Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-277-RC1, 2019

Hydrology and Earth System Sciences Discussions

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Interactive comment on "Effect of disdrometer type on rain drop size distribution characterisation: a new dataset for Southeastern Australia" *by* Adrien Guyot et al.

Anonymous Referee #1

Received and published: 10 July 2019

Review of "Effect of disdrometer type on rain drop size distribution characterisation: a new dataset for Southeastern Australia" by Guyot et al.

The article presents a detailed comparison of drop size distribution (DSD) measurements taken by four collocated instruments (by two different manufacturers) located in Australia. Such southern-hemisphere comparison studies are uncommon, especially for the mid-latitudes. The study is clearly organised, well written and presents a thorough analysis. The results are useful and future directions are outlined. Some minor changes are required before the article will be ready for publication: there are occasional grammar errors and spelling mistakes that should be fixed in the next revision.

At times more references should be provided (I have indicated below when this is the case). In a few cases, the statements made in the text were not supported by the figures, and these require clarification. It is significant that the DSD database collected by the authors is freely available for use.

Specific comments follow:

1. Lines 70–72: This section is rather light on references; please include some of the pioneering studies about microstructure and its effect on QPE. I would think scattering properties, being instantaneous, depend more on microstructure than microphysics as such.
2. Line 73: The DSD describes microstructure, not microphysics (unless changes in the DSD are studied over time).
3. Line 76: References should be provided for stain and oil immersion techniques.
4. Line 87: The reference Thurai et al. 2017 seems to be missing from the references list.
5. Line 109: A reference should be provided for the 20 years of observations near Darwin.
6. Line 140: Please also specify how far apart the two instrument types were and their relative orientation.
7. Line 160: N_t is usually defined as the particle or drop concentration and given in m^{-3}. Is this N_t different? It is also listed as unitless in Table 1.
8. Line 189: Should "min" be "minute"?

9. Table 1: Please double-check the units for PSVD (decibel seems an odd choice) and the symbol used for rainfall amount (amount is not a summation of hourly rain rate).
10. Line 201: The range around the expected velocity should be 60% to match the reference and Figure 6.
11. Line 214: Please show how A_i was calculated; since depending on the Parsivel version used, the formula used to calculate A_i differs (whether or not D or $D/2$ is used should depend on whether or not the Parsivel automatically removes drops detected in the edge region).
12. Line 217 and Eq. 2: This version of Z is in mm^6 m^{-3}, not dBZ.
13. Line 224: Is this canting angle the standard deviation of an angle distribution?
14. Line 228: "attentuations" $\rightarrow$ "specific attenuations".
15. Line 238: Λ has unit of mm^{-1}. Which fitting method was used to find the ordinary gamma model parameters?
16. Line 266: By my reading the Parsivels showed more than 100 mm difference in cumulated amount.
17. Line 268: This is first mention of a second tipping bucket 9 km away; it should be introduced alongside the first gauge in Section 2.5.
18. Table 2: What does "Equivalent" mean in this instance? I assume the cumulated amounts are for all rainy minutes per instrument, not over the 40062 common time steps?
19. Line 290: "can measure smaller diameter drops and include a 0.125 to 0.25 mm bin size than OTT" – sentence doesn't make sense.

20. Line 300: What explanation is there for the differences being greatest at the ends of the spectrum? I would guess sensitivity differences for the Thies differences for small drops, and sampling effects for the large drops.

21. Figure 3: why do no Parsivels record any drops larger than 6 mm? (Were they removed, or were there simply no recorded drops by the OTTs?). This is also strange given that in the example event in the next section, OTTs record larger numbers of larger drops.

22. Line 308: It would be useful to include the time (or at least month/season) of the event.

23. Line 329: Please include a reference for KDE. KDE is used to estimate probability distributions of observed variables, not to estimate the DSD parameters for each minute as written here.

24. Figure 4: What are the lines in the density distribution plots? Please also label the plots a) to g) to match the caption.

25. Figure 5: D_{max} and D_0 should be defined in the text. The "spiky" density estimated for D_{max} is presumably due to the discrete diameter classes used and would disappear if different KDE bandwidth were used. Incidently, is D_{max} here calculated on the shared classes? If not I would expect the densities to differ by instrument just because of the different class definitions.

26. Line 345: μ_0 and μ are used interchangeably here. It would be of interest to show the mean DSD per instrument (ie mean/median and bars for quantiles on $N(D)$ by D class) to empirically show the differences in shape.

27. Lines 357–358: "one of the Thies LPM instruments (T3) measured a very large number of particles falling into these two categories" – I do not see this very large

number of particles in Figure 6, which shows more particles outside the expected velocity ranges for OTT1 and OTT3.

28. Figure 7: Please clarify “mean diameter” as meaning DSDs with small drops removed, since “mean diameter” could be taken to mean D_m. The discussion on lines 369–373 is confusing and requires rephrasing – is the point that the Thies instruments seem to have a lot of drops in the first class after 0.6 mm, where as the drops are more evenly distributed in the OTT cases?

29. Lines 374–375 and Figure 7: I interpret the plot for R in Figure 7 as showing that without the small drops there are more very low rain rates, since for example the left tail on the solid red line is left of the blue and green lines; this appears to clash with the statements made in the article text. In the OTT distributions and the T1 > 0.6 distribution there are rain rate values less than 0.1 mm h^{-1}, which was stated to be the minimum allowed in these analyses. What explains these low values?

30. Lines 391–392: The statement here (that differences between instruments are larger as R increases) is not supported by Figure 4, in which there is not a clear effect on the differences that correlates with the peaks in R.

31. Lines 397–398: The statements here are not supported by the plots in Figure 8. OTT1 shows similar frequencies to OTT3 for for high reflectivity, attenuation, and R. The big difference is that in the OTT1 distributions there are more low values and less frequent mid-range values than in OTT3 distributions.

32. Line 398: Which variables are meant by “first order” moments here? Since high rain rates mean many drops of all sizes, the stated link between high rain rates and large-drop sampling uncertainty requires some more argument, e.g. by looking specifically at the variables influenced more by large drop occurrences (D_m, Z). I think that sampling uncertainty due to sample numbers decreasing with

increasing rain rates may play a much larger role than the large drops in the observed differences (e.g. there are only 129 points in Figure 8, but 29815 in Figure A1).

33. Lines 415–419: It is not clear why it is important to separate D_m at 0.6 mm, or which previous bimodal distributions the authors are referencing. A reference to the scheme used to separate convective from stratiform regimes should be included here.

34. Lines 428–430: The statement that b decreases with increasing D_m is not true when comparing the fits on the two stratiform data sets, and because all data contains the convective data it is hard to compare the results for "all" to the convective results.

35. Line 444: While true that this paper showed Thies can capture small drops related to drizzle, no estimate of the measurements' accuracy can be made without another instrument that also captures those drop sizes.

36. Table 4: $\log \rightarrow \log_{10}$, and mm^6 mm^{-3} $\rightarrow$ mm^6 m^{-3}. Over what range of Z_H values were these differences calculated; i.e. for each value in the left-most column, what was the class size in dBZ? Which instrument was taken as the reference, i.e. the percentage is of which value of R?

37. Figure 9: The logarithms should be specified as $\log_{10}$ in the axis labels. Some brief discussion in the main text about the differences between the fitted relationships and the normal Marshall-Palmer Z-R relation should be included. Also, the b exponent in the Marshall-Palmer version is 1.6 not 1.5 as stated in the plot key; please include a reference to Marshall 1955 in the caption for this plot and clarify which relationship is shown in the plot.

38. Figure 11 caption: "augmented by numbers of authors since" – which authors? Please provide references.

39. Line 544: Another possible future direction could be comparisons of Thies LPM to other instruments that, unlike Parsivel, are able to accurately measure concentrations of small drops.

40. Line 545: The reference to Raupach et al. 2019 has year 2019 in the references list but 2018 in the text.

41. Lines 525–535: The appearance and discussion of Figure 11 do not fit well into this paragraph, which is ostensibly about a lack of DSD observations in Australia. Are the authors aiming to highlight the mismatch between their observations and the climate regimes shown in Figure 11? This discussion feels incomplete.

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