FIRST REFEREE MINOR COMMENTS

P8105, lines 9 and 10: amsl or a.m.s.l.
Fixed

P6(for 8106), 10 (for line 10): “This procedure ensures”
Fixed

P6, 12-15: why should 1-min DSDs present no gap? Setting the corresponding drop counts to 0 is not really justified in my view.
This point has been discussed in al details in (Ignaccolo and De Michele 2010) to the satisfaction of both referees of that manuscript. It would be extremely helpful if you could articulate what is not justified and why or how it affects the material presented.

P7, 16 – P8, 10: I do not find this discussion about the “quantization error” useful, if, at the end, you compute p_G (D_R) as the simple average of the normalized spectra (eq. (2)).
We also compute and compare instantaneous spectra which are affected by the quantization error.

P8, 15: 0.64 is the mode of the Darwin normalized DSD. Why do you use the same skewness classes for the other sites for which the modes are different (greater)?
Here the value 0.64 is used not because is the most probable value for the BBY or CZD data set but to generate the same skewness classes as in the first manuscript so that we can compare results for different skewness classes at different sites. The text was modified accordingly.

P8, 22-24: this property is simply mentioned in the companion paper, not demonstrated, so this reference is not useful
<u>As we already mentioned in our reply to your comment relative to the first manuscript</u>, Eq. (9) of the first manuscript demonstrates the mentioned property.

P8,20- P9,14: It is strange to go back to the “not renormalized” spectra
Why? Please notice that skewness, kurtosis and all the centralized moment are preserved by the renormalization procedure (Eq. 9 of first manuscript). Therefore the skewness of the renormalized spectra is the same of that of the not renormalized spectra.

P9,14: I would have expected “the flatter” instead of “the steeper”.
The rationale for steeper not renormalized spectra → flatter renormalized spectra is clearly explained in the manuscript (pg.17 lines 8-14 of the original manuscript).

P10, 2: “strongly peaked”,
this expression doesn’t mean so much; the kurtosis value could tell if the distribution is more or less peaked compared to the Gauss distribution. . .
Yes , Table 1 is the answer to your comment, in the sense that quantify how “peaked” the distribtuon are. We could have used the kurtosis but we did not: what is exactly the problem? Moreover if we used the kurtosis we should differentiate between the kurtosis of the distribution of skewness and the kurtosis of renormalized and not renormalized spectra (they are the same as Eq. 9 of the first manuscript shows) and this could be confusing to the reader.

I don’t understand why you use the DRW skewness classes for the other sites?
Because (as stated in the manuscript) we want to compare results between the different sites DRW, BBY, and CZD, thus we want to use the same skewness classes so that eventual differences are due to the “site” chosen and not to the adoption of a different criteria to define the skewness classes.

P10, 26: ”have to few time intervals”?
Fixed.

P10, section 3.2: in these analyses, you stratify by skewness classes: what is the physical reasoning for doing so? Without a proper argument here, the subsequent analyses are pure mathematical play with little meaning or practical application.
We already answer to this comment for the first manuscript. A probability distribution is determined by all its moments (a quite basic concept in statistical science) as such all moments (the skewness being the third centralized moment) are “physically” significant. Your objection is flawed.

P11, 3-14: There is some subjectivity in determining what is statistically significant or not. . . The mean distributions differ for Dr values greater or equal to 3-4, a spectrum region probably largely affected by sampling issues. . .
The criteria of using a 10/M (with M being the number of sample) as an indicator of statistical accuracy is a pratical thumb rule that is more than sufficient for the purpose of this manuscript. If you consider the value of M (they are reported in the manuscript) you will see that there is no flaw in our statements.

P11, 15-24: this paragraph is in contradiction with the objective of the paper as stated in the title: the skewness is not a good measure of the invariance of the DSD if unable “to capture the BB and NBB discrepancies”
By “skewness as a measure of invariance” we obviously do not mean that “all instantaneous spectra must have the same skewness otherwise the skewness is not useful” as you imply but that the skewness is our “meter stick” to check if two instantaneous spectra are “equal” or not.

P12, 1-2: grouping the s0, S+1 and s+2 classes and ignoring the other spectra is (again) very subjective
Not at all, we did the analysis for each skewness class separately and the results are the same for each skewness class (as we clearly stated in the manuscript). The choice of grouping s0, s1, s2 classes is to make the presentation shorter.

P12, 19-20: apparently, like the skewness, the kurtosis is not able to depict differences between the BB and NBB spectra. If I understand well Fig 3, there is a deterministic relationship between the skewness and the kurtosis. This paragraph is pure mathematical play: I cannot find any practical significance.
Yes you understanding is correct. . The physical significance is quite obvious as it indicates that higher order moments of the drop diameter distribution do not add extra “physical” information. Quite frankly the characterization of a distribution trough its moments/central moments is a quite standard statistical procedure.

P13, section 3.3.2:
you have to come back to the "not renormalized" spectra, a proof of the inability of the proposed normalization procedure at detecting what you want to detect? I find a contradiction between, on the one hand, the fact that the normalized DSD (sorted into skewness classes – this may be the point) are identical for the BB and NBB cases (fig. 6) while, on the other hand, the additional shape parameters you are considering here have very different pdfs (Fig. 4): please clarify.
See reply to you main comments.

P15, 19: using the gradients of the "not renormalized" spectra is in contradiction with the scaling concepts intended at defining a "general distribution" independent of some scaling moments (mean diameter and variance in your study).
See reply to you main comments.

P16, 12: "the drop count Nl is the inverse of the average precipitation rate": waouh! This is new physics!!!!!!!!!!!!!!!!! P18, 3: diameter;
Indeed.

P16, 22: dramatic
Fixed.

SECOND REFEREE MINOR COMMENTS

Intro – Last paragraph
"removing steep timeintervals" what ismeant by'steeptime interval'–this is not clear at all at this stage of the introduction.
The text was modified to clarify this point.

2.2methods
P8108–l15–I don't understand why the authors seem to take for granted a most probable value of skewness at 0.64.What is the rationale behind that statement?
Thank you for this comment. We adjusted the text to clarify this point. The rationale to choose 0.64 is that of using the same skewness classes as the first manuscript.

P8109=l15:the smaller the gradient,the steeper the distribution? You should mention the 'absolute' value of the gradients or steepness of the slope,it would be more clear for the reader.
We prefer to keep the notation unchanged, as we do not consider this point clear.

P 8111–l 10 to12:what I see in Fig2 is that NBB has a fatter right (and not left) tail inS0 to s+2.I don't understand the analysis of this figure made by the authors. This paragraph is pretty unclear.
The fatter right tail is the most evident discrepancy in the log-normal plot adopted. Of course the left tails are not identical since the integral of all distributions must be 1 (due to the adoption of the log-normal plot a small discrepancies in the the tail is more evident than the other differences which

compensate for a fatter tail).

3.3.2
Are the gradients calculated on the raw or normalized spectra? It is not clear from the text which method and why it was chosen.
We modified the text in the introduction and in this section to clarify this point. The choice of parameters reflects what are the expected properties (from previous studies in literature) of orographic DSDs.

P8116–L20onwards.
The analysis of Fig5 on a visual only basis is not very convincing.
The authors should find the equations of the 'separation lines' (the line that produces the max of separation between the red and blue symbols),and check quantitatively the number of 'misclassifed' points; on the left and on the right column. This would be a more objective way to compare the 2 methods.
We think the visual separation is clear enough for the scope and purpose of the manuscript and a fully quantitative would not add much at the present stage other than make the manuscript longer.