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Discussions

Interactive comment on "Climatological characteristics of raindrop size distributions within a topographically complex area" *by* S.-H. Suh et al.

Anonymous Referee #3

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The paper presents a detailed study of climatological characteristics of the raindrop size distribution (DSD) in Busan, Korea. The measurements were taken using a single disdrometer, over a three year and 10 month period. The disdrometer rain amount estimates were quality controlled by reference to a tipping bucket rain gauge.

Recorded precipitation events were divided into categories: typhoon, changma, heavy rainfall, spring, summer, autumn, winter, daytime, and nighttime. They were also classified as convective or stratiform. DSDs are approximated using a DSD model. Bulk variables considered are rain rate R [mm h^{-1}], mass-weighted mean drop diameter D_m [mm], and normalised intercept parameter N_w [m^{-3} mm^{-1}].

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Discussion Paper

The DSD model parameters and bulk variables are then compared for (a) stratiform and convective events, (b) climatological characteristics (using D_m vs N_w only) and (c) diurnal variation, with respect to winter and summer seasons (the other two seasons are ignored).

The title refers to a topographically complex area; however there is no discussion in the paper of how the Busan region is topographically complex. It is understood from the paper that there are complex interactions between ocean and land in this region. A topographic map of the region should be supplied and its topographic complexity discussed.

The climatological characteristics of the DSD are important for precipitation and weather modelling, and quantitative precipitation estimation from radar, and the investigation of DSD variability and climatological characteristics form relevant scientific questions. The results presented in this paper are useful for these purposes, but the authors have not explicitly explained how.

The analysis is based on a single instrument; I would like to see included a discussion on possible sampling effects related to the location of the single instrument.

The tools and methods used are not novel, but the paper reports on interesting new conclusions reached using a new dataset. The scientific methods are valid and assumptions are clearly stated. The conclusions are supported by the results.

A comparison is made between DSD characteristics measured in Busan to those measured in Järvenpää, Finland. This comparison appears to me to have been added as an afterthought. The comparison is not thorough and does not add substantial information to the manuscript, and it is mentioned only at a few points in the paper.

The paper presents interesting and useful results, but is scattered and unclear. There are some errors in the mathematical equations (see the technical comments). It requires more analysis, rather than statements of results. With revision this paper is

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publishable.

Specific comments

1. **Description of the DSD**. It would be helpful to have a short description of the DSD and its bulk parameters early in the manuscript. Describe what bulk parameters mean and their units, before introducing (e.g. on page 4008, line 7 where D_m is introduced without explanation). Units are often missing in bulk variable definitions. It is important to explain what each bulk variable means. For example, D_m provides one measure of the characteristic drop diameter of the DSD.

2. **Instrument description**. Please provide a reference to (a) paper(s) describing the operation of the POSS disdrometer, its manufacturer. A brief description of its operating principle should be provided in the manuscript. The coordinates of the instrument location should be given.

3. **Data quality control**. It is important that care is taken with quality control, and the authors have done a good job of explaining their quality control procedures. A description of the way in which the POSS disdrometer detects precipitation type would be helpful. On Page 4011, line 21, it is not clear whether DSDs with $D_m < 0.5$ mm were removed or not. What is the temporal resolution used? On Page 4011, line 25, the accumulated rainfall amount is stated as "about 4269 mm" but it is not clear what "about" means in this context.

4. **POSS to AWS comparison**. The comparison of POSS-derived rain amount with AWS-derived rain amount is important because it speaks to the performance of the POSS disdrometer. The RMSE is provided (units should be specified), but I would also like to see the bias. The plot (Fig. 3) is in log-log scale which means that differences at the start of the time series are highlighted, while differences at

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the end of the time series are hidden. Please provide the plot in linear scale. The final amounts (5081 mm for AWS vs 4269 mm for POSS) show that the there is significant bias between the two instruments and this should be addressed in the manuscript.

5. **Mathematics**. Equations 2, 6, 9, and 10 all require revision (see the following technical comments for details).

6. **Drop-shape model**. On page 4012, line 9, the authors state that they have performed numerical simulations and wind tunnel tests for the drop shape from drop diameter relation, yet no results or further discussion is provided. It is not explained why the diameter ranges for different drop-shape algorithms are so chosen. The two drop shape models are inverted (ie Eq. 13 is actually Beard and Chuang 1987, while Eq. 14 is Andsager 1999).

7. **Radar equations**. The radar equation in Eq. 11 is for non-polarimetric weather radar reflectivity derived from the DSD. Strictly, Eq. 12 is incorrect; it is not the definition of Z_h but rather the conversion of non-polarimetric Z from linear units to dBZ. The diameter D used in Eq. 11 should be specified (i.e. is it the equivolume drop diameter?). The definitions of Z_{dr} and K_{dp} used are not provided, and nor is the assumed radar frequency.

8. **The comparison with Järvenpää**. What are the similarities and differences between Busan and Järvenpää, in terms of climate type and topographical complexity? For the comparison to be properly done, a section should be devoted to it and much more information, analysis, and results should be given. The few comparisons in the current paper read like an afterthought and should be expanded.

9. **Division into convective and stratiform**. In Table 3, the percentages of convective and stratiform rain do not add to 100%. An explanation of what the non-categorised rainfall is should be included. On page 4016, lines 12-14, the au-

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thors conclude that classifying rainfall as convective for $Z > 38$ dBZ would not be suitable, based on radar reflectivities in their classification using R (from Bringi et al. (2003)). It is not proven by this study whether the R-based method is, however, appropriate. This difference is important to state, but the authors lack sufficient evidence to conclude that radar reflectivity thresholds as per Gamache and Houze Jr. (1982) can not be used.

10. **Stratiform/convective PDFs (Section 3.1)**. There is a lot of discussion of PDFs in the manuscript, and the writing is often unclear. A distribution cannot be "more frequent" as stated on Page 4014, line 21. The word "frequencies" on page 4016, line 7, is misused; the authors mean that most values lie between 25 and 55 dBZ. On page 4015, lines 11 to 17, a contradiction is found (stratiform rainfall has larger raindrops than convective, yet the opposite is observed). This contradiction requires explanation. Overall, many results are stated from the plots, but there should be more discussion of what the results mean and imply.

11. **Climatological characteristics (Section 3.2)**. Figure 5b shows D_m vs. $\log_{10}(N_w)$ with vertical lines representing the standard deviation around the mean value of $\log_{10}(N_w)$. Horizontal bars should show the spread in D_m as well. A linear model for the relationship between these two variables is found using the mean values for Typhoon events; it would be better to fit this relationship to all data. Instead of using standard deviation, I would recommend using an interquartile range to give an indication of the skewness of the distributions for these variables.

12. **Diurnal variation (Section 3.3)**. Figure 8 shows (presumably) average values of various variables. These distributions are likely to be skewed (indeed as shown in Fig. 9) and should therefore be shown through, for example, box-and-whisker plots instead of simple means. At the least, the skewness of the distributions should be mentioned in the manuscript, and it should be explicitly stated that Fig-

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ure 8 shows mean values. The paragraph on Page 4019, lines 22 to 27 should either be moved to the introduction/literature review section, or should have further explanation on how these previous results compare to the current results. The results on diurnal variations of DSDs with respect to season are interesting. The paragraph on Page 4021, lines 13 to 18, is unclear. Fig 12 again shows mean values for variables that are likely to have skewed distributions, so box-and-whisker plots should be used.

Technical comments

- Page 4006, line 7: specify that the 'shape parameter' is the DSD model shape parameter and refer to model definition.
- Page 4006, line 11: The categories not only cover different temporal and spatial scales, but different rainfall types.
- Page 4006, line 24: The DSD describes microphysical properties, it does not control them.
- Page 4007, line 23: Replace "fall to ground" with "fall to the ground".
- Page 4009, line 5: Define the DSD mathematically (as $N(D)$).
- Page 4009 and onwards: Equations should be followed by punctuation (e.g. a comma or full stop).
- Page 4009, Eq 2 is incorrect, the first numerator in Testud et al. 2001 for $f(\mu)$ is $\Gamma(4)$, not 6.
- Page 4010: Units are missing for D_m, LWC, and R.

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- Page 4010, line 6: Define “SD” before use.
- Page 4010, line 11: Remove “and rainrate (R)”.
- Page 4010, Eq 6 is incorrect, this is the expression for σ_m, not σ_m/D_m (see Leinonen et al., 2012).
- Page 4010, Eq 9 is incorrect, R requires $v(D)$.
- Page 4011, Eq. 10 is incorrect, -0.6 should be -6.
- Page 4013, line 3: The fact that one-minute DSDs are used should be mentioned in the instrument description in Section 2.2.
- Page 4013, line 20: “especially Busan” should be removed since Busan is mid-latitude.
- Page 4014, line 10: Section 3.1 is really more about comparison between stratiform and convective rainfall than DSD and radar parameters.
- Page 4014, line 19: The table reference is incorrect and should be Table 3. The convective contribution is stated in the table as 5.7% but 5.8% here.
- Page 4015, line 21: “often” is too vague.
- Page 4016, line 12: What are the differences between the frequency distributions in Busan and Darwin?
- Page 4016, line 17: From Fig. 4g, this value (0.8) should be 1.8 dB?
- Page 4017, line 7: Table 3 should be introduced when the catagorisation is first mentioned (Section 2.4).

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- Page 4017, line 13: An average cannot “spread”. Specify that what is meant is standard deviation around the mean to represent distribution spread.
- Page 4020, lines 24-25: This sentence contains repeated information.
- Page 4020, line 27: No clear pattern for any N_w value; “larger values” is unclear (refers to time of day?).
- Page 4022, line 27: Replace “DT” with “day”.

[Note references mentioned here are the same as those in the reviewed manuscript].

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 4005, 2015.