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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.

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Anonymous Referee #2

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Summary:

This manuscript presents a detailed analysis of a 4-year drop size distribution (DSD)

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Interactive Discussion
Discussion Paper

dataset as obtained from a POSS disdrometer. This works presents both the general distribution of the parameters of a normalized DSD as well as the different rainfall integral variables. Furthermore, the authors show the impact of wind direction, daytime vs. nighttime and summer vs. winter on the characteristics of the DSD. Overall quality: Scientific research on the topographical variations of the DSD are very interesting. These variations result from different meteorological forcing mechanisms. Understanding these processes is important both from a scientific point of view as well as from a societal perspective (as it may lead to improved precipitation estimates. The current work presents analyses of a long term (4 year) disdrometer dataset observed in South Korea. The work presented here is very interesting. However, it fails to provide an in depth analysis on the processes leading to these variations in South Korea. Therefore, the manuscripts currently still has a number of concerns that should be addressed before it can be accepted for publication in HESS. Below you'll find a detailed overview of these concerns.

Major concerns:

1) Page 4007, line 23-25 states: "The raindrop size distribution of stratiform rainfall observed at the ground is larger than that of convective rainfall, due to the resistance of ice particles to break-up mechanisms". This statement might be true for maritime precipitation as e.g. observed by Tokay and Short, 1996. However, for continental precipitation it is really dependent on the type of precip around the zero degree isotherm (snow vs. ice) whether the average diameter is smaller or larger for stratiform precipitation in comparison to convective rainfall. See for an in depth discussion on this issue Hazenberg et al. 2011.

A. Thank you for your comment and I agree to your opinion. I would like to say the common characteristics of rainfall rate for each rainfall type. But I did not explicit the specific word like as 'for a same rainfall rate'. Also, your comment is very useful to understanding DSD of rainfall system. I'll modify the sentence more explicitly.

HESSD

12, C2442–C2449, 2015

2) Page 4011 line 21: “(vi) The DSD tends to be overestimated when D_m < 0.5 mm.” This line is unclear and should be rephrased.

A. Thank you for your comment. Actually, this is typing error. Originally, the threshold is D_0 < 0.5 mm not D_m < 0.5 mm. Therefore, I will modify the word as soon as possible. Thank you for your comment.

3) Page 4014-4015, lines 23-3 “The value of mu : : : in the negative mu range”. These sentences are not very clear and should be rephrased. From what I understand, the authors state that in the current work, the values of mu are generally smaller for convective rainfall, which is in contrast to other previous studies. The authors note that a larger value of mu for convective precipitation is to be expected, because of the fact that due to break-up mechanisms the number of larger drops decreases, while the number of larger drops decreases. I only partially agree with the authors on this. A decrease of the number of larger drop would indicate an increase in the slope parameter of the gamma distribution (which is the combined value -(4+mu)/D_m value). Furthermore, mu generally tends to be heavily influenced by the number of small droplets. For South Korea these apparently observed quite frequent for convective precipitation and result in small values of mu (mu<0). This is in contrast with other maritime studies where the number of small droplets decrease due to below cloud evaporation.

A. Thank you for your comment. I agree to your idea. In present study, however, we used normalized gamma DSD. Therefore, I could not use the slope parameter. Also, what I want to say is the dramatic result of distribution of shape parameter for a convective rainfall type. Already as you told, shape parameter is heavily influenced by the number of small droplets. It is also the result from the break mechanism of relatively larger raindrops for convective rainfall in my point.

4) Page 4015, lines 10-12 “Generally, stratiform : : : of raindrops.” See previous comment 1 above.

A. Thank you. I will add the specific word like as ‘for a same rainfall rate’.

HESSD

12, C2442–C2449, 2015

5) Page 4015, line 23 “The PDF : : : (Fig. 4d and e)” This is to be expected since LWC is related to the third moment of the DSD while R is related to about the 3.67 moment of the DSD.

A. Thank you. Your idea absolutely right. Also, my explanation a little bit short in present paper. To avoid confusion, I will add the specific word as you told.

6) Page 4016, lines 12 “The method : : : in Busan.” Since the work of Gamache and Houze Jr. (1982) many different type of algorithms have been developed to identify convective precipitation in a radar image. In e.g. the work by Steiner et al. (1995) a step wise approach was taken where all precipitation with Z>42 dBZ is identified as convective, while for lower reflectivity values, horizontal variabilities are taken into account. As such, the statement by the authors is a bit dated and can be removed.

A. Thank you for your detailed comment. The reason I explained the convective rainfall threshold of other research result is that rainfall type threshold is depend on the location. Also, I did not consider recent research papers as you told. If I have a chance to change the main sentence of present paper, I will modify and add the recent research results related in it.

7) Page 4017, lines 3-4 “Based on : : : (Table 2).”: It is not clear to me why a comparison is made to observed DSD in Finland. Such a motivation is also not provided in the Introduction or Section 2. In case the authors wish to provide comparisons to other locations, it would be nice if the authors could add a number of extra sites for which these parameters were estimated.

A. Thank you for your point. The purpose of comparison with Finland is to find the characteristics of DSD between high and middle latitude. Also DSD data in Finland collected during around 5 years which is similar to present study (4 years). Furthermore, analysis method is very similar. Therefore, I was compare with Finland’s results. Anyway, I will add the motivation for comparison with Finland’s results if I have a chance to change the present paper.

HESSD

12, C2442–C2449, 2015

8) Page 4019 line 8 “A larger : : : in DT.” Please motivate this statement. A. Thank you for your comment.

9) Page 4019 lines 19-21: “In accordance : : : convective system.” For me this statement is quite counter intuitive, since convective events usually occur during daytime under influence of boundary layer processes influence by solar energy flux. It would be nice if the authors could elaborate on the type of processes that result in convective precipitation at night time.

A. Thank you for your comment and I’m so sorry to make you confusing. Actually, the purpose of the sentence is not convective rainfall type but continental rainfall type. Continental and maritime rainfall are originated by convective rainfall type. In short, continental rainfall was dominant during nighttime (NT) compared to daytime (DT) because of land breeze.

10) Page 4019, line 28 “In the : : : are similar.” See comment 5) above.

A. Thank you. Also, my explanation a little bit short in present paper same as review No. 5).

11) Fig. 10: In order to improve comparison between panel a) and b). Please add both histograms into one figure with summer and winter bars next to each other.

A. It is very good opinion. However, the reason why I divided to plot images is to show the differences between daytime and nighttime for each season. Also, your idea is good for me. I will consider it.

12) Fig. 11. See previous comment 11.

A. Thank you for your comment. I will consider it like as review No. 11.

13) Please combine Fig. 12 and 13 into one figure to improve comparison.

A. Thank you for your comment. I will consider it.

HESSD

12, C2442–C2449, 2015

14) Please combine Fig. 14 and 15 in a joint 4 panel figure to improve comparison.

A. Thank you for your comment. I will consider it like as review No. 13.

15) In section 3.3.2 the authors show differences in the DSD characteristics between summer and winter, and between daytime and night time. These results show that there is both an impact of season and of the period of the day on the DSD. However, the authors do not provide any additional information on the meteorological characteristics that cause these differences. As such, the authors currently only present the results and fail to provide any in depth understanding/hypotheses. I would suggest to add an extra discussion section that addresses this issue.

A. Thank you for your comment. I absolutely understand and agree with your idea. I also thought that there is another reason and hypothesis for a diurnal variation of DSD. As considering your comment, I will analyze more deeply about other reason of characteristics of DSD between daytime and nighttime if I have a chance.

Minor concerns:

1) Please remove “within a topographically complex area” from the title as this paper does not deal with that. Instead just state “as observed in South Korea.”

A. Thank you for your point. I did not explain the reason why Busan region is topographically complex. Busan has so many mountains in there and includes ocean. Also, the area of the ocean is relatively small and Busan is very close to Japan. Considering these features, Busan city is able to say like as ‘topographically complex area’ in my opinion. However, your opinion also reasonable. I will consider it.

2) Page 4010, line 6 should be: “the ratio between the standard deviation (SD) of D_m and D_m.

A. This is my mistake. Thank you for your comment. I will modify if I could change the main sentence. Thank you.

HESSD

12, C2442–C2449, 2015

3) Page 4010, eq. 9: rho_w should be v(D)

A. Your opinion is absolutely right. This is typing error. I will modify this word as soon as possible. Thank you.

4) Fig. 2: Please add an x- and y-axis or some kind of distance scale to the lower panel

A. Thank you for your comment. Because I already show the tick of x and y axis for a upper panel (a), I did not show the tick of x and y axis for a lower panel. There is no problem to understand of picture as I think.

5) Fig 4: Legend should be “(h) A_h and (i) K_dp”

A. Thank you for your point. The position of individual image is changed between K_dp and A_h. So I will change the position as soon as possible. Thank you.

6) Table 3: Change “Entire rainfall” with “All rainfall”. Does this table provide rainfall rate (as mentioned in the legend) or do the number show the total number of minutes observed?

A. Thank you for your comment and I am so sorry to make you confusing. I will change the word from ‘Entire rainfall’ to ‘All rainfall’ in upper part.

7) Fig 5 change “with respect to stratiform rainfall” into “for stratiform rainfall”. Furthermore, what does the dashed line in this figure indicate? For panel b) would be nice if the authors could zoom in on the D_m 1-2 mm range.

A. Thank you for your comment. As I understand, dashed line is the broken grey line shown in Fig. 5 a-b that explained in the caption of Fig. 5. Also, it is good question to zoom in on the D_m 1-2 mm range. However, I want to show the result of dominant characteristics of DSD. Therefore, I explained just two part like as heavy rainfall type and the others in present paper.

8) Table 4 please rephrase “Type” for “Period”

HESSD

12, C2442–C2449, 2015

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

References:

Hazenberg P., N. Yu, B. Boudevillain, G. Delrieu and R Uijlenhoet, 2011, Scaling of raindrop size distributions and classification of radar reflectivity–rain rate relations in intense Mediterranean precipitation, J. Hydrol., 402, 179-192. Steiner M., R. A. Houze Jr. and S. E. Yuter, 1995, Climatological characterization of three-dimensional storm structure from operational radar and raingauge data, J. Appl. Meteorol., 34, 1978-2007

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