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Hydrology and Earth System Sciences Discussions

Interactive comment on "Spatial moments of catchment rainfall: rainfall spatial organisation, basin morphology, and flood response" *by* D. Zoccatelli et al.

Anonymous Referee #2

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Review of paper HESSD-8-5811-2011 "Spatial moments of catchment rainfall: rainfall spatial organization, basin morphology, and flood response" by Zoccatelli et al.

General comments:

In this paper, Zoccatelli et al. analyse the relationship between rainfall spatial organisation, river network topology and flood response. For this purpose, they define spatial moments of catchment rainfall which describes the spatial rainfall organisation as function of the distance measured along the river network. They also introduce a storm velocity to show the importance of storm movement on the catchment response. The

interest of the defined statistics is illustrated using 5 rainfall-runoff events from various parts or Europe and model results. The topic of the paper is of interest as the proposed statistics can be used to compare different rainfall fields/catchment response in the world, and assess in which case the spatial rainfall variability has a significant impact on the runoff response.

The authors mention that their work builds on Woods and Sivapalan (1999) and Viglione et al. (2010). However, it is difficult to judge how the proposed statistics are similar/different from these previous works, and what is really new in the proposed framework. At the beginning, simplifying hypotheses are stated, but are not discussed enough. Some of them seem to correspond to simplifications of the Viglione et al.(2010) framework (for instance the authors neglect hill slope travel time whereas Viglione at al. (2010) consider it). The assumption of a constant rainfall coefficient is also a strong one. On the other hand, most of these hypotheses are removed when using the distributed hydrological model. So what can be compared between the analytical framework and the model results? In addition, the applications show the interest of the approach, but it is unclear how it could be used in practice: what are the required data for the application of this method, which accuracy is required on rainfall description, on the river network description? What can be expected when calculating those statistics?

The topic of the paper is suitable for publication in HESS. However moderate revision is required to make the paper clearer. The authors should therefore address the above comments, before possible publication in HESS.

Specific comments:

1) p.5813, lines 20-25. The authors propose to “introduce measures to quantify the catchment filtering effect which, as a function of rainfall organization, basin scale and the heterogeneities embedded in the basin geomorphic structure, control the possible extent of the influence of rainfall spatial organisation on the hydrologic response.” To

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what extend is this objective reached in the paper? This point would require further developments in the discussion in relation with the illustration of the practical use of the approach.

2) p.5814, lines 5-10. The authors should better justify/discuss the hypothesis that "Runoff routing through branched channel networks imposes an effective averaging of spatial rainfall excess at equal flow distance, in spite of the inherent spatial variability."

3) p.5815, lines 14-15. The authors say that their work is a generalization of Viglione et al. (2010) approach. But it seems to be more a simplification than a generalization.

4) p.5816, lines 5-10. The hypothesis that hill slope travel time cannot be neglected in the analysis is strong. The authors should discuss it a little more, all the more than hill slope travel time is considered in the model used in section 3.

5) p.5817, Eq.(2). This equation provides a kind of average distance to the catchment outlet, for n=1. As hill slope travel time is neglected, is the calculation performed only on river network grid points?

6) p.5817, lines 12-18: a scheme/figure could help the reader in understanding the physical interpretation of the proposed scaled moments.

7) p.5817 Eq. (5). The definition of the instantaneous ïĄd' and temporally integrated statistics ïĄĎ are similar. Is it possible to derive analytical relationships between both quantities?

8) p.5818, Eq.(6). The introduction of the catchment scale velocity requires further development. What are the rationales behind the definition proposed in Eq. (6)?

9) p.5820, lines 5-8. The sentence is not clear.

10) p.5810 Eq.(9) (but also Eq. (13) and (15). An appendix providing more details on the derivation of these equations would be useful to the reader. In addition, such appendix could permit a better explanation of the links/differences between the rela-

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tionships found in Viglione et al. (2010).

11) p.5821, lines 22-25. These sentence seem to be trivial and evident. I guess that the proposed statistics allow the derivation of more quantitative conclusions. It could be better explained in the paper.

12) p.5822, lines 1-4. The conclusion that the storm velocity is independent of E(Tq), derives directly from the hypotheses stated about the flow velocity. To what extend can this conclusion be generalized?

13) p.5822, lines 15-23. A scheme/figure could be useful to visualize what the authors mean here.

14) p.5823, Eq.(16). The authors introduce a new storm velocity. How does it relates to the one introduced in Eq.(6)?

15) p.5826, lines 13-15. This sentence is not very clear

16) p.5827, line 11-12. Clarify better what you mean with flow celerity and how it is related to the storm velocity?

17) Section 5. The use of the model to assess the relevance of the proposed spatial scale moments is not clearly presented. In particular, the authors should better explain why they retain a model which does not fulfil the hypotheses they have made in the analytical framework (neglecting the hill slope travel time, a constant runoff coefficient, etc..) 18) p.5832, lines 23-26. These sentences are not very clear. How do the authors conciliate their analytical simplified approach and that of the detailed model?

19) p.5833, lines 18-25. How the method could be used for other catchments? Which data are required? With which accuracy?

20) p.5834, lines 5-7. This is an interesting perspective for the use of the method. Could the authors elaborate a little more about this possibility to contribute to “comparative hydrology”

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21) Plates 1 and 2 are very small and difficult to read. In addition, the authors could use the same scale between catchments for easier comparison.

22) Figure 4 and 5: they could be shown with the same scale, so that they can be compared more easily.

References

Viglione, A., Chirico, G. B., Woods, R., and Blöschl, G., 2010. Generalised synthesis of spacetime variability in flood response: An analytical framework, J. Hydrol., 394, 198–212, doi:10.1016/j.jhydrol.2010.05.047. Woods, R. and Sivapalan, M., 1999. A synthesis of space-time variability in storm response: Rainfall, runoff generation and routing, Water Resour. Res., 35(8), 2469–2485.

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