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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

Anonymous Referee #1

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General comments and recommendation

I carefully read this paper several times and I am sorry to say I find it both unnecessarily complex and insignificant. 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 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.

I cannot recommend publication of this article.

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Interactive Discussion

Discussion Paper

Specific comments

P8105, lines 9 and 10: amsl or a.m.s.l. P6(for 8106), 10 (for line 10): “This procedure ensures” 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. 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)). 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)? P8, 22-24: this property is simply mentioned in the companion paper, not demonstrated, so this reference is not useful P8,20- P9,14: It is strange to go back to the “not renormalized” spectra P9,14: I would have expected “the flatter” instead of “the steeper”. 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... Table 1, Fig. 1: Again, I don’t understand why you use the DRW skewness classes for the other sites? P10, 26: ”have to few time intervals”? 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. 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... 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” P11, 8: significant P11, 14: classes P12, 1-2: grouping the s0, S+1 and s+2 classes and ignoring the other spectra is (again) very subjective 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. 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. 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)... P16, 12: "the drop count Nl is the inverse of the average precipitation rate": waouh! This is new physics!!!!!!!!!!!!!!!!!! P18, 3: diameter; 22: dramatic

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