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Comment

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

## **Anonymous Referee #1**

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The paper presents an interesting study for characterizing video disdrometer uncertainties on estimates of snowfall rates and radar reflectivities. The authors consider sampling and misclassification as principal causes of errors in disdrometer measurements. The analysis is made possible by a sagacious combination of 2DVD and Snowflake Video Imager data collected during the C3VP field campaign. Particularly relevant is the discussion of what is the particle size which is actually measured by the video disdrometers and how that relates to the true dimension of snow particles. While the paper is generally well written I found sometimes difficult to follow the authors' reasoning, which is seldom supported by figures. I certainly recommend publication but I do

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recommend revisions (which I would characterize as minor/major and not indeed major) to facilitate the reader (especially the ones who are not expert in the field) according to the following guidelines.

- 1) Maybe it is a good idea to introduce the adjective 'video' in 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'?
- 3) Units are sometimes wrong. dBz units are for reflectivities but uncertainties in reflectivities are measured in dB!
- 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).
- 5) Table 1: instead of listing the different dimensions it would be nice to have a picture here with the visual explanation of them.
- 6) Tab 2-4 , check the significance of the digits you are listing
- 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.
- 8) I would use  $Z_e$  with subscript instead of  $Z_e$
- 9) Sect 4.: when the authors are referring to the observation vector  $y$  they should motivate why they have chosen it to include  $Z$  and  $S$ . Why not considering snow water content for instance?
- 10) The forms in Eq 16 and 19 are trivial They probably do not deserve to be reported.
- 11) End of Sect 4.2: again a plot with an example of the different 5-min PSD discussed would help the reader.
- 12) The numbers at the end of Sect 4.2.1 and in Sect 4.2.2. are not consistent with the

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values listed in the Tab2.

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?

14) Fig3: y-label should read log S

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 little bit too discouraging.

15) Appendices: some of the formulas are trivial (e.g. B6-B7) and can be skipped.

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

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