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Discussions

Interactive comment on "A Comprehensive Quasi-3D Model for Regional-Scale Unsaturated-Saturated Water Flow" *by* Wei Mao et al.

Wei Mao et al.

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Response to Review of the Manuscript

Dear Editor, We would like to thank you and the reviewers. The reviewers have raised a number of important comments which we address in this Response. Below are our point-by-point responses to the review comments.

Comments from Referee #2:

General Evaluation:

The paper by Wei Mao et al proposes a novel approach to deal with the modelling of

regional flow in both the unsaturated and saturated zone using a coupling between a simple 1D unsaturated flow model – UBMOD – and the 3D groundwater flow model MODFLOW. The approach proposed is interesting and relevant because the degree of complexity is in between the full approaches that use the 3D Richards equation and simpler approaches that rely for instance on the Boussinesq equation. I like the fact that the authors went through a detail testing of their coupled model using synthetical test cases and an intercomparison with a detailed model. The application to a real-world system is also to be praised. The paper is well written and structured. Although of interest, I think that the paper should be improved before being considered for publication in HESS. I would like the authors to consider the following comments to improve the overall quality of the manuscript.

Response:

We thank the reviewer for his/her positive evaluation of the manuscript. Below are the detailed responses to the comments.

Specific comments

Comment 1:

The introduction should be improved to clearly state what are the advantages of such approaches compared to full approaches or even simpler approaches. Some inconsistencies should be corrected so that to make it clearer (see specific comments). The statements from line 127 to 131 about practicability is only partly true regarding the progresses made in pre and post-processing associated to GIS and dedicated software developments. I think this part and all the comment associated to the python scripts can be removed from the paper.

Response 1:

We thank the reviewer for the suggestion. In the revised manuscript, we will reorganize the introduction by adding more introduction of the fully 3-D models and simpler models. As suggested by the reviewer, the statements about the practicability regarding the pre and post-processing associated to GIS may oversell our model and will be rephased. The comments associated to the Python scripts will be removed in the revised manuscript.

Comment 2:

Although already published in Mao et al (2018), the UBMOD model should be presented with more details so that the reader can understand the interest and advantages of using it instead of another approach. Equation (1) should be explain clearly as the q term does not appear afterwards. The way I is computed/estimated should also be explained as it may control the way moisture dynamics is simulated. The correction factor mentioned lines 200-201 should also be explained clearly as the ability to handle heterogeneity is presented as one of the strengths of UBMOD compared to other approaches (see line 100). A proper description of UBMOD is also needed because the coupling algorithm strongly depends on how the different recharges are computed.

Response 2:

The description of UBMOD was too simple in the current manuscript, which led to the confusions. The subscripts will be added in Eq. (1). The variable qi indicates the allocated water per unit area for layer i. The variable I indicates the whole quantity of infiltration water per unit area. The UBMOD model is a water balance model which can simulate both upward and downward soil water movement in heterogeneous situations. Moreover, only four physical meaning parameters (saturated hydraulic conductivity Ks, saturated water content θs, field capacity θf, and residual water content θr) are needed, and the model can be used with a coarse discretization in space and time. As demonstrated by hypothetical examples and real-world applications, the model can simulate soil water movement effectively and efficiently. We add an appendix as attachment to introduce the major procedures of UBMOD. We will add more details about UBMOD in the revised manuscript. Also, the source code and report of UBMOD will be uploaded

in website during the revision process.

Comment 3:

In my opinion, using ARE and RMSE is not enough to efficiently compare the simulated results. The quality of the proposed approach is based on the comparison between ARE/RMSE indices produced by the coupled model and other models. Overall, I think that the results presented should be commented in greater details.

Response 3:

Thanks for pointing out that. The Index of Agreement (IA) and the determination coefficient (R2) will be added to evaluate the misfit between the coupled model and other models. And more discussions about the results will be added as suggested by the reviewer in the following comments.

Comment 4:

The results presented in section 3.2 on the two synthetical test cases raise some serious questions about the relevancy of the proposed approach. For test case 1, the patterns of soil moisture are similar, but the profiles are very different. The water table depths evolutions with time are also quite different. For test case 2, the largest differences appear at early time and for profile close to the recharge zone. The authors state that these differences are related to lateral flow, but I am not convinced regarding their figures. The main problem for me is that the recharge is clearly underestimated in test case 1 (higher water table with the coupled approach) while recharge is clearly overestimated (mainly at early time but also a little bit after) for test case 2. I acknowledge that the authors made a great effort to discuss the limitations of their model, but I think this part should be improved. As the recharge simulated by UBMOD and HYDRUS for the 2 test cases are very different, I wonder if UBMOD has really the ability to simulate a correct recharge with a coarse vertical discretization.

Response 4:

We agree that the results presented by case 1 and case 2 have deviations. The parameter specific yield is used in our groundwater model to calculate the change of groundwater table, while this parameter does not exist in models with Richards' equation, e.g., HYDRUS-1D. For case 1, we used a constant specific yield in MODFLOW to calculate the water table depth while the specific yield estimated by HYDRUS-1D was changeable. So, we do not think the deviation is caused by recharge while we would think it is caused by the inconsistent specific yield. We recalculate the case by giving the average specific yield estimated by HYDRUS-1D, which can improve the results. The improved results of case 1 and more discussions about reasons causing the deviations will be added in the revised manuscript. For case 2, as the constant flux only applied to a part of the upper boundary, there is a significant lateral flux. The two-dimensional SWMS model gives the correct results. Our model fails to capture the groundwater table at time of 2 h as the unsaturated soil water flow are treated as a vertical movement. The purpose of the case is to discuss the limitations of our model under extreme conditions. The model matches the observation data well at the simulation times of 3 h, 4 h and 8 h. The similar deviations were also found by Xu et al. (2012) in their coupling model SWAP-MODFLOW. In this case, the recharge in the steady state should be equal to the upper flux. The calculated recharge by our model is 3.55 m/d per unit area in the steady state, which demonstrates the model can capture the recharge accurately. The comparison of recharge in the steady state will be added in the revised manuscript, and we will add more discussion about the deviations as mentioned above.

Comment 5:

The calibration results presented figure 9 also raise some questions about how UBMOD can properly estimate recharge. It seems that the coupled approach does not allow to simulate properly the variability on time of water table depths. This question should be addressed as this could be linked to the fact that the recharge computed by UBMOD and the coupling algorithm is approximative. I also do not agree with the

sentence line 504-505. Table 4 demonstrates that the coupled model can be used to estimate the recharge annually, but in my opinion the good performance of the coupled approach at a smaller time scale are not clearly demonstrated and should at least be discussed.

Response 5:

We agree that the conclusion in sentence 504-505 "these results indicate that our model can reasonably simulate the saturated water table depth in space and time." is too strong. As shown in Fig. 9, The simulated groundwater table for the farm land fits the observations well, while the results for village and the bared land are poor. The reason is caused by the inappropriate upper boundary conditions for village and bared land. In the current simulation, we ignored the plant in village and bared land, which led to the much smaller evapotranspiration rate used for village and bared land. We will recalculate the case with more propriated upper boundary conditions in the revised manuscript. As inspired by the reviewer, we will evaluate the recharge and phreatic evaporation at a smaller time scale to assess the model performance, e.g., we will calculate the recharge for groundwater during the autumn irrigation event and compare it with the experiment date in this area, and calculate the phreatic evaporation during the period without rainfall or irrigation and compare it with the experiment data. More discussions mentioned above will be added in the revised manuscript.

Comment 6:

The way temporal and spatial discretization are chosen and impact the results should be clearly discussed. The way the three levels of time discretization are chosen for each case should be explained somewhere as it may control how the coupled model converge, the accuracy of the coupled simulation as UBMOD and MODFLOW exchange information based on the definition of the stress period and the computation cost. Maybe a sensitivity analysis could be performed on the first test case to show how temporal and spatial resolution can affect the simulated result for the coupled model.

Response 6:

Thanks for the comments. We agree that the definition of the stress period ΔT and the spatial discretization own non-negligible impact on the simulation accuracy and computational cost, which worth to be carefully evaluated. In the revised manuscript, the sensitivity analysis of temporal and spatial resolution will be added in case 1.

Comment 7:

In my opinion, several sentences in the conclusion should be rephrased as the results presented does not clearly demonstrate what is stated. Specific comments: Abstract needs rewriting. The first sentence should be changed as many publications have shown that models relying on the full 3D Richards equation can be used at regional scale. The part on the results and the findings should also be modified.

Response 7:

The sentence was too strong. In the revised manuscript, we will rephrase these sentences and rewrite the abstract.

Comment 8:

Line 57-65: a clear distinction between vertical and horizontal discretization should be made here. A fine vertical resolution should be used to solve properly the Richards equation. For catchment scale simulation, a fine horizontal discretization is not always needed everywhere. I don't understand the last sentence of the paragraph, especially": : : the latter are commonly based on coarse discretization."

Response 8:

Thanks for pointing out this. The sentence in line 57-60 was insufficiently rigorous. The fine vertical space resolution and time resolution are needed for solving the Richards equation in the unsaturated zone. The vertical cell sizes near the soil surface are required the order of 1 cm (Downer and Ogden, 2004). For the groundwater models,

the vertical cell sizes may range from the order of 1 m to 100 m, while the corresponding time steps are used range from the order of 1 day to 1 month as usual. In the sentence ": : : the latter are commonly based on coarse discretization.", the latter indicates the groundwater models. In the revised manuscript, we will give a clear distinction between vertical and horizontal discretization for the line 57-65, and the groundwater models are commonly used with coarse time and space discretization will be described clearly.

Comment 9:

Lines 78-81: Please double check the references used here. For instance, in Mawxell et al (2014), most models solve the 3D Richards equation to describe flow processes in unsaturated/saturated porous medium. This reference is not appropriate here. I did not check all the references.

Response 9:

Maxwell et al. (2014) compared 7 coupled surface-subsurface models based on 5 synthetic benchmark problems, 5 (CATHY, HydroGeoSphere, OGS, ParFlow, PIHM) of which are developed based on fully 3-D Richards' equation, and 2 models (PAWS, tRIBS+VEGGIE) by Quasi-3D schemes. The reference may lead to ambiguity. In the revised manuscript, we will delete this reference here.

Comment 10:

Line 119-126: This part is not clear to me. It seems regarding the references cited that an iterative coupling through hydraulic head has already been used but the sentences after state that a new scheme must be developed. Please rephrase to make it clearer.

Response 10:

The commonly used exchange information is hydraulic head for the existed iterative coupling scheme. The iterative coupling scheme between a hydrodynamic model MODFLOW and the water balance model UBMOD is different with the former one. The water balance model which uses "θ" as the independent variable is iterative coupled with the hydrodynamic model which uses “h” as the independent variable. Therefore, we state a new scheme developed in the manuscript. In the revised manuscript, we will rephrase this sentence to make it clearly.

Comment 11:

Line 134-135: This sentence is not consistent as it is stated that the coupling is performed through groundwater recharge and the depth of the unsaturated zone (related to the hydraulic head). Please make the description of the coupling consistent in the introduction.

Response 11:

The question is consistent with the above one. We will rephase the sentence to make it clearly.

Comment 12:

Fig 1(a) is not needed as the approach/scheme is very classical.

Response 12:

The Fig 1(a) will be deleted in the revised manuscript.

Comment 13:

Line 258-260: How the other boundary conditions are handled?

Response 13:

The lower boundary condition of UBMOD is treated as the upper boundary condition of MODFLOW. The other boundary conditions are the lower boundary condition of the whole region and the surrounding boundary conditions. The lower boundary condition of the whole region is set in MODFLOW. As the soil water movement is reduced to one dimensional flow, the surrounding boundary conditions for the unsaturated zone are no-flux boundary, while the surrounding boundary conditions for the saturated zone

are set in MODFLOW as usual. The details will be added in the revised manuscript.

Comment 14:

Line 260-262: see previous comment on Abstract – some models that relies on the full 3D Richards equation are used and applied at the catchment scale.

Response 14:

Thanks for pointing it out. The sentence was too strong and led to ambiguity. In the revised manuscript, we will rephrase the sentence.

Comment 15:

Line 302 and after: please discuss how the three-time levels are chosen and the potential effect of the choice on the simulated results (convergence, computational cost,...)

Response 15:

Thanks for the thoughtful comment. The stress time step (ΔT) is used in the iterative process and it is constant during the calculation. The time steps for UBMOD (Δtu) and MODFLOW (Δts) are the calculating time step for the unsaturated flow and groundwater flow respectively. It is not necessary to set the Δtu and Δts to be equal. The time step for UBMOD (Δtu) is a priori value and cannot be changed during the calculation. The specific time step for MODFLOW (Δts) is not constant and can be adjusted during the calculation. A time step multiplier is set priori to adjust the time step in MODFLOW (Harbaugh, 2005). As recommended by reviewer, the sensitivity analysis of the stress time step (ΔT) will be added in case 1. What’s more, the analysis for the appropriate stress time step (ΔT) in the real-world application will be added and discussed.

Comment 16:

Line 427-437: It seems that the gain in computational cost is not so big. Can you comment?

Response 16:

For case 1, HYDRUS-1D solves the 1D Richards equation, while our model has two components and the iterative process is needed. Therefore, the computational cost of our model for case 1 is larger than that of HYDRUS-1D. For case 2, our model shows its advantage with half computational cost than SWMS2D. This is caused by fewer nodes needed in the unsaturated zone and only 1D vertical flow is considered by the proposed model. The advantage of decreasing computational cost is not obvious for these two cases due to its relative smaller scale. When the application scale becomes larger, the advantage of the simplification method will be more obvious. In the revised manuscript, we will add the discussion how the developed model decreases the computational cost, and also the computational cost for the real-world application will be added to emphasize it.

Comment 17:

Line 464-465: the way ET0 is computed for 2004 is not clear for me.

Response 17:

We lack the weather data to calculate the potential evapotranspiration in 2004. However, we have the measured evaporation data from the 20 cm pan (ET20). Therefore, the potential evapotranspiration (ET0) was calculated by multiplying the ET20 with the empirical coefficient (K) provided by Hao (2016). The empirical coefficient (K) was 0.55, which was obtained by comparing monthly ET0 and ET20 with 8 years' data in this area. The description will be added in the revised manuscript.

Comment 18:

Line 486-487: the stress period is set to 5 days, which means that UBMOD and MODFLOW exchange information every 5 days. This should be linked to an estimation of the time needed for the rainfall/irrigation water to reach the water table.

Response 18:

We agree that the upper boundary condition should be considered when setting the stress period. Currently, this value is set as a priori and is constant during the calculation. In the revised manuscript, the analysis for choosing the appropriate stress time step ($\triangle$T) will be added for this case.

Downer, C. and Ogden, F.: Appropriate vertical discretization of Richards' equation for twoâĂŘdimensional watershedâĂŘscale modelling, Hydrol. Process., 18(1), 1-22, doi:10.1002/hyp.1306, 2004. Harbaugh, A.W.: MODFLOW-2005, The U.S. Geological Survey modular ground-water model — the Ground-Water Flow Process. U.S. Geological Survey Techniques and Methods 6-A16, variously p, 2005. Mao, W., Yang, J. Zhu, Y., Ye, M., Liu, Z. and Wu, J.: An efficient soil water balance model based on hybrid numerical and statistical methods, J. Hydrol., 559, 721-735, doi:10.1016/j.jhydrol.2018.02.074, 2018. Xu, X., Huang, G., Zhan, H., Qu, Z. and Huang, Q.: Integration of SWAP and MODFLOW-2000 for modeling groundwater dynamics in shallow water table areas, J. Hydrol., 412, 170-181, doi:10.1016/j.jhydrol.2011.07.002, 2012.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-87/hess-2019-87-AC2-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-87, 2019.