

Interactive comment on “Statistical characteristics of raindrop size distribution over Western Ghats of India: wet versus dry spells of Indian Summer Monsoon” by Uriya Veerendra Murali Krishna et al.

Anonymous Referee #1

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Major comments: This study presents an overall analysis of the rain droplet size distribution (DSD) characteristics over the Western Ghats (WG), India. The authors contrast measurements during the relatively dry and wet periods, as well as between stratiform and convective clouds. Additionally, they propose new μ - Λ relations for the region within a context provided by previous studies. Overall, the results presented carry significance since the DSD characteristics throughout different climatic/cloud type, regimes are determinant for accurate cloud-resolving modelling. However, the manuscript requires further corrections, less Figures and better discussions. Here I provide the major issues followed by specific comments in the next section. In Section 4.1 the authors provide an analysis of the orography effect on the DSD measurements

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using GPM. While I think this is an interesting study, it is not clear what motivated it and what were the conclusions. The authors showed that D_m presents a higher variability on the leeward side, which was due to the presence of deeper clouds. The deep convection in the leeside is well known and published in several papers. But it is not clear if the authors claim this is due to the orography effect, due to larger rain rate during wet spells or if there is something else. Additionally, the GPM microphysical retrievals have large errors and the readers has no information if the errors are larger than the differences or not. In addition, it is not mentioned how the authors expect the orography to affect the overall conclusions of the manuscript. For sure it is important to discuss the effect of the topography because the data was collected at the leeside, but the results presented are incomplete and has no connection with the conclusions. Going through Sections 4.2-4.5, I noticed a significant amount of repetition in the analysis. While the authors are looking at different things in those sections, the overall conclusions are fairly similar – that rain droplets tend to be smaller in the dry spells as compared to the wet spells (which is not surprising). This is repeated in each section as though they exist in isolation. There does not seem to be a logical thread motivating the reader to go through the figures and sections in order. This is representative of an issue present in the whole manuscript, which is the lack of synthetization. There is a lot of text dedicated to describing the figures, while only a small portion is dedicated to discussing the physical reasoning. I believe the authors could do a better job of describing what they are looking for in their dataset and possibly even compile Sections 4.2-4.5 into a single section. However, the main problem in this Sections are related to the bias in the results due to the different rain rate from dry and wet spells. In the last part of the manuscript the authors do a nice job, organizing the results by rain rate and cloud type. In the last part you can conclude some important issues, however, in this Sections we only see the signal of different rainfall. I suggest the authors to cut this part and add a figure showing the rain rate as function of the diurnal cycle, dry/wet spells, cloud type. These will be much more important. Another major issue regards the authors understanding of the microphysical processes explaining the DSD character-

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istics. The most serious problem is a small comment starting in line 261. The authors claim that condensational growth can partly explain the presence of small raindrops, which is wrong based on the accepted theory of droplet growth and past observations. The JWD measurements provided in the manuscript deals with droplets larger than 0.3 mm, which is a size range where the condensation is negligible. While this was only a small comment, it highlighted some misconceptions about the authors understanding of droplet growth. Granted, it is very difficult to discuss microphysical processes looking only at ground-based measurements since the DSDs are the result of multiple competing mechanisms. The ideal scenario would be to provide a more interesting analysis about the measurements themselves instead of only inferring microphysical processes from the DSD measurements. This is the reason I think Section 4.6 is the strongest part of the manuscript, which introduces a contextualized analysis of the DSD parameters through the μ - Λ relations. Another important point is the no evaluation of the CCN in each situation, the authors remember in the middle of the manuscript to talk about aerosol, however, the discussion and results should take into account the aerosol effect in the DSD. In addition, the methodology to adjust a Gamma function each one minute add noise to the results, because many times one-minute distribution is not significant to describe a Gamma function. The authors should at least evaluate if an adjust each 5 minutes presents the same result. I had a lot of problem when I used one minute to adjust a Gamma function. The section describing the synoptic features for dry and wet spells is out of the scope of the paper and several others papers (cited in the manuscript) described the main synoptic differences between dry and wet spells. In addition, most of the characteristics seems to be a consequence of dry/wet conditions, and not a synoptic difference responsible for a dry/wet period, for instance, temperature is naturally higher when you have less rainfall. Furthermore, as there are differences among leeside and windward and the resolution is 0.5 degrees it can mix different situations. The conclusions of the manuscript are rather descriptive as well. It is not very clear what were the gains in understanding from this study. However, given the limited setup available, I don't think the authors will be able to make substan-

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tial claims about microphysical processes. I'd rather see a bigger focus on the DSD measurements themselves, as done in Section 4.6. Based on the comments above, I suggest that the authors do major changes to the manuscript before submitting the revised version. The last Sections of the manuscript are the most important the authors can address the same contents using much less figures and be more clear, objective and precise in the study.

2- Specific comments:

L.23-24: "comparison with the earlier studies" -> "Comparison with previous studies".
L.29: the first sentence is awkwardly phrased. Please review – suggestion: "The Western Ghats (WG) is one the two Indian regions subject to heavy rainfall". What is the other region? Should mention as well. L.42: What is "good rainfall"? Could mean multiple things for different readers. Change to "heavy rainfall" or something similar. L.58: "DSD is related to" -> "DSD is related to processes such as". There are other processes affecting the DSD formation (advection, ice melting, droplet breakup to name a few). L.73: How did the authors obtained geopotential height anomalies and OLR using a X-band radar? Should mention that the authors used more than X-band measurements. L.76-77: Use either "a positive geopotential height anomaly" or "positive geopotential height anomalies". Many of these features are consequence and not the driver. L.80: "speculate" -> "infer". L.84-85: What did the Konwar et al. (2014) study concluded? You only mention the shortcoming of the study and not how it motivated your manuscript. Would be nice to provide a few more details on the Harikumar (2016), Das et al. (2017) and Sumesh et al. (2019) studies as well. After summarizing their results, you can point to the knowledge gaps still remaining. L.95-97: do not have to repeat that the microphysical processes can be inferred from the DSDs. L.101: "terrain, WGs" -> "terrain of WGs". L.116: indicate the JWD was placed at the leeside. L.118: "a size" -> "sizes". L.121: on size or concentration? Or both? L.128: "the raindrops" -> "raindrops". L.128-130: how do you know there are no droplet over 5.5 mm if you can't measure them? I suggest removing the last three sentences of this para-

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graph. L.137: I believe the authors estimate R and Z from the JWD measurements and not “JWD estimates rain rate (R) and reflectivity (Z). . .”. L.151: “momentum” -> “moment”. L.166: “lacking to study” -> “difficulties of studying”. L.172: is the 1-degree resolution enough to solve the topography differences? L.187: is the data collected at the leeside? L.191: “the comparison” -> “comparison”. L.191-193: avoid repeating “comparison”. L.206: “time series are” -> “time series is”. L.216: The use of model to define rainfall has some issues, because model does not describe very well the rainfall. Why you did not employed the raingauge employed in this study? L.218: “that dry days are more” -> “that there are more dry days”. L.228: “windward side and leeward side” -> “windward and leeward sides”. L.229: should note that you use GPM for this analysis – in the beginning of the section. L.234-237: the logic here is weird (at least the way it is written). First you mention that GPM underestimates (overestimates) Dm (Nw) and, because of that, GPM can be used for your study. Should be something like “while GPM have X and Y shortcomings (as pointed out by previous studies), it can be used for our objectives with the following caveats”. L.251: what is the conclusion from this section? L.256: How the data showed in Figure 4 was normalized? It is not clear, have you used the total amount for each class of rainfall? Also, this Figure is missing large droplets. Why the authors do not show the rain rate each hour? L.258: The large number. . .it is normalized so I have no idea bout the total number. Fig. 4: put label in the y-axis in panel a). L.261-264: I don’t agree that condensation has any effect on the JWD observations. The lowest size bin of the JWD is 0.3 mm, while condensation is mostly effective below 30-40 μm . Therefore, the “condensational growth of raindrops” is most certainly negligible – unless you provide calculations supporting your claim. L.263: This is a place to discuss the effect of the aerosol. L.265-266: evaporation can be more efficient during the dry spells but is not limited to it. L.271: If evaporation is the reason, why it is similar every time, during the cloud formation it should not be as important as during dissipation phase. L.274: could there be ice melting as well? L.267-268: I don’t think negative Λ values were ever reported in the literature. L.299: this sentence is missing something. It has no relation with the former discussion L.302: At this point

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of the study you have not presented any cloud type discussion and this result could be due to different cloud type. L.308: your scale goes up to 3.0 mm!!!! how can you talk about 4.8 mm? L. 320: The wet spells also have a bimodal feature, actually this is a typical feaure of tropical convection. L.342: I would argue that the shallow convection is mostly due to the weak updrafts and not the collision-coalescence process. L.480: The results now depends from rain rate as already explained. The authors should re-organized the manuscript in order to have an easy flow of useful information. L.520: “remote” -> “remote sensing”. L.580: “small Dm, large Nw” -> “small Dm and large Nw”.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-832>, 2019.

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