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## Comment on hess-2021-50

Anonymous Referee #1

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Referee comment on "Evaluation of hillslope storage with variable width under temporally varied rainfall recharge" by Ping-Cheng Hsieh and Tzu-Ting Huang, Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-50-RC1>, 2021

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In this manuscript, a new analytical solution to the Boussinesq equation for variable widths and recharge rates is presented and analyzed.

I am in favor of the idea of the paper, and the paper is reasonable well written. However, there are a number of issues:

- Huyck et al. (2005) presented an analytical solution to the Boussinesq equation (in a different form) for variable widths and recharge rates. This is highly relevant work and has not been discussed. For example, equation 6 implies that the recharge is constant within each time step, which is exactly the same approach as Huyck et al. (2005).

- It should be clarified how the results from Troch et al. (2004) were obtained. Were these provided by any of the authors of that paper? Lines 216 and further indicate that the authors did not code these analytical solutions. Then how were these results obtained.

- The statement on line 222 is problematic: Verhoest and Troch (2000) do not state anywhere that they require 999 terms. They only state that after so many terms the residuals become insignificant, but they never performed an analysis on this. Usually, with these solutions, the results become stable after less than 100 summations.

- My major concern is the statement on line 247: the analytical solution is supposed to be highly sensitive to the fitting parameter  $b$ . When comparing a numerical solution to an analytical solution, the results should ALWAYS be equal, regardless of the parameters that are used. The only exception is when oscillations are obtained, but then either the temporal or spatial discretization should be modified. Looking at figures 8 through 15, it is clear that the discrepancies are too large, and something must be wrong. I did not check

the mathematical solution, but either there is an issue there, and/or there is something wrong in the coding, and/or the numerical solution has issues. This is something that must be corrected before the paper can be accepted.

- Line 304 states that the results from Troch et al. (2003) were obtained by solving their equation numerically. Line 230-231 states that the numerical solutions of Troch et al. (2003) matches the newly developed numerical solution well. This supports my suspicion that something is not right with the new analytical solution.

- There are too many figures in the paper. Something like 12 figures for a paper of this length should be the maximum. For example, I do not think that figure 2 is needed. The comparison with Troch et al. should be presented in less figures, as well as the comparison between the numerical and analytical solutions.

Based on these comments, I do not think that the paper is acceptable at this point.