Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2022-192

Anonymous Referee #2

Referee comment on "A translation wave model: Güneycedere case study" by Hülya Çakır and Mustafa Erol Keskin, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-192-RC2, 2022

The presented paper proposes a new flood routing model 'TWM' in which slope variables are neglected in the momentum equation of SV equation. The TWM is compared with kinematic wave model KWM for only one flood event occurred in actual creek reach. Furthermore, another flood event for hypothetical reach was simulated using both TWM and KWM flood routing models.

The paper is unacceptable to be published in its current form in this reputable journal. The research idea is original, which is the only thing good in the paper. The manuscript is written poorly. Presentation of the results is poor and there is no discussion of the results. Paper does not provide scientific soundness. Paper needs to be re-written again.

The most important thing is that the number of studied flood events are very less (only 2) which does not justify the capability of TWM. Number of flood events needs to be increased significantly along with number of cross-sections of channel. Also, TWM needs to be tested in various gradients of slope. The author keeps saying that TWM is good for mild slope but two flood events on mild slope tells nothing. Methodology needs to be clear. The results should be presented in detail with discussion focussing on what factors/reasons are making TWM better and why, also discuss the situations where TWM are underperforming and why.

The paper title should include 1-D.

Introduction part need major revision, What are the previous works done using KVM and why KVM was better for steep slope and is does not simulate correctly the attenuation and dispersion in outflow hydrographs. Why dynamic model according to previous works was not good for varying cross sections and very small slopes. How it can be concluded in introduction that TVM can be suitable for mild slopes, its a new approach, never been

tested. Although it is written it neglects slope variables but neglecting of them is not discussed in introduction. Furthermore, assumption of TVM better performance by ignoring slope without any testing done yet is not justifiable in introduction section.

Line 86-92: slope of the studied area needs to be given as the slope is neglected in the modified SV equation.

Line 109: what is slope gradient of the study and what is the slope gradient of the creeks with which author is comparing from to say that the understudied slope is mild. It is confusing to say relatively mild. Does is make steep is some areas but in most in mild?

Line123: what is meant by good hydrograph shapes?

Please put Fig 1, 2 and 3 together. Make another fig showing the observed inflow and outflow hydrographs that are discussed in section 2.2.

Please give the units of the variables given in the equations.

Section 3.1 please state clearly that author is using 1-D SV equation and derivating it.

Line 177: author should write that term 1-3 in eq 2 representing inertia and pressure terms are neglected for KVM.

Line 199-200: it is not proved yet TWM is suitable for the mild slope, not tested it yet.

Line 261: upstream boundary condition should be from tp to tb ,it can not start from zero.

What is the downstream boundary condition? Is it free flow? If so, please give the numerical scheme also for that.

Line 287-289: did the author divide the reach into segments, if so, does the cross section area variables (bottom width and inverse of side slope) of channel is changing, is it not uniform. Why converting the reach length in segments, please discuss.

Line 305: please tell which version of HEC-HMS was used.

Line 356 Qj+2 is Q3 and so on.

Line 332 to line 361 author discuss the functionality of scheme after the results which is irrelevant and basic as it is obvious by seeing the numerical scheme given in section 3.2 numerical solution. This numerical scheme has been used for decades to solve KWM. The author did not create a new numerical scheme, why discuss it. Author did not discuss the rising or recession limb of hydrographs, please discuss.

Line 363-366 why making the cross-sections from digital topographic map. Did the author divide the reach length in segments and than give bottom width and inverse of side slope for each segment.

The author need to make the figure showing the division of segments in the reach and state clearly what actually he/she did. And, do not discuss division of segments or discussion of scheme in results it should be done before showing the results fig 5.

Line 370-378 Author did not discuss the rising or recession limb of hydrographs, please discuss.

Figure 8 is not discussed-comparison between TWM and KWM.

Section 3.3.2 why the KWM hydrograph is delayed with respect to TWW. Also hydrographs characteristics (peak, rising and recession limb shapes) are different. There is no discussion why it is happening.

Conclusion 4- line 487-491: Inflow peak is closer to KWM peak, KWM is simulated outflow. how it tells the capability of KWM, observed outdlow not compared with KWM. How it is concluded that TWM has acceptable value range and had good shape.

Line 493-496: TWM needs one boundary condition? If downstream boundary condition is open, it is still downstream condition. If it is closed, than does author mean this TWM scheme will not work? Superficial flows does not need downstream boundary condition? What is the relevance of superficial flow, boundary condition and TWM? Every numerical scheme simulating flows needs boundary conditions.