation gauge observations".

5) Table 1: instead of listing the different dimensions it would be nice to have a picture here with visual explanation of them.

Yes, we agree a figure would be preferable. We suggest Table 1 be replaced with Figure 1 attached to this reply, with appropriate changes to section 3.1 to refer to the figure, and explanatory text placed in the caption.

6) Tab 2-4, check the significance of the digits you are listing.

Yes, we expect that largely due to the precision of the disdrometer observations, reflectivities and logP are likely not modeled with better than 1 part in 100 significance. We would adjust the reported values in the tables to reflect that.

7) Sect.2: maybe it would be good to have an idea about the dataset considered.

Maybe you could do a pdf of the measured sizes for the two different disdrometers considered.

Histograms (PDFs) of the size distributions for each of the seven snowfall events and for both instruments would add several pages of figures to the paper. Instead, we've computed the averaged size distributions for the SVI for each event, and produced a single plot showing this for all seven events. This is shown in Figure 2 of this reply. We suggest it be inserted at the end of section 2, accompanied by a short explanatory text taken in part from the first paragraph of section 2.

8) I would use Z_e with subscript instead of Ze.

The use of Ze (un-subscripted) was used due to the need in some related papers to label reflectivities with additional subscripts. Although those additional subscripts are not used in this particular work and Z_e (subscripted) is typical, we feel the usage here is clear and would leave this as is.

9) Sect 4.: when the authors are referring to the observation vectory y they should motivate why they have chosen it to include Z and S. Why not consider snow water content for instance?

We agree this motivation is needed, but believe it is more appropriate in the Introduction, and would add text to paragraph 3 of the Intro, i.e.,

"In this work, the contributions of disdrometer uncertainties to uncertainties in models for near-Rayleigh radar reflectivity and snowfall rate are evaluated. These uncertainty estimates are essential for use in retrievals that would use coincident ground-based observations of radar reflectivity and snowfall rate or accumulations to estimate snow microphysical properties."

When combined with the description of prior work in paragraph 2 of the Introduction, we believe this clarifies the motivation for the construction of the forward model and observation vector.

10) The forms in Eq 16 and 19 are trivial They probably do not deserve to be reported.

Agreed. (16) would be removed and, instead, S_B described briefly at the beginning of section 4.1. Similarly, (19) would be removed and S_F described briefly at the the beginning of section 4.2.

11) End of Sect 4.2: again a plot with an example of the different 5-min PSD discussed would help the reader.

We've produced a 3-panel plot showing a time series of 2DVD particle sizes, the resulting discrete size distribution, and the truncated discrete size distribution for a single sample used in this analysis. It's attached as Figure 3, and we suggest it be inserted at the end of section 4.2 and referenced in the 2nd paragraph of 4.2.

12) The numbers at the end of Sect 4.2.1 and in Sect 4.2.2 are not consistent with the values listed in the Tab2.

Thanks, this would be corrected.

13) Sect3: it is not clear what measurement uncertainties in S are the authors referring to. For what instrument is the 0.3 uncertainty in log S applicable?

We believe this actually refers to Section 5, the second paragraph. This estimated uncertainty is intended to represent uncertainty in snowfall rates obtained from a precipitation gauge (e.g., a Geonor or Pluvio) measuring at short time intervals. There is a lack of experiments which might characterize this sort of uncertainty for high frequency snowfall rate measurements (i.e., which compare measurements from side-by-side gauges of the same type at short time intervals). This seemed a conservative estimate, in the the sense it seems unlikely the uncertainties would be grossly worse than this. We would add text in the second paragraph of section 5 to clarify this.

14) Fig3: y-label should read log S

Thanks, this would be corrected.

15) Sect5. The problem I see here is that the authors are collectively computing the uncertainties in the forward modelled Z and S, i.e. they are averaging over the whole dataset. Of course different errors will impact differently for different observed PSD and the chosen dataset may have a strong impact on the results. Maybe it would be better to cluster results according to reflectivities or snow rates. Otherwise the overall uncertainty of 4 dB, also quoted in the abstract, may appear a bit too discouraging.

Of the two major contributors to the uncertainties per Table 3 (S_F and phi), only S_F, representing discretization and truncation, shows signficant sensitivity to the size distribution. Following the suggestion, after binning the results for the HE10 microphysics in bins of 2 dBZe and the snowfall rate results in bins of 0.1 in logS, biases and residual uncertainties were calculated bin by bin. Note that S_F contains only the residual uncertainties after biases have been corrected. For dBZe, the residual uncertainty for discretization plus truncation averaged over the bin-by-bin results was reduced to from 1.87 to 1.04 dB, and for logS, from 0.081 to 0.053.

We would insert text into the first paragraph of section 5 indicating that using reflectivity- or snowfall-rate dependent bias corrections would allow the discretization and truncation uncertainties to be further reduced, and provide these results as an example.

16) Some of the formulas are trivial (e.g. B6-B7) and can be skipped.

We agree that the general forms such as B6 and B7 are trivial, but they succinctly define terms used in the discussion in the text, so we prefer to retain them.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 6329, 2013.

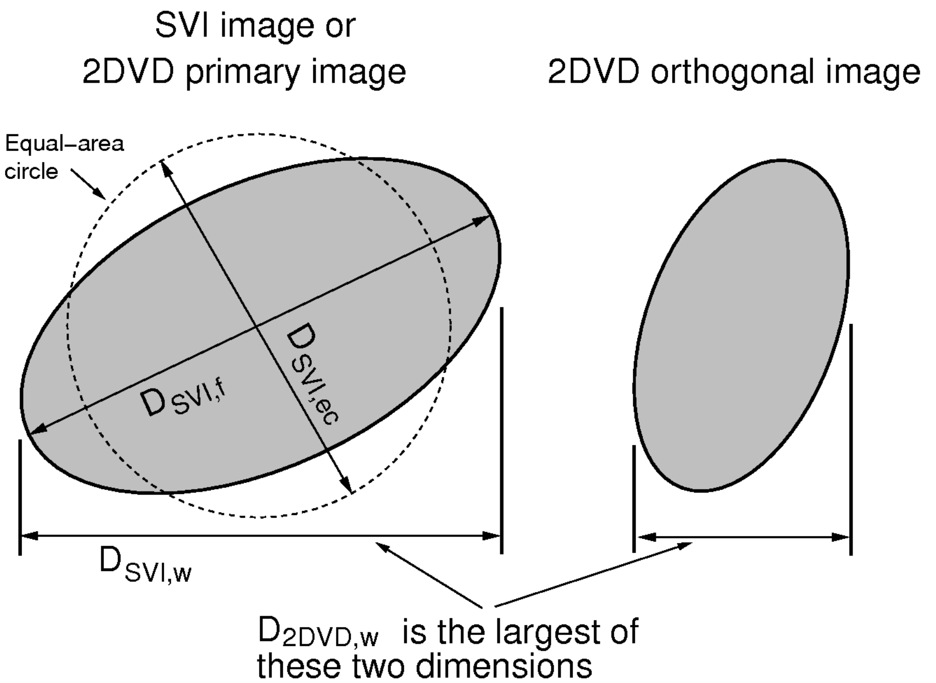

Fig. 1. Disdrometer-observed particle sizes

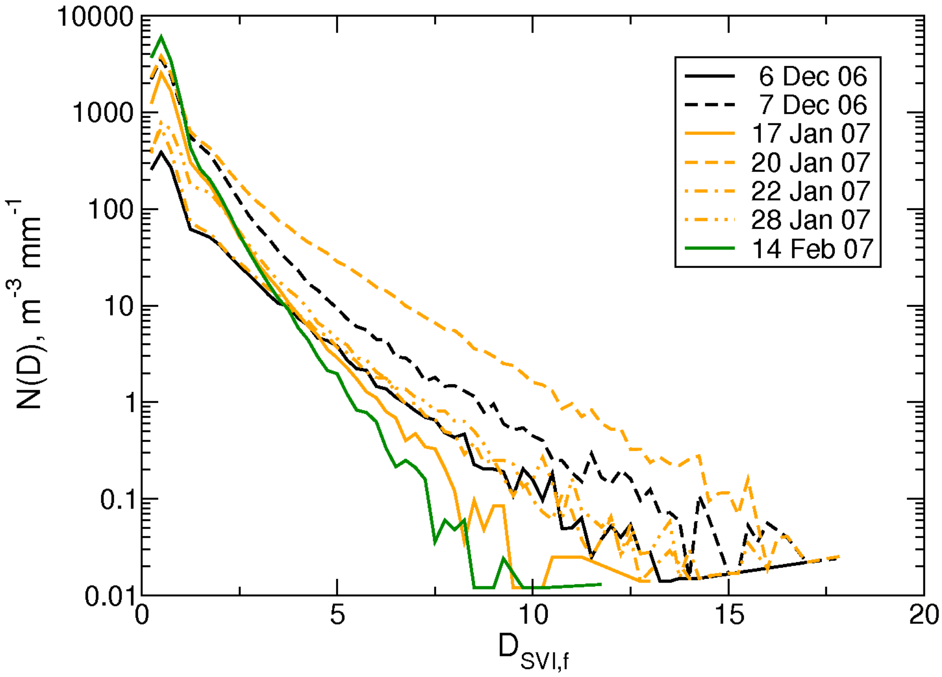

Fig. 2. SVI size distributions for snowfall events

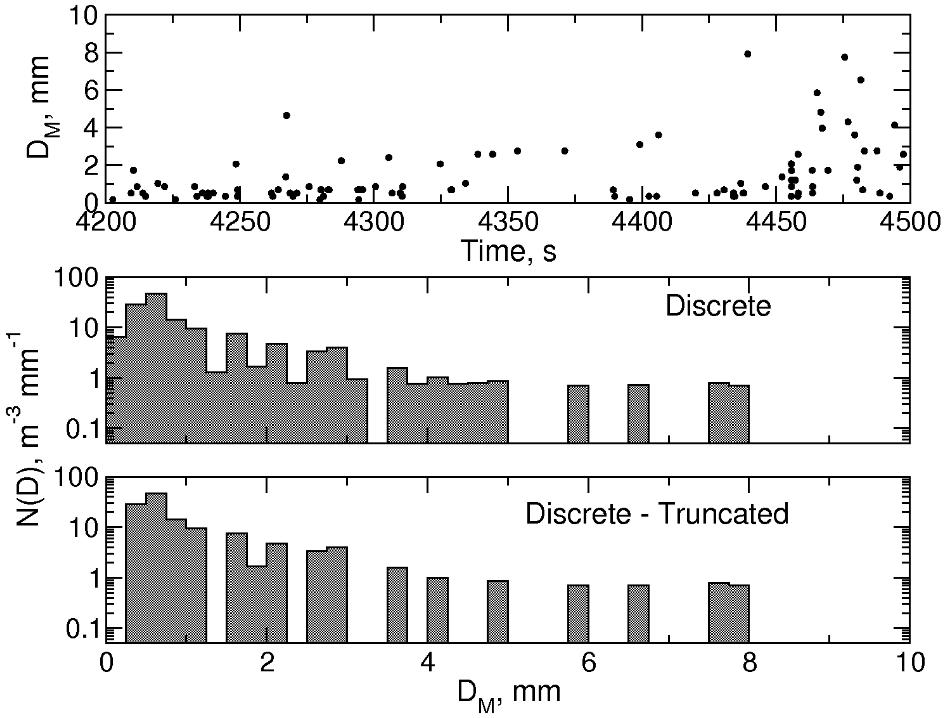

Fig. 3. 2DVD particle size observations (top) with corresponding discete and discrete-truncated representations