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

Anonymous Referee #2

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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.

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Discussion paper

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.

General comments:

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.

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.

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.

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.

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.

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.

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.

- 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."

- 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.

- 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.

- 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.

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

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

- 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.

- 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,...)

- Line 427-437: It seems that the gain in computational cost is not so big. Can you comment? - Line 464-465: the way ET0 is computed for 2004 is not clear for me.

- 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.

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