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Comment on hess-2021-470

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Referee comment on "A Time-Varying Distributed Unit Hydrograph considering soil moisture content" by Bin Yi et al., Hydrol. Earth Syst. Sci. Discuss.,
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Review of "A Time-Varying Distributed Unit Hydrograph considering soil moisture content"

Summary: In this paper the authors propose an extension of the distributed unit hydrograph (DUH) method for routing of water in distributed hydrologic models. The default DUB approach takes into consideration the topography of the land surface in computation of surface runoff and river inflow but does not account for spatial variability of the soil moisture capacity within the catchment. The authors present a simple extension of the flow velocity equation which is thought to account for a varying moisture content.

Evaluation: I am not an expert on routing methods, in fact, never taken a course in surface hydrology, but I do know something about math, hydrology, and modeling. The authors presented in this paper are interesting, yet, I believe the paper warrants a major revision before it can be judged to making a significant contribution to hydrology and water resources. Specifically, the paper is not very well written, syntax and grammar need major improvements. I believe this is a first requirement before the paper is ready for detailed review. What is more, the methodology needs a much better physical underpinning citing properly past publications on this topic, presenting relevant units of variables, addressing sensitivity of results to variables such as gamma. Furthermore, the case study / demonstration of the methodology is not particularly convincing. I highlight my main comments below - not in a particular order of importance.

1. The authors should reference previous work on routing/modeling approaches. For example, Line 269, Eq. 10 does not provide a reference, whereas this equation is simply the Pareto distribution function, used by Moore (1985) to describe the spatial variability of the soil moisture storage capacity in the watershed. This is just one example - this comment applies to many equations used by the authors; the continuity equation, Manning's equation, etc.

2. The derivation of the TDUH method needs significant improvement. Not well written - the derivation has been given in other manuscripts so either present it briefly and clearly, otherwise, I recommend the authors refer to the original sources as the present derivation brings up more questions than it answers. I am particularly bothered with the way the equations and variables are presented. It reads as a collection of some equations with some variables. Also, I am left wondering whether the equations presented are derived for the first time by the authors or whether they have been presented in the literature years ago? For example, Eqs. (6), (7), (8) and (9).

3. Authors should give units of variables they use. This will make it easier for readers to digest the material, and for students to reproduce/implement the approach the authors have presented.

4. Line 208-209: Variable "m" is introduced, but is not used in Equation (1) or (2). Introduce variables when they are used and not ahead of time, unless this makes sense to do.

5. Line 261 - 302: The extension the authors propose, essentially applies the ideas of Moore (1985) to the unit hydrograph. Authors should do a much better job connecting what they do to the literature.

6. Equation (15) - is this equation a simple extension of Eq. (14) with " w_{t} " raised to gamma ? or is this equation from Bhattacharya et al. (2012) and/or Bunster et al. (2019)?

7. The authors state that their routing method takes account of soil moisture content, but soil moisture content does not appear in any of the equations. Instead, they use the Pareto distribution function to express the spatial variability of the soil moisture capacity. They assume that this capacity represents temporal variations in soil moisture content. Furthermore, the representative volume of the soil moisture content is unknown. Are we considering the topsoil moisture content, or the moisture content of the first 50 or 100 cm of the profile? I guess I am looking for a better physical underpinning of the presented method.

8. Line 261 - 268: I read this paragraph several times, and it is still not clear to me. What is B_{t} ? Is " A_{t} " the same A as used in the continuity equation? Why not use symbol theta for moisture content? Why do I need to compute the ratio of A and A+B?

9. Why does WM' use the prime symbol?

10. Line 261 - 268: We have A for moisture content, B for the maximum soil moisture storage, W for current soil moisture storage. Why using so many different variables for essentially the same thing? Also, what is the unit of storage and moisture content? Are they similar, or different (as they should be). Is soil moisture storage not simply equal to soil moisture content x depth of profile. Why not use theta for soil moisture content and S for soil moisture storage, where $S = L \cdot \theta$, where L is the depth of the profile?

11. Line 269 - 289: I have a hard time following the different steps of the methodology. I think the authors unnecessarily confuse readers - the methodology can be presented in much easier to understand language - and in a much more coherent style.

12. Equation (10) presents how we compute alpha, but then in a next equation, alpha is a function of time. Please make clear in your entire derivation which variables are constant (scalars), which ones vary as function of time/space (scalars) and, if necessary, which are vectors and/or matrices. WM' varies as function of time? Otherwise alpha is constant.

13. The exponent b of the Pareto distribution function. Is this constant, or varies throughout your watershed?

14. Line 283: Mention that w_t varies between 0 and 1, thus, $w_t \in (0,1]$ (in latex). I assume that w_t cannot be zero as soil can never be entirely be depleted from water.

15. Equation 13: The denominator may need further explanation. Either solve analytically the integral of Eq. (12) and substitute this in Eq. 13, or do the analytic integration explicitly in Eq. 13.

16. The denominator of Eq. 13 - last step - is wrongly formulated. Do we do $1 - (b/(b+1)) \cdot 1 - \alpha_t^{1/b}$ or $1 - (b/(b+1)) \cdot (1 - \alpha_t^{1/b})$; If the first then remove the 1 to get $1 - (b/(b+1)) - \alpha_t^{1/b}$, etc. The present formulation is unclear.

17. Line 296: The variable 'gamma', is this constant for the entire watershed, or varies per sub-catchment or grid cell or ? I recommend that for each parameter the text explains how this parameter is treated, besides its units, a description of what the parameter represents, etc.

18. The authors use an aggregated objective function. Why use a single aggregate objective function? I would advise analyzing the performance metrics separately. The SCE-UA method can do three separate trials - each using a different objective. Then you can compare the results of the proposed method against existing unit hydrograph routing

methods proposed in the literature. If so desired, you can even consider the aggregate objective function - but then as fourth option.

19. What is the definition of flood peak? Need more information to compute the first objective function; are you looking at the peaks of the measured discharge record? and then use the exact same indices of the simulated record to compute the objective function? Or are you getting the indices of the peaks from the simulated record? and then use the corresponding measured values to compute the OF? This difference in implementation may seem insignificant, but can lead to widely different results.

20. Why define the peak error as bias? Why is this preferred over a standard squared residual metric? L2-norm versus L1 norm. Same question for the timing error - and see above comment as well for this 2nd metric.

21. Why not use a metric such as the sum of squared residuals to compare the measured and simulated discharge records of the different routing methods?

22. I doubt that the improvement of the new routing method proposed by the authors is related to incorporating what the authors believe to be soil moisture content. What is key to the performance of the new routing method is what is done to the parameter gamma. The authors articulate what they have done with gamma on Lines 365 - 373. The value of gamma will determine the results of the new method; hence, is the performance improvement related to the gamma parameter, simply as this provides additional flexibility to routing?

23. The different routing methods amount to a model selection problem - and proper techniques such as information criteria or the marginal likelihood should be used to compare the different routing methods. I strongly doubt that what is shown in this paper is the result of soil moisture. The methodology and statistical analysis should be much improved to inspire confidence in this conclusion. As it stands right now, there are many other reasons so as to why the new method outperforms the existing routing methods. One of which is the parameter gamma. If nothing else, the analysis should show the sensitivity of the new routing method to the choice of gamma. The same should be done for parameter k in the existing formulation.

24. Some section names confuse the reader; for example, section 4.2 is labeled "Derivation of TDUH considering time-varying soil moisture content", but the derivation has already been presented. In 4.2, the authors simply apply their method to a case study. Unless "derivation" has another meaning and refers to the computation of the TDUH.

I leave it with this for now as further review will reiterate similar points. I very much

appreciate the efforts of the authors in trying to improve the description of the unit hydrograph for distributed hydrologic modeling. Yet, the present paper needs a major revision before it can be judged to making a significant contribution to the field and warrant publication in HESS/HESS-D. As it stands right now, the methodology needs to be much better embedded into the literature and cite relevant papers when using existing equations, etc. and improve considerably the physical underpinning of the presented method. The authors should also revisit their calibration method - and provide compelling information about the sensitivity of their findings to the choice of gamma and k. Furthermore, the presentation and writing need considerable improvement.

I hope my comments are useful to improve the presentation and description of the methodology, including its application in a case study.