Review of AMT-2022-209

General comments

This study shows a thorough analysis of DSD measurements from three co-located disdrometers of different types, located in Gadanki, India, during the landfall of cyclone NIVAR in November 2020. The spectra and values of key rainfall parameters are compared between the disdrometers, for different storm regions and wind speeds. The results provide a useful comparison between these three disdrometer types, and a novel aspect here is that the rainfall is not "typical" because NIVAR was a tropical cyclone.

While the comparison is thorough, it is disappointing that there is not more analysis of what the results mean in terms of the properties of the cyclone rainfall that has been sampled. The rich data source explored here would make for a useful comparison of the rainfall properties that are experienced in the different parts of the tropical cyclone - for example which parts are influenced most heavily by the drop concentration and which are influenced most heavily by drop size. Particularly concerning Figures 10, 11 and 12, the results are simply stated without physical explanations. A proper discussion of the results and their physical meanings, with references to the literature on tropical cyclone rain properties, is required. I have listed other recommendations below.

General comments

1. There are some grammatical errors which can affect the readability of the manuscript at times. These errors are often to do with comparisons: for example, on line 6: "high" should read "higher"; on line 7 "large" should read "larger", and so on throughout the paper. On line 61, the artefacts and errors themselves are not essential but taking them into account is essential. The use of tense in Section 3 is inconsistent. These errors are generally minor in nature and a thorough edit will fix them.
2. The introduction should include an introduction to what the raindrop size distribution is and its importance (to e.g. remote sensing and numerical weather prediction).
3. The results need to be put into more context with other studies. For example, the authors have found that different parts of the cyclone produced very different Z-R relationships. What have other authors found for cyclone Z-Rs and how do they compare to these results?

This lack of discussion extends to the other results and their physical meanings in terms of rainfall in tropical cyclones.

Line-by-line comments

1. Line 24: "Convective processes and resulting rainfall in a TC are primarily governed by the evolution of the microphysics of a TC." This statement needs a reference.
2. Line 35: On underestimation of small raindrops by disdrometers, Thurai et al. (2017) also reported on this underestimation and Raupach et al. (2019) proposed a possible solution.
3. Line 70: For the laser disdrometers was any filtering on fall velocity by drop size done, as in e.g. Jaffrain and Berne (2011)?
4. Line 75: i must be the diameter interval number, not the number of intervals.
5. Equation 2: What do the numbers 4600 and 1000 in this fraction
6. Line 80: A reference for the LPM should be included. represent? They do not align with the given laser dimensions.
7. Line 93: Units should be provided for $v(j)$.
8. Line 97: A reference for the PARSIVEL disdrometer should be provided.
9. Line 100: In Equation 5, $D/2$ is often used (as stated here) and yet newer PARSIVEL disdrometers automatically remove any raindrop that touches the edge of the laser area; in this case the effective sampling area should be calculated using D instead of $D/2$. The authors should check which is used in this case.
10. Equation 6: $v(j)$ should be properly defined here to show that it refers to the jth PARSIVEL velocity class.
11. Line 104: The locations (ie coordinates) of the disdrometers should be given, as well as their altitudes and the situation in which they are installed (e.g. open field, building roof, etc).
12. Lines 106-109: How are these thresholds decided; were they based on previous studies?
13. Line 111: It should be noted in the paper that this 6th-DSD-moment Z is reflectivity in the Rayleigh regime, whereas the T-matrix calculations used later in the paper are in the Mie regime.
14. Line 111: D_m should be labelled here as mass-weighted mean diameter.
15. Equation 11: I think the π^4 in this equation should be π^5; please double check.
16. Lines 140-141: The authors should reference attenuation-correction studies that use this technique here.

17. Equations 11-16: λ, K should be defined with units and the meanings of *Re* and *Im* should be written out.

18. Equations 17 and 18: γ_{DP} and γ_H require definitions, and these equations require better explanation.

19. Figure 1: axis labels are missing; the Dvorak classification requires a reference on line 160; it should be stated what time interval is represented between each point that is plotted.

20. Line 164: This statement about the eyewall requires a reference.

21. Figure 2: Axes are missing labels, and the caption should state that the black solid lines show the inner/outer boundaries.

22. Line 170: What type of rain gauges were used and how close were they to the disdrometers?

23. Figure 3: It is important that the caption states the time resolution of the measurements shown here, since rain rate depends on resolution.

24. Line 174: The maximum D_m is 2.5 mm – why do the authors discount the LPM measurement?

25. Figure 4: The 1-minute resolution should also be mentioned in the caption for this figure.

26. Line 184: Given other studies (e.g. Thurai et al. (2017)), it is possible that the PARSIVEL has underestimated the number of small drops rather than the LPM overestimating the numbers of drops.

27. Line 188: The authors mention corrections based on theoretical fall velocity, yet no corrections are mentioned in Section 2.

28. Figure 5: Are these linear fits statistically significant? The authors should show significance information and discuss.

29. Line 206: What method is used to fit the Z-R relations? If a linear relationship in log space is used it needs to be stated to distinguish the method from other methods that fit power laws specifically. The caption mentions a power-law fit but not which method was used.

30. Line 213: It's not clear here why vertical wind speed near the surface is insignificant – I would think that vertical wind strong enough to loft 4 mm drops is easily obtained both aloft and near the surface in convective storms.

31. Line 220: Exactly how many data points with wind over 4 m s^{-1} were observed? From Figure 1 it appears that this number cannot be insignificant since there are large areas where the five-minute averaged wind speed was in the 5-8 m s^{-1} range. Given that the event in question is a cyclone it seems reasonable that there may have been some strong winds that could skew the statistics for a ¿ 2 m s^{-1} wind speed category. The authors should discuss this point and justify the categories used.

32. Line 222: It would be helpful to briefly explain the DSD classification used here.

33. Line 226: Rainfall variability is high enough that even "co-located" instruments metres apart sample different rainfall properties, so not all differences can be put down to instrument error or measurement principle.

34. Figure 6: It is possible that extreme values skew the mean DSDs shown here. The authors should test whether the median DSDs are very different - if they are, then showing the median DSDs may be more representative of the "characteristic" DSD.

35. Line 230: Again I wonder whether what the authors call an "overestimation" by the LPM is actually an underestimation by PARSIVEL and JWD?

36. Line 234: The different properties of JWD underestimation in different storm regions makes me wonder whether the physical set-up of the instrument could play a role - i.e. if there is a nearby building wind direction could make a difference.

37. Line 233: The JWD also records more large drops than the other instruments in the inner rainband for low rain rates.

38. Lines 240-245: It is unclear here how the authors are judging whether a difference in slope between the different plots is significant or not, and this should be stated. For example the difference between JWD lines in the eyewall is only slightly larger than differences in the other storm regions. The authors should also use language that acknowledges the uncertainty – ie use "little difference" instead of "no difference" when the differences are not significant, and "similar" instead of "same" distribution, since there are still differences present.

39. Line 252: Do the authors mean at wind speed > 2 m s^{-1} instead of $R > 2$ mm h^{-1}?

40. Lines 255-265: The repetitive nature of the results shown here makes this section difficult to read and it is unclear whether the authors are referring to Figure 7 or Figure 6 or to both figures. The discussion could be made more concise.

41. Lines 259-261: This sentence reports on all wind speeds but also wind speeds less than 2 m s^{-1} which does not make sense.

42. Line 263: The claim that LPM shows smaller D_m than PARSIVEL in the outer rain band is not supported by Figure 7.

43. Line 267: $10 \log_{10}^{N_w}$ should be written $10 \log_{10} N_w$.

44. Line 269: The statement that "In general, N_w increases with increasing R" is not supported by Figure 8. Do the authors mean that they expect N_w to increase with R, given previous work?

45. Line 272: "N_w is smaller in the eyewall while larger in the inner and outer rainbands than at lower wind speeds" - this statement contradicts Figure 8 which shows that N_w is generally larger in the eyewall than in other storm regions. The authors meaning here is unclear.

46. Figure 8: The statistical significance of the linear fits should be discussed. It may well be that some of these fits are not significant enough to show an increase or decrease of N_w with

R, since by eye the slopes often look close to zero.

47. Line 277: "This could be due to the presence of more large drops in LPM than PARSIVEL" – I think LPMs larger N_w values are more likely owing to the much larger numbers of small drops recorded by LPM compared to the other two instruments.

48. Line 280: The last two sentences here are unclear (to which figure are the authors referring? Where is the 10^3 number from? Which variable is affected by the imbalance the authors mention?).

49. Line 297: I think "increase in wind speed" should be "increase with wind speed" here.

50. Line 328: There are differences in the PARSIVEL and LPM values in the eyewall that are not discussed in the text.

51. Line 331: Earlier studies are mentioned but not referenced – the authors should cite them here.

References

Jaffrain, J., and A. Berne, 2011: Experimental quantification of the sampling uncertainty associated with measurements from PARSIVEL disdrometers. **12 (3)**, 352 – 370, doi:10.1175/2010JHM1244.1.

Raupach, T. H., M. Thurai, V. N. Bringi, and A. Berne, 2019: Reconstructing the drizzle mode of the raindrop size distribution using double-moment normalization. *J Appl Meteorol*, **58 (1)**, 145–164, doi:10.1175/jamc-d-18-0156.1.

Thurai, M., P. Gatlin, V. N. Bringi, W. Petersen, P. Kennedy, B. Notaroš, and L. Carey, 2017: Toward completing the raindrop size spectrum: Case studies involving 2D-video disdrometer, droplet spectrometer, and polarimetric radar measurements. *J Appl Meteorol*, **56 (4)**, 877–896, doi:10.1175/jamc-d-16-0304.1.