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Interactive comment on "Imperfect scaling in distributions of radar-derived rainfall fields" by M. J. van den Berg et al.

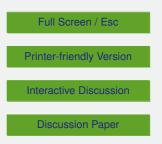**A. Seed (Referee)**

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This paper evaluates a basic assumption that underpins the use of multiplicative cascades to model the spatial distribution of rainfall, namely that the increments at each level in the cascade are independent and identically distributed. A set of radar rainfall fields are used to confirm that the fields have a multifractal behaviour and thereafter the statistical structure of the cascade increments are analysed.

The range of scales that are used in the multifractal analysis is quite small, 0.6 km to 9.6 km, and the analysis is based on a single image. I expected to see an analysis that was based on the entire data set, all 17 storms, and over a larger area under the radar. Multifractal analyses require large data sets so as to demonstrate that on

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average the fields have a multifractal behaviour. A 200 km domain under the radar is entirely reasonable in the summer months and an extra couple of points in the scaling analysis would add credibility to the analysis.

I also missed seeing a power spectrum for the untransformed rainfall fields that is based on the entire data set. The scaling break that is observed in rainfall is typically around 20-30 km in my experience, eg Seed et al 2013, Water Resources Research, 49. The slope of the power spectrum is an important diagnostic in deciding the nature of the scaling and some comments on the implications of the value that is found would be useful. Rainfall is a physical process that has absolute zeroes in the field, there are times when it simply is not raining irrespective of the sensitivity of the radar, and the rain-no rain process is the likely cause of the scaling break, not the sensitivity of the radar or thresholds.

The paper makes the reasonable assumption that the increments in the transformed cascade follow the Levy stable probability distribution, but no evidence is presented that this is actually the case. An analysis based on the entire data set for each of the cascade levels would add value to the paper.

I found that the notation that is used for the probability distributions is unnecessarily complex (Equations 7-10) and the paper would be easier to read if it was simplified.

The captions for the figures are very informal and do not provide adequate information about the figure.

The questions that are posed in Section 1 are not very clear and the conclusion does not reference them. The two questions in my mind are: 1 Are the increments in the cascade IID? The answer to this is that they are not IID and there are dependencies with the scales above and the distribution parameters change with scale. This is a big deal since the fields were transformed using the Laplacian so as to render them conservative, IID in other words, so a comment about the implications of this finding is required. The alternative is to use a multiaffine frame work and use the rainfall fields

directly. 2. If not IID, what is the nature of the dependence? The paper provides a nice framework and analysis for this question.

The subsequent analysis of the temporal behaviour of this scaling behaviour is a big topic and is not really done justice in this paper. I would be inclined to remove it and publish a more thoughtful analysis of the dependence of scaling parameters on the meteorology of the day as a separate paper.

Alan Seed

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11385, 2013.

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