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Hydrology and Earth System Sciences Discussions

Interactive comment on "A stochastic approach for the description of the water balance dynamics in a river basin" *by* S. Manfreda and M. Fiorentino

Anonymous Referee #1

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This paper presents an interesting theoretical derivation that ultimately produces pdfs for the distribution of soil water content and saturated locations in a watershed. The pdfs are derived in a framework that seeks to account for the basin structure through a term that describes variability in soil depth. Overall, I think the approach is quite interesting. However, I think the current incarnation of the paper needs improvement to clarify the objectives, expose the reasoning behind the derivation, and assess of the validity and implications of the final results. More details are given below:

1. I believe the objectives of the paper need clarification. Do the authors seek to account for the role of topography in their derivation or the role of variations in soil depth? I do not understand the specific “gap” that the authors aim to fill by this paper. What specific quantities or pdfs are the planned products of this paper?

2. I believe that the derivation needs improved organization and explanation. In many locations, variables need to be more carefully defined. For example, the meaning of h is implied by the introduction of Equation 1, but it is never explicitly defined. This tendency is repeated throughout the paper and makes the mathematics very difficult to follow. For example, water storage capacity (W) is used as early as line 22 on page 727, but it is finally explicitly defined on line 4 of page 729. Also, is wmt a single variable or two variables? In addition to this stylistic issue, the general path of the derivation is meandering. It is difficult to tell what the overall objective is from the beginning (what is being derived?) and the general strategy that is being used to get there. Can a brief overview be provided at the beginning that gives the objective and general strategy?

3. The reasoning behind the steps of the derivation and physical interpretations are also difficult to determine. Many strategies are used and assumptions are made that are not interpreted physically or justified to the reader, so it is difficult to determine their meaning and implications on the results. (a) What is the purpose of the normalization in Equation 1? (b) What is the reasoning behind the parabolic distribution (a key point in the paper)? (c) On page 728 line 7 the term “uniform distribution” is used, but doesn’t the author mean constant in space? (d) What is the purpose of introducing the “runoff difficulty” and the reference to Gou et al. (2000)? It is confusing because f/F is previously described as being related to soil depth. (e) Why is it assumed that “the soil water content is redistributed within the basin cumulating in the areas with lower soil depth? “ This assumption needs some support. I don’t understand the logic. (f) The distinction between rooting depth and soil depth is awkward in the paper, particular around Equation 4 and relating back to previous equations. (g) The loss equation is assumed to be linear, which is not supported by data (as far as I have seen). The author states that it is a “reasonable approximation,” but provides no evidence to this effect. How is “reasonable” defined in this case? (h) The reason that precipitation is multiplicative or additive noise is not clear based on the derivation shown in this paper. (i) It is not clear how the model is actually representing redistribution of

HESSD

5, S316–S318, 2008

moisture within the basin. Please explain and/or demonstrate. (j) Aside from mathematical convenience, why consider steady-state conditions? Obviously, many previous papers have also taken this approach, so it’s not unprecedented, but it needs to be explicitly justified and related back to the objectives of the paper. (k) At the beginning of the results section, the authors state that “the model provides a realistic description of the basin water balance under a wide range of conditions.” This statement is a conclusion and should be stated after it has been demonstrated in that section. There are similar cases throughout that section.

4. It is a pity that no data from a real watershed are presented in this paper. It is very difficult to know whether the cumulative effect of the assumptions in the derivation has been disastrous or negligible in comparison to reality. The authors state that an experiment has been designed to test this theory, but the comparison is not included here. Without a comparison to data or other means to gain confidence in the model, general conclusions are difficult to achieve. In the end, I’m not sure what has been demonstrated conclusively by this paper.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 723, 2008.