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Hydrology and Earth System Sciences
Discussions

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

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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 es-

timates 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 Dm [mm], and normalised intercept parameter Nw [m-3 mm-1]. The DSD model parameters and bulk variables are then compared for (a) stratiform and convective events, (b) climatological characteristics (using Dm vs Nw 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 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 Dm is introduced without explanation). Units are often missing in bulk variable definitions. It is important to explain what each bulk variable means. For example, Dm provides one measure of the characteristic drop diameter of the DSD.

A. Thank you for your comment. I did not consider a short description of the DSD and other parameters and I appreciate to your kindness example. If I have a chance, I will add the short comment and description of DSD and other parameters.

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.

A. Thank you for your comment. I understand and agree with your idea. While I modifying the present paper, some part of sentence related with POSS disdrometer is removed. I will add the more description of POSS disdrometer. Also, I missed the detail explain of location of POSS disdrometer in Sec. 2. However, in present paper, coordinate of the instrument location was presented at the abstract and page 4008 line No. 22. It is located at 35.12° N, 129.10° E.

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 Dm < 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.

A. Thank you for your comment. This is type error. As you seen that, the threshold is not Dm < 0.5 mm but D0 < 0.5 mm. I will modify as soon as possible. And the temporal resolution is 1 min for the entire period. I was already explain on the Table 3. However, I would like to add the explicit word in main sentence to avoid confuse. Also, I agree with your point. I will modify the word ‘about’ and add the specific word ‘4269.060 mm’.

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 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.

A. Thank you for your comment. I did not consider the unit value of RMSE for a rainfall amount between AWS and POSS disdrometer. I will add the unit value of it (mm). And, I also understand to your idea to make linear scale of plot chart between AWS and POSS accumulated rainfall amount. If I have a chance to modify the figure, I will modify it.

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

A. Thank you for your kindness.

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).

A. Thank you for your comment. This is fatal type error and I am so sorry to make you confused. What I want to say in present paper is not 'we performed' but 'we assumed'. Therefore, we assumed the drop shape relations created by Andsager et al. (1999) and Beard and Chuang (1987) as used by Bringi et al. (2003). If I have a chance to modify this sentence, I will modify right away.

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 Zh 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 Zdr and Kdp used are not provided, and nor is the assumed radar frequency.

A. Thank you for your comment. I though the horizontal radar reflectivity is same to the non-polarimetric radar reflectivity (Z). As consider of your idea, I will modify the definition of equation properly. And, the symbol 'D' means 'the volume-equivalent raindrop diameter'. For the explicitness, I will explain the definition of D more detail. Last, the radar equations like as Kdp, Zdr is the results of T-matrix simulation based on the POSS data. The condition of T-matrix simulation is already explained at page 4012 line 8 to page 4013 line 3. Therefore, I did not add the detail equation of dual-pol radar parameters.

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.

A. Thank you for your comment. The reason why the results in Busan compared with Järvenpää is that the observation period (Järvenpää : 5 years, Busan : 4 years). Also, I want to show the differences of dominant DSD between two reason (Järvenpää :

high latitude, Busan : mid latitude). Also, observation site in Järvenpää is considerably close to the ocean (about 30 km). However, I did not explain the reason why the present results are compared to Järvenpää. I will add the detail explain if I have a chance as you told.

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 authors 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.

A. Thank you for your comment. First of all, rainfall type division method is cited by Bringi et al., 2003. Stratiform rainfall type is considered when rainfall rate larger than 0.1 mm h-1 as well as standard deviation of rainfall rate smaller than 1.5 mm and convective rainfall type is considered when rainfall rate larger than 5 mm h-1 as well as standard deviation of rainfall rate larger than 1.5 mm. It means that this method does not consider the rainfall when 0.1 mm < rainfall rate < 5 mm as well as standard deviation of rainfall rate > 1.5 mm. Therefore, total percentage of stratiform and convective rainfall is not 100%. Second, considering rainfall division method by Bringi et al. (2003), PDF of convective rainfall higher than stratiform rainfall when Z > 34 dBZ. Also, I would like to show the convective rainfall thresholds of other research results depend on the location.

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.

A. Thank you for your comment. I want to say the value of PDF as 'Frequency'. I am so sorry to make you confused. I will change the word from frequency to distribution as you told. And, I miss the specific explanation as 'for a same rainfall rate'. If I have a change, I will add more specific sentence as soon as possible. Also, I want to show the dominant distribution of DSD parameters in mid latitude like as Leinonen et al., 2012. If I have a chance to study, I would like to deep research for the PDF of DSD part.

11. Climatological characteristics (Section 3.2). Figure 5b shows Dm vs. log10(Nw) with vertical lines representing the standard deviation around the mean value of log10(Nw). Horizontal bars should show the spread in Dm 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.

A. First of all, thank you for your good advice. However, some of previous studies did not show the standard deviation of Dm. I am so sorry to make you confused. Eq. (15) is made by the mean value of 10 categories rainfall type for all convective rainfall data not only Typhoon events. Therefore, your idea is same to the present study and I will change the sentence more detail to avoid confuse. Also, I am considering your advice to use an interquartile range. Thank you.

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 Figure 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.

A. Thank you for your comments and opinions. Actually, it is good method to use skewness of plot chart. However, we could find the clearness feature of time series easily in Fig. 8. If I have a time, I will study the skewness of plot chart. Also, shape of PDF (Fig. 9) is similar to the previous study (Leinonen et al., 2012). It means that I would like to show the intrinsic distribution of DSD and integral parameters between daytime and nighttime. It is more clear to compare between daytime and nighttime than using other method as I think.

Technical comments

Page 4006, line 7: specify that the 'shape parameter' is the DSD model shape parameter and refer to model definition.

A. Thank you for your comment. I understand and agree with your comment. I will add more specific expression.

Page 4006, line 11: The categories not only cover different temporal and spatial scales, but different rainfall types.

A. Thank you for your kindness explanation. I will modify like as you told.

Page 4006, line 24: The DSD describes microphysical properties, it does not control them.

A. Thank you for your point. This is type error. I will modify like as 'controlled by - '.

Page 4007, line 23: Replace "fall to ground" with "fall to the ground".

A. Thank you for your kindness. I will modify as soon as possible. Thank you.

Page 4009, line 5: Define the DSD mathematically (as N(D)).

A. Thank you for your comment, I will add more detail explanation of DSD definition.

Page 4009 and onwards: Equations should be followed by punctuation (e.g. a comma or full stop).

A. I miss the punctuation for all equations. Thank you for point.

Page 4009, Eq 2 is incorrect, the first numerator in Testud et al. 2001 for f() is gamma(4), not 6.

A. Thank you for your comment, Actually, the previous study (Bringi et al., 2003) used 6 not ĐŞ(4). Therefore, I used like as.

Page 4010: Units are missing for Dm, LWC, and R.

A. Thank you for your comment, I will add the units for each parameter.

Page 4010, line 6: Define “SD” before use.

A. Thank you for your point. I miss the definition of SD in present study. This is abbreviation of standard deviation (SD). I will add the sentence in present study. Thank you.

Page 4010, line 11: Remove “and rainrate (R)”

A. Thank you for your comment. I will eliminate this word.

Page 4010, Eq 6 is incorrect, this is the expression for sigma_m, not sigma_m/Dm (see Leinonen et al., 2012).

A. Yes and thank you for your point. This is typing error. I will modify as soon as possible. Thank you.

Page 4010, Eq 9 is incorrect, R requires v(D).

A. Thank you for your comment. Also, it is already advised by previous anonymous reviewer. I will modify right away.

Page 4011, Eq. 10 is incorrect, -0.6 should be -6.

A. Thank you for your comment. This is just typing error not technical error. Therefore, I will modify this equation.

Page 4013, line 3: The fact that one-minute DSDs are used should be mentioned in the instrument description in Section 2.2.

A. Thank you for your comment. Despite already shown in Table 3, I will add the specific expression in Sec. 2.2.

Page 4013, line 20: “especially Busan” should be removed since Busan is mid latitude.
A. Thank you for your comment, I will change the word from ‘especially’ to ‘including to’. Thank you.

Page 4014, line 10: Section 3.1 is really more about comparison between stratiform and convective rainfall than DSD and radar parameters.

A. Thank you for your comment. However, the main purpose of Sec. 3.1 is to show the dominant characteristics for each parameter. Of course, it could be fine the difference of distribution between stratiform and convective rainfall type. I will consider your idea. Thank you.

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.

A. Thank you for your comment. This is typing error. I will modify right away.

Page 4015, line 21: “often” is too vague.

A. It is compared to the word ‘rarely’ line 20. The reason what I want to use the word ‘often’ is that convective data also exist > 5.5 mm-3m-1 compared to the stratiform

data. However, if you feel confused I will modify the word from 'often' to 'more exist'.

Page 4016, line 12: What are the differences between the frequency distributions in Busan and Darwin?

A. Thank you for your comment. As I understand, PDF shape of radar reflectivity for the convective rainfall at lower altitude (3 km from MSL) in Darwin is similar to those of Busan. However, PDF shape for the stratiform rainfall type is considerably different each other. The reason why I explain the results of Darwin is to show the difference of DSD according to the locations.

Page 4016, line 17: From Fig. 4g, this value (0.8) should be 1.8 dB?

A. Thank you for your comment. This is typing error. I will modify the word from 0.8 to 1.8.

Page 4017, line 7: Table 3 should be introduced when the catagorisation is first mentioned (Section 2.4).

A. Thank you for your comment. I understand and agree with your idea. However, we considered the Table 3 is the one of the results. Therefore we select the location of Table 3 like as. Also, I will consider your advice. Thank you.

Page 4017, line 13: An average cannot "spread". Specify that what is meant is standard deviation around the mean to represent distribution spread.

A. Thank you for your comment. What I want to say is that 'the 10 mean values of Dm and Nw is distributed between 1.2 and 1.5 mm and 3.2 to 3.5, respectively'. I am so sorry to make you confused. I will change from the 'spread' to the 'be distributed'

Page 4020, lines 24-25: This sentence contains repeated information.

A. Thank you for your comment. Maybe the sentence in line 24-25 is similar to the sentence in line 20-21. I will merge the two sentences as one.

Page 4020, line 27: No clear pattern for any Nw value; "larger values" is unclear (refers to time of day?).

A. Thank you for your comment, Actually, the target of 'larger values' is Dm not Nw. Commonly Dm and Nw has negative relationship (you could find the feature at Fig. 5, 6 in present paper). However, time series of Nw in entire period (Fig. 12b) does not show the negative relationship compared to the Dm (Fig. 12a). For the convenience of explanation, I used the value of Dm instead of Nw. Also, the word 'larger' is matched to the 'relatively small' for Dm in line 26.

Page 4022, line 27: Replace "DT" with "day".

A. Thank you for your comment. I will modify the word 'night' to 'NT' not DT for the regularity.

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