Comment on paper ' skewness as a measure of the Invariance of Instantaneous renormalized drop diameter distributions' – part 2 -
By Ignaccolo and De Michele.

In this second part of their study , the authors apply their DSD normalization technique, to 2 additional data sets gathered in California : Bodega Bay and Cazadero. According to the title, the objective of the study, as for part 1, is to use the sorting of DSD by skewness classes to demonstrate the 'invariance' of the instantaneous renormalized DSD.

The additional data sets used in part 2, were chosen as representative of regions where different interactions are expected between land and rain formation processes, compare to Darwin . According to previous studies by Martner et al, cited by the authors the California data set is well suited for studying rainfall from orographic origin.
The authors use the radar signature and divide their data set into Bright Band dominated versus non Bright band dominated cases ; the latter being considered as representative of situation where orographic precipitation processes are predominant.

Comments :

As in part 1, the spectra are classified in skewness classes, in order to demonstrate their 'invariance' .

First, the paper essentially demonstrates (section 3.2 – Fig 2) that the proposed normalization technique fails to provide the expected 'universal' normalized DSD.
What worked in part 1 when comparing stratiform vs convective DSD, in the single site of Darwin, cannot be generalized to another location and when comparing orographic vs non orographic precipitation.

In the rest of the paper (section 3.3 onwards) the authors use different mathematical tools to analyze the differences between the BB/NBB spectra , and the orographic/non orgraphic spectra.
Some of the analysis is applied on the normalized spectra (kurtosis analysis) and some of the analysis is applied on the raw spectra before normalization (gradients etc …).
The objectives and the organization of this second part of the paper are not very clear ; It appears as a juxtaposition of several empirical methods to analyze the spectra ; the physical reasons behind the observed differences are mentioned only superficially. This part of the paper does not fit in the announced scope of the paper/title and seems somehow 'unfinished'.

Altogether the paper is long and wordy, which makes it difficult to read.

I cannot recommand publication of the study in the present form, for the reasons above.

My suggestion : the result presented in section 3.2 (i.e. the normalization method fails to

provide a universal shape that includes orographic/non orogr DSD) could be included in the companion paper part1, and would illustrate the limitation of the method, when applied to other location /other rain type.

Detailed comments :
Intro : Last paragraph 'removing steep time intervals ' : what is meant by 'steep time interval' – this is not clear at all at this stage of the introduction.

2.2 methods
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 ?

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.

P8111 –l10 to 12 : what I see in Fig 2 is that NBB has a fatter right (and not left) tail in S0 to s+2. I don t understand the analysis of this figure made by the authors. This paragraph is pretty unclear.

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.

P8116 –L20 onwards.
The analysis of Fig 5 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.