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Reply on RC1

Ping-Cheng Hsieh and Tzu-Ting Huang

Author 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-AC1, 2021

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).

Thank you very much for your comment. After examining the study of Huyck et al. (2005), we could find that in their study the Boussinesq eq. is in a different form, and they derived the analytical solutions by the Laplace transform method for different time steps and then took a summation of all the solutions. The derivation process of analytical solutions is clear but a little complicated when compared with ours. Although the concept of our Eq. (6), meaning that the recharge is constant within each time step, is the same as Huyck et al. (2005), the expressions are different. Don’t you think our expression is more concise and neater? We will add the citation and discussion of Huyck et al. (2005) to the article. Thank you again.

- 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.

Thank you very much for your comment. The results obtained from Troch et al. (2004) were not provided by any of the authors of that paper. Because the analytical solutions are given as Eqs. (33) and (35) of the paper of Troch et al. (2004), anyone can use them to reproduce the results in the paper. We will revise the description as follows:

“Both figures reveal that our results using the generalized integral transform technique agree well with the analytical solutions derived by the Laplace transform method, i.e. Eqs. (33) and (35) in Troch et al. (2004), thus validating our analytical solutions.”

- 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.

Thank you very much for your comment. We mistook the meaning "summation of the first 999 terms" from Verhoest and Troch (2000), and we will revise the description as follows:

"As stated in Verhoest and Troch (2000), after solution summation of the first 999 terms, namely $O(10^3)$, the residuals become insignificant. In the present study, the solution summation of the first $O(10^2)$ terms, usually less than 50, could reach convergence. The convergence of the present solution is better."

- 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.

Thank you very much for your comment. The reviewer said "When comparing a numerical solution to an analytical solution, the results should ALWAYS be equal, regardless of the parameters that are used." I totally agree with this point when both solutions are derived from the same governing equations, initial/boundary conditions and input parameters. However, an analytical solution to a LINEARIZED governing equation is possibly not equal to a numerical solution to a NONLINEAR governing equation. This present analytical solution is obtained for a linearized equation, but the present numerical solution is for a nonlinear equation which was described on Line 177 (original version of MS) "a numerical model was developed to solve the original nonlinear equation, Eq. (4)". Both solutions are to different governing equations, so there are discrepancies in between. For the numerical solution by a finite difference method (F.D.M.) to the same LINEARIZED equation, the results are given below:

It shows that the numerical solutions are equal to the analytical solutions based on the same governing equations and same scenarios, thus justifying that the present analytical solutions are correct.

- 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.

Thank you very much for your comment. The present numerical solutions are for the nonlinear governing equation in our study. In Troch et al. (2003), they derived a numerical solution by finite difference for the same nonlinear equation, Equation (6) in their study. Both results match each other, and this justifies the present numerical solutions in our study are correct. The present analytical solutions to the linearized equation have been justified correct as shown in the response of last comment.

- 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.

Thank you very much for your comment. Original Figures 2, 5, 8, 11, and 14 are deleted now.

Thank you very much for all of your precious comments and suggestions.

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2021-50/hess-2021-50-AC1-supplement.pdf