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Hydrology and Earth System Sciences Discussions

Interactive comment on "Skewness as measure of the invariance of instantaneous renormalized drop diameter distributions 2. Orographic precipitation" *by* M. Ignaccolo and C. De Michele

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Here we report the reply to the main comments and recommendation of Referee 1.

A new scaling method has been proposed by the authors in previous contributions . The method is shown to be useless in the present paper for differentiating orographic and stratiform DSDs.

The scope of the paper, as clearly stated, is to investigate the variability of the (normalized) diameter distribution in presence of the orographic precipitation, this issue is, at our knowledge, not covered in Literature.

In particular we address the following questions: 1) Is there a "most common" distribution for orographic precipitation as in the case of stratiform and convective precipitation? 2) Given a value of skewness are renormalized spectra of orographic precipitation equal to those of stratiform/convective precipitation?

The answer to the first question is Yes. The answer to the second one is "sometimes yes, sometimes no". Orographic precipitation can produce extremely steep distributions which results in renormalized spectra with fatter tail then stratiform/convective ones. These results improve our understanding of the precipitation and its modeling especially in complex terrain.

I find it both unnecessarily complex and insignificant

It is not clear the meaning of the sentence and the overall judgment.
In paragraph 2, we introduce additional parameters as descriptors of the drop size distributions, in paragraph 3 we make the analysis of data and results, and in paragraph 4 we present our conclusions. From the mathematical point of view, we make use of two new equations, and introduce the concept of "Steepness filter". So what do you mean for "unnecessarily complex"? As said in the previous reply, We cannot consider the topic and the investigations "insignificant".

The authors finally use gradients observed in the unscaled spectra, which likely depend on the scaling moments, for making this separation, in contradiction with the basic idea of the scaling approach.

No, there is no contradiction. In principle, one could explore what are the "statistical" connection between the steepness and the other moments. So what? Here,
1) We adopt a renormalization procedure based on the average and the standard

deviation of the drop diameters.

2) We characterize the not normalized distributions using the parameters A,B,C,D,... (steepness, span,..... in our case). The parameters A,B,C,D have a statistical relations whit the moments of the distribution. Of course they do , what is the surprise here? Could it be otherwise?

3) We divide the not normalized distribution in classes according to some observed properties of the parameters A,B,C,D.... (in our case steep vs not-steep).

4) We compare the renormalized spectra of the different classes (e.g. steep and not-steep classes have different renormalized spectra).

Quite frankly the procedure 1) $\rightarrow$ 4) seems to us quite legitimate: which "contradiction" do you refer to?

An additional point. Please note that contrary to your claim the fact that the not normalized and normalized spectra have the same skewness has been demonstrated in the first manuscript and not just mentioned, as you claim. The demonstration is Eq. 9 of the companion manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 8101, 2011.