

Interactive comment on “Statistical characteristics of raindrop size distribution during rainy seasons in Beijing urban area and implications for radar rainfall estimation” by Yu Ma et al.

Anonymous Referee #2

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General Comments

In summary, this study analyzed the statistical characteristics of raindrop size distribution (DSD) during rainy seasons (May-October) in Beijing based on a 5-year observation (2014-2018) from a Parsivel2 disdrometer deployed at Tsinghua University, compared the differences in diameter and concentration between rain types, rainfall intensity, urban heat island (UHI) stages and months, and finally explored its implications for two types of radar rainfall estimations. The manuscript is overall detailed and well written with analysis of DSD parameters and suggestions for precipitation forecast, while it has some minor problems and lacks further explanation of precipitation micro-

C1

physics. Therefore, I suggest a minor revision and encourage the authors to improve this manuscript. Detailed suggestions are listed below. As I'm not working on this specific researching area, some suggestions may not be suitable for this manuscript, and the authors can decide whether or not to accept them.

Major Comments

(1) I've noticed the authors actually show their results together with discussions in Section 3 and 4, while I personally prefer an independent Discussion Section to clarify the differences and significance of this study compared to others on DSD characteristics in Beijing (and other cities). For example, the authors derived an opposed conclusion referred to Wen and Zhang's work (P7 L10), and it would be better if the authors mark their observation locations in Fig. 1(b), explain the differences in physical mechanism and show detailed possible causes.

(2) Abstract Section. I suggest the authors should first clarify the meanings before using symbols or abbreviations such as D_m and I_{gNw} when showing results in Abstract Section. In addition, although P4 L15 defined N_w as “normalized intercept parameter”, I have not found its clear physical meaning which expected to be similar to N_t , the total number concentration.

(3) P6, L15, the authors use specific mean and standard derivation values of rain rate (R) as thresholds to separate convective rain from stratiform rain. However, it seems that R is only related to D spectra considering equation (10) and (3), so in my opinion this classification method is equivalent to solving nonlinear equations and will probably cause the “clear boundary” in DSD characteristics between rain types mentioned in Abstract Section. The authors should pay attention to the classification method chosen in this study, and it would be better if they obtain more information on rain types from other data sources.

(4) There is a mistake in Table 5. The correct UHI stage labels in the table should be UHI D, W UHI, UHI U and S UHI, which is consistent with Figure 9 and 10 indicating

C2

UHI W stage has the largest mean concentration and lowest Dm.

(5) Figure 13. This figure may mislead the readers as the study focused mainly on low rain rate values (less than 25 mm/h). I suggest the authors should plot it on double logarithmic coordinates, which will make it a linear relationship (i.e. convert $Z=238R^{1.57}$ to $\lg Z=1.57\lg R+\lg 238$). Besides, the derived line for total rainfall are below both convective and stratiform lines for low rain rate values, and the authors should explain this.

(6) Section 4.1 and 4.2. How did the authors figure out the relationship equations (14)-(17)? In my opinion, it is more likely that the uncertainty in parameter values, other than suitability of algorithms, may be the main sources of normalized absolute error (NMAE).

Minor Comments

(7) P2, L19-26. These sentences are weird to read with duplicate words such as “high spatial and temporal variabilities”. I guess the authors here wanted to elaborate the complexity of measuring and modeling precipitation in Beijing due to its high urbanization (i.e. densely populated) and large heterogeneity (i.e. high spatial and temporal variabilities), and show the significance of analyzing DSD characteristics which could help us to understand urban precipitation. I suggest that the authors should rewrite this part to keep it concise and clear.

(8) P2, L21, “. . . stations network de Vos et al., 2017”, add “by” after “network”. In addition, I prefer a standard usage of references in the text.

(9) P2, L22, “monitoring networks . . . have been applied”, here using “established” may be a better choice.

(10) P2, L34, “warn” -> “warm”.

(11) P3, L5, “methodologies” -> “methods”.

C3

(12) P3, L7. I suggest the word “Section” should be capitalized.

(13) P3, L15-17 and L25. From the manuscript, I guess these 32 non-uniform bins are set by THUD and fixed for all rainfall events, leaving the maximum observable diameter to be 24.5 mm. However, P5 L20 mentioned that the biggest raindrops ever reported are around 8 mm. The authors should clearly point it out if the latter diameter value can only represent precipitation in Beijing.

(14) P3, L24-25. How to obtain D_j if only the number of raindrops belonging to each bin was recorded? I’ve noticed that the maximum value of D_{max} happened to be 7.5 mm in Table 1, so I guess there should exist a bin ranging from 7 mm to 8 mm, and the authors took its average as corresponding diameter.

(15) P5, L30. How did the authors figure out the relationship between Dm and D0?

(16) P13, L9. I guess the authors missed “(MP-Stratiform)” after “NEXRAD”.

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C4