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. We have revised our manuscript to address the comments. Below are our point-by-point responses to the review comments.

Comments from Referee #1:

General Evaluation:

The paper presents a combination of a simplified one-dimensional unsaturated flow model and a full-3D MODFLOW aquifer model to achieve regional-scale modelling of a system consisting of a soil and an underlying phreatic aquifer over an impermeable layer.

The paper presents a logical step in the model development of the unsaturated zone model (UBMOD). I was unfamiliar with that model but I like it. Unfortunately, we have a conflict with Elsevier at the moment so I have no access to the paper that describes the model. Perhaps for that reason I would like to have the equation used for the drainage function included in the paper. Also I would like to know how the hydraulic conductivity is related to the water content, and how the water content is related to the matric potential.

I agree with the way the authors established the coupling between UBMOD and MODFLOW. This coupling is the main contribution of the paper, as both models have already been published. The coupling is not trivial and appears to be well-conceived, so I have no reservations about the suitability for publication in HESS.

Response:

We thank the reviewer for his positive evaluation of the manuscript. We reorganize the description of UBMOD to make it self-contained in the revised manuscript. An appendix is now attached to show the details of UBMOD. Moreover, the source code of UBMOD can be found in the website https://github.com/Weiwei-Mao/UBMOD.

Specific comments

Comment 1:

The structure of the paper is logical and clear. The writing is mostly clear, but the English will need editing. The only sections that I really could not follow were Equation (1) and the description of the iterative procedure.

Response 1:

We have done a thorough editing to improve the English flow. The responses to the comments are as follows.

Comment 2:

In Eq. (1), I expected the layer thickness M to have an index running between 1 and j indicating the layer number. From the description it is not clear to me if the equation applies to M_1 (the top layer) or involves a summation over all layers ($M_1 ... M_j$). I believe this can be easily clarified. I also would like a more thorough explanation of the way infiltration is handled. I do not understand the difference between I and I_d. I also could not find anything about the partitioning of rainfall between infiltration and runoff, and about the way infiltration is added to the soil water. The paper mentions 'allocation of infiltration' but I do not understand what that means. Evapotranspiration was not discussed either. As I explained I am unable to consult the paper in which UBMOD was discussed, but I believe it is acceptable to repeat the key points of UBMOD here, with proper referencing to the earlier paper.

Response 2:

The description of UBMOD was too simple in the previous manuscript. In the revised manuscript, we rephrase the introduction of UBMOD. Equation (1) is rewritten and more illustration about the allocation of infiltration water is added as "*Firstly, the vertical soil column is divided into a cascade of "buckets" and each "bucket" corresponds to a soil layer. The "buckets" will be filled to saturation from the top layer to the bottom layer if there is infiltration, which is referred as the allocation of infiltration water. Specifically speaking, the infiltration water first fills the top "bucket",* *and then the excessive infiltration water moves downward to the next "bucket", until all the infiltration water is allocated in the "buckets". The governing equation of layer i is,*

$$q_i = \min\left(M_i \times \left(\theta_{\mathrm{s},i} - \theta_i\right), I - I_{\mathrm{d},i\text{-}1}\right), \tag{1}$$

where i indicates the vertical soil layer, i = 1, … , j; q_i is the amount of allocated water per unit area of layer i [L]; M_i is the thickness of layer i [L]; θ_i is the initial soil water content of layer i [L^3L^{-3}]; $\theta_{s,i}$ is the saturated soil water content of layer i [L^3L^{-3}]; I is the quantity of infiltration rate [L]; $I_{d,\ i-1}$ is the consumed infiltration water per unit area by all upper layers above layer i [L]. The infiltration rate I is an input data in the model, and the partitioning of rainfall between infiltration and runoff has not been considered by now." Please see Lines 171~185.

The description of evapotranspiration is added as "*The Penman-Monteith formula and Beer's law (also known as Ritchie-type equation) are adopted to estimate the potential soil evaporation E_p and potential crop transpiration T_p. Then E_p and T_p are distributed to each layer based on the evaporation cumulative distribution function and the root density function. The actual soil evaporation and crop transpiration are obtained by discounting E_p and T_p with the soil water stress coefficient.*" Please see Lines 197~202.

What's more, an appendix is added to introduce more details of UBMOD. Please see the attached Appendix.

Comment 3:

Regarding the iterative solution, Figure 2 is not always helpful in supporting the text to explain the iterative process. I indicated where I got lost in the pdf file. I also make suggestions for improvements there. I was also puzzled by the three time increments (the stress time and the time steps for UMBOD and ModFlow). I cannot really see how they interact in the iterative process, where you only use one type of time step without indicating which of the three it is.

You set the values of the three time steps a priori for all tests without indicating how you arrived at the chosen values, or helping the reader find the optimal values for a given problem. Also, if I am not mistaken, the time steps for UMBOD and ModFlow are constant and equal for all test cases. Is this a necessity in this model?

Response 3:

The Fig. (2) is reorganized. We add more explanations in the figure to make it self-contained. The fonts of the figure have been enlarged. Please see Fig. 2.

More details about the three time increments are added as "*The time coupling method is shown in Fig. 2(b). There are three levels of time discretization in the coupled model as follows: the stress period ΔT used in MODFLOW, the calculation time step for MODFLOW Δt_s, and the calculation time step for UBMOD Δt_u. The stress time step (ΔT) is also used in the iterative process, and the unsaturated model UBMOD and saturated model MODFLOW exchange information at the end of each stress period. Δt_u is a priori value and cannot be changed during the calculation. The UBMOD can give acceptable results when Δt_u is shorter than 10 d for assumed cases and 1 d for a real-world case (Mao et al., 2018). Δt_s is set as the technical report described by Harbaugh (2005) and can be changed during the calculation.*" Please see Line 309~317.

As also recommended by Reviewer 2, the influence of the temporal and spatial discretization and stress period on simulation results are added in the revised manuscript, which will be helpful to find the optimal values. A section "*3.2.2 Influence of the temporal and spatial discretization as well as the stress period on simulation results*" is added. Please see Lines 456~476.

Fig. 2(c) is now redesigned to illustrate the iterative processes, and description is reorganized in the revised manuscript as " *The implementation of iterative coupling scheme is shown in Fig. 2(c), which shows the calculation period from t to $t+\Delta T$. At the time t, the saturated hydraulic head is known, marked as $\boldsymbol{H}^t$ ($m\times n$ dimension). When the model runs from t to $t+\Delta T$, firstly, the initial saturated hydraulic head $\boldsymbol{H}^{t+\Delta T}$ at $t+\Delta T$ is set to be equal to $\boldsymbol{H}^t$, and then the average unsaturated depth from t to $t+\Delta T$ is calculated according to $\boldsymbol{H}^{t+\Delta T}$, marked as $\boldsymbol{Du}^{t+\Delta T,p}$ (l elements). p is the iteration level. The $d_u^{t+\Delta T,p}$ for one soil column is calculated as follows,*

$$d_u^{t+\Delta T,p} = \overline{D} - \overline{H^{t+\Delta T}}\ , \tag{8}$$

where $\overline{D}$ *is the average depth from the soil surface to the impermeable layer of the controlling domain of the soil column [L];* $\overline{H^{t+\Delta T}}$ *is the average thickness of controlling saturated domain of the soil column [L].*

Secondly, the model runs UBMOD with the unsaturated time step Δt_u *to obtain the vertical recharge at each time step (marked as* r_t*) until the time comes to be* $t+\Delta T$*. The total recharge during the stress period* ΔT *(from* t *to* $t+\Delta T$*)* $R_{\Delta T}$ *can be obtained by summarizing the recharge at each unsaturated time step, as follows,*

$$R_{\Delta T} = \sum_{t}^{t+\Delta T} r_t\ , \tag{9}$$

The average recharge R *from* t *to* $t+\Delta T$ *can be obtained by,*

$$R = R_{\Delta T} / \Delta T\ . \tag{10}$$

Then the average recharge from all 1-D soil columns can be obtained, represented as $\boldsymbol{R}^{t+\Delta T,\ p}$*, which is then used by the MODFLOW RCH package. Subsequently, the model runs the MODFLOW model with the saturated time step* Δt_s *to obtain the saturated hydraulic head until the time comes to* $t+\Delta T$*. The hydraulic head at the time* $t+\Delta T$ *is marked as* $\boldsymbol{H}^{t+\Delta T,\ p}$ *(*$m \times n$ *dimension). The convergence of the iteration is determined by using the difference of hydraulic head between the present* $\boldsymbol{H}^{t+\Delta T,\ p}$ *and the initial* $\boldsymbol{H}^{t+\Delta T}$*. The convergence criterion is,*

$$if\ \ \max\left(\left|\mathbf{H}^{t+\Delta T,p} - \mathbf{H}^{t+\Delta T}\right|\right) < \varepsilon_H\ , \tag{11}$$

where ε_H *is a user-specified tolerance [L]. If the criterion is met, the iteration stops, and* $\boldsymbol{H}^{t+\Delta T,\ p}$ *is the convergent results at time* $t+\Delta T$*, and the model proceeds to the next stress period. Otherwise, the iteration continues to* $p+1$ *and* $\boldsymbol{H}^{t+\Delta T,\ p}$ *will be used to calculate the average unsaturated depth shown in Eq. (8). The above procedures will be repeated until the convergence criterion of Eq. (11) is met.*" Please see Lines 318~348.

Comment 4:

The test cases are limited in scope and very much non-regional. I suggest to reduce the overselling of the test case based on data from the Hupselse Beek, since it is really only a single profile that is being considered. The second test case is a 2D problem of a system of only a few meters. I do not consider the limited scale to the test cases a serious drawback because they do the job of providing a test of various model components. And the demonstration case that follows the test cases truly aims at the scale for which the model is intended.

Response 4:

Thanks for the suggestion. In the revised manuscript, we declare that we only simulated a single field soil profile with the data provided by HYDRUS-1D technical manual. Please see Lines 369~371.

The purpose of case 2 is to discuss the limitations of our model which ignores the lateral flow in the unsaturated zone. We agree that the soil water movement at a laboratory scale is different with that at the regional scale. This case may help us to understand the model performance under situations with non-ignorable lateral flow in the unsaturated zone. Therefore, we would like to keep case 2. In the revised manuscript, we clarify the purpose of the case. Please see Line 392~393.

Comment 5:

In a few locations the grammar was such that I could not discern the meaning of a sentence (see detailed comments in the manuscript).

Response 5:

The language of the manuscript has been checked. Thank the reviewer for the comments.

Comment 6:

I printed out the figures so I could consult them while working on the pdf file to do the review, but the fonts were so small that I had a hard time reading the texts.

I suggest redesigning Figure 2. It cannot be read stand-alone, and I found that it not always helped me understand the iterative process.

Response 6:

The Fig. (2) has been redesigned. The fonts of the figure are enlarged. We add more details in the figure to ensure that it can be read on its own. Please see Fig. 2.

The presentation of the iterative processes has been reorganized in the revised manuscript. Please see the details in Response 3 or Lines 318~348 in the revised manuscript.

Comment 7:

In Eq. (2) I believe the minus sign should only be there is the vertical coordinate is defined positive downward, but the text indicates otherwise.

Response 7:

The referee is right that the vertical coordinate is defined positive downward. The ground surface is used as the base level. This description is added in the revised manuscript. Please see Line 189~190.

Comment 8:

All in all I consider this a good paper that deserves publication in HESS after moderate revisions. To help with the revisions I made some remarks directly on the manuscript.

Please also note the supplement to this comment:

https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-87/hess-2019-87-RC1-supplement.pdf

Response 8:

Thank the reviewer again for the comments. Most comments in the supplement are listed above. More comments are responded below.

Comment 9:

Don't you need an average water content as well to do so? How did you determine this average water content? I suspect that most of the downward water movement take place when the soil is relatively wet. Therefore, a simple time-averaged water content probably underestimates the amount of groundwater recharge.

Response 9:

It is a thoughtful comment. The tracer is injected at a specified depth (1 m) from the soil surface. The downward velocity of soil water is calculated from the monitoring of the downward movement of tracer peaks. The recharge rate (R) is estimated as,

$$R = \theta \cdot v = \theta \cdot \frac{\Delta z}{\Delta t} \tag{1}$$

where v is the vertical percolating velocity of soil water, which equals to the downward movement of tracer peaks; Δz is migration depth of the tracer peak; Δt is the time between tracer application and sampling; θ is the averaged volumetric water content from the initial position of the tracer to the final position of the tracer. The soil water content along depth profiles are measured during each sample.

The accuracy of the soil water content θ will influence the results. We agree with the opinion that most of the downward water movement take place when the soil water is relatively wet. According to the measurements results of Peng (2015) in the same area (11 monitoring points, 1-3 times every month), the soil water content θ at the depth of 1 m is relatively stable in temporal with small variations in this area (See the below Fig.1). The soil water content θ at the depth of 1 m stabilizes at 0.3 cm^3/cm^3. The annual averaged variable coefficient ($cv = \sigma / \mu \times 100\%$, where σ is the standard deviation and μ is the mean value) is 8.96%. Therefore, the simple averaged water content would not greatly affect the results. We add the details of tracer experiment in the revised manuscript. Please see Lines 607~614.

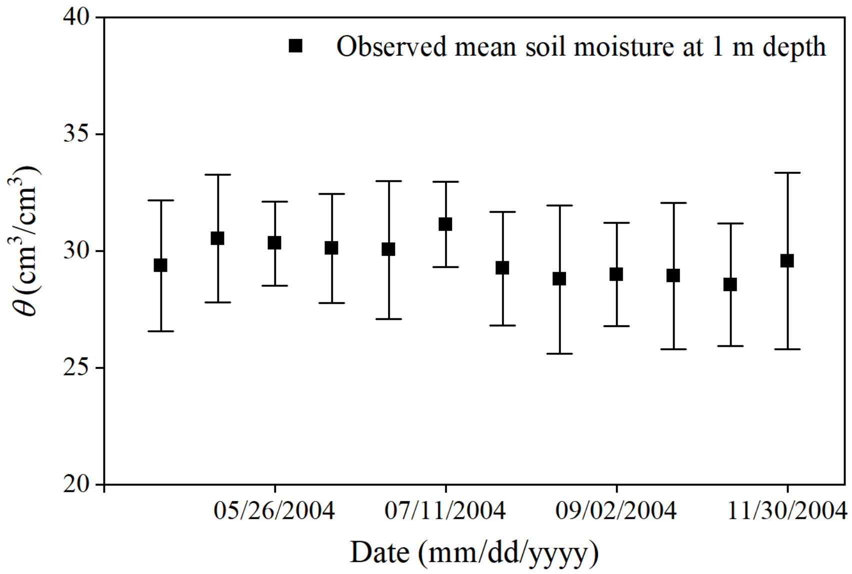

Fig. 1 The soil water content along time at the depth of 1 m in the study area. (Peng, Z.Y., 2015, Mechanism and modeling of coupled water-heat-solute movement in unidirectional freezing soils, Doctor thesis, School of Water Resources and Hydropower Engineering, Wuhan University, China, 2018.)

Comment 10:

I suspect the model also does not perform too well if there is a substantial transfer of water between soil profiles through the groundwater: recharge in one location is wetting up the soil elsewhere. This can occur in areas with slopes and in areas with bare soils at one place and thriving vegetation elsewhere.

Although your model should be able to handle this if the soil columns are well chosen and placed correctly. I am not so familiar with Modflow, but I believe it can handle slopes.

This implies that you need to know beforehand that the problem may occur so you can set up your model accordingly. If you are not aware of the problem and choose too few subunits, the model will probably not simulate the true extent of the lateral transfer of water.

Response 10:

The developed model can simulate the lateral flow through the groundwater since MODFLOW is a three-dimensional model, while the model cannot simulate the lateral flow in the unsaturated zone because UBMOD is a one-dimensional model ignoring the lateral unsaturated flow. So, the model can be applied with slopes with lateral groundwater flow.

Indeed, the land usage type, the climate condition and topography condition should be considered to set the soil columns when setting up the model. The land usage type has the most important impact on the division of the subareas in agricultural areas, since it significantly impacts the upper boundary conditions (irrigation and evapotranspiration). Therefore, for the real-world application in the manuscript, the subareas were divided by the land usage type.

The declaration of choosing soil columns for the real-world application is added in the revised manuscript as "*This irrigation area was divided into three sub-areas according to the land usage since they own significantly different upper boundary conditions, which are farm land, villages and bared soil, as shown in Fig. 8(b).*". Please see Lines 525~527.

Comment 11:

Line 135, "the depth of unsaturated zone", This is equal to the groundwater depth, correct?

Response 11:

The referee is right. The depth of unsaturated zone indicates the depth from soil surface to the groundwater table, which is equal to the groundwater table depth. This is clarified in the revised manuscript. Please see Line 137.

Comment 12:

Line 189, "pressure head", In the unsaturated zone, the term 'matric potential' is more accurate.

Response 12:

Thanks for the suggestion. We have revised it in the manuscript.

Comment 13:

Line 234, “Fig. 1(b)”, In the figure it is incorrectly labeled 'a'.

Response 13:

This has been revised in the manuscript.

Comment 14:

Line 267, “overlap region”, Am I correct to summarize that the overlap zone needs to have a high vertical resolution to ensure that the groundwater level dynamics are simulated correctly?

Response 14:

The reviewer is right. The overlap zone should have a fine vertical discretization in the unsaturated zone to ensure accuracy. This is clarified in the revised manuscript. Please see Lines 274~275.

Comment 15:

Line 254-256, “The vertical net recharge is represented by matrix **R** with $m \times n$ elements”, “**Du**”, So, the matrix is updated every time step, correct? And Du is udated every time step as well.

Response 15:

The referee is right. The vertical net recharge **R** and the depth of unsaturated zone **Du** are updated at the end of each stress period. The implementation of iterative coupling scheme is rewritten in the revised manuscript, and this is clarified in the revised manuscript. Please see Lines 318~348.

Comment 16:

Line 284, “q_I is the flux across the water table caused by allocation of the infiltration water per unit area [L]”, This phrase is not clear at this point. Also, I do not see how infiltration flow at the soil surface has to be added to the gravitational and matric potential-driven flows at the groundwater level. I would think that infiltration in the soil is modeled by the unsaturated flow model, and the resulting flux at the groundwater level arises through qa and qd. Or does qi represent the fraction of infiltration that travels through macropores directly to the groundwater level?

Response 16:

We have rephrased the section, and an Appendix about UBMOD has been added. The new description is shown as "*Firstly, the vertical soil column is divided into a cascade of "buckets" and each "bucket" corresponds to a soil layer. The "buckets" will be filled to saturation from the top layer to the bottom layer if there is infiltration, which is referred as the allocation of infiltration water. Specifically speaking, the infiltration water first fills the top "bucket", and then the excessive infiltration water moves downward to the next "bucket", until all the infiltration water is allocated in the "buckets".*" If there is a residual infiltration after all soil layers are filled, the infiltration water will across the water table. The amount of residual infiltration is denoted as q_I. The description of q_I is added in the revised manuscript. Please see Lines 293~296.

Comment 17:

Line 285-286, "q_A is the flux across the water table caused by the advective movement per unit area [L]", Is this gravity-driven flow?

Response 17:

The referee is right. q_A indicates the gravity-driven flow. The description is added as "*Then the advective flow q_A across the water table driven by gravitational potential is calculated by Eq. (2).*" Please see Lines 296~297.

Comment 18:

Line 287-288, "q_D is the flux across the water table caused by the water diffusion per unit area [L]" Is this flow driven by vertical gradients in the matric potential?

Response 18:

The referee is right. q_D indicates the flow driven by vertical gradients of the matric potential. The description is added as "*The amount of the upward flux between the virtual layer and layer j is denoted as q_D.*" Please see Lines 306~307.

Comment 19:

Line 293-294, “The q_S term is upward and resulted from groundwater by evapotranspiration.” But what happens when water infiltrates into a dry soil? In that case, the matric potential gradient is such that it creates a downward flow as well.

Response 19:

q_S is the flux across the groundwater table caused by evapotranspiration. The infiltration is not considered in this term, while it is calculated as q_I. The clarification is added as “*The q_S term is caused by evapotranspiration. When the critical depth of evapotranspiration is shallower than the groundwater table depth, the groundwater can be consumed by evapotranspiration and it causes an upward q_S term.*”, which can be found in Lines 297~300.

Comment 20:

Line 305-306, “The unsaturated zone and saturated zone exchange information at the end of each stress period.” I would like to see the equations so i can see exactly at what moment in time water contents and hydraulic heads are computed, and for what time interval the exchange of water between the saturated and the unsaturated zone is calculted from these two quantities. As it is I cannot see if you use a forward or a backward scheme, or something in between. Fig. 2 is not designed in a way that it provides that kind of information.

Line 307 “the iterative coupling scheme”, It does not do a good job at that. I really would like some more clarification on the iterative process.

Line 309, “at the beginning of the time or iteration loop”, This would suggest you use a forward scheme.

Line 312, “each time step”, You introduced three time steps, so you have to be very careful in explaining which of the three you mean. Is it delta tu here?

Response 20:

All the above comments are related with the iterative coupling scheme of the coupled model. We did not make the iterative coupling scheme clear in the previous manuscript. We rewrite this part in the revised manuscript to make it clear. And the Fig .2(c) is redesigned to show the iterative coupling scheme. The description of the iterative coupling scheme in the revised manuscript can be found in Lines 318~348.

Comment 21:

Line 488, “Model calibration results”, I think it is easier to report the *ARE* and *RMSE* values in tables instead of in the text.

Response 21:

Thanks for the suggestion. Table 2, Table 3 and Table 5 are added in the revised manuscript to show the statistical index of the two test cases and the real-world application.

Comments from Anonymous 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 have reorganized the introduction by adding the advantages and disadvantages of the fully 3-D models and simpler models as "*These fully 3-D models have solid theoretical foundation, and have been used for regional scale unsaturated-saturated water flow simulation. However, since the soil water flow is highly nonlinear in nature and sensitive to atmospheric changes, soil utilizations, and human activities, the numerical schemes require using fine discretization in vertical space and time for accurate numerical solutions (Downer and Ogden, 2004; Varado et al., 2006). This makes the numerical solutions computationally expensive, especially for large scale modeling.. (Van Walsum and Groenendijk, 2008; Shen and Phanikumar, 2010; Yang et al., 2016; Szymkiewicz et al., 2018). There are also many conceptual unsaturated-saturated water flow models, e.g., SWAT (Arnold et al., 2012), INFIL 3.0 (Fill, 2008), HSPF (Duda et al., 2012) and SALTMOD (Oosterbaan, 1998), which show advantages in mass balance and computational cost. However, these models usually adopt many empirical equations which result in poor performance comparing with the fully 3-D numerical models.*" Please see Lines 58~71.

As suggested by the reviewer, the inconsistencies pointing out by the specific comments are corrected. The statements about the practicability regarding the pre and post-processing associated to GIS may oversell our model. Therefore, the comments associated to the Python scripts are 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 previous manuscript, which led to confusions. In the revised manuscript, we rephrase the introduction of UBMOD and add an appendix to introduce the details of UBMOD.

Equation (1) is rewritten and more illustration about the allocation of infiltration water is added as "*Firstly, the vertical soil column is divided into a cascade of "buckets" and each "bucket" corresponds to a soil layer. The "buckets" will be filled to saturation from the top layer to the bottom layer if there is infiltration, which is referred as the allocation of infiltration water. Specifically speaking, the infiltration water first fills the top "bucket", and then the excessive infiltration water moves downward to the next "bucket", until all the infiltration water is allocated in the "buckets". The governing equation of layer i is,*

$$q_i = \min\left(M_i \times \left(\theta_{\mathrm{s},i} - \theta_i\right), I - I_{\mathrm{d},i\text{-}1}\right), \tag{1}$$

where i indicates the vertical soil layer, i = 1, … , j; q_i *is the amount of allocated water per unit area of layer i [L];* M_i *is the thickness of layer i [L];* θ_i *is the initial soil water content of layer i* $[L^3L^{-3}]$*;* $\theta_{s,i}$ *is the saturated soil water content of layer i* $[L^3L^{-3}]$*; I is the quantity of infiltration rate [L];* $I_{d,\,i\text{-}1}$ *is the consumed infiltration water per unit area by all upper layers above layer i [L]. The infiltration rate I is an input data* *in the model, and the partitioning of rainfall between infiltration and runoff has not been considered by now.*" Please see Lines 171~185.

We add the declaration of the relationship between the description of UBMOD (Sect. 2.1) and the coupling scheme (Sect. 2.4) when calculating the groundwater recharge. The details about calculating the groundwater recharge are rewritten as, *"The net recharge R from soil zone is calculated by UBMOD as follows,*

$$R = q_{\mathrm{I}} + q_{\mathrm{A}} + q_{\mathrm{S}} + q_{\mathrm{D}}, \quad (6)$$

where q_I, q_A, q_S *and* q_D *are the fluxes across the water table caused by allocation of the infiltration water, the advective movement driven by the gravitational potential, source/sink terms and the water diffusion driven by the matric potential per unit area, respectively [L].*

These four terms are corresponded to the four major components in UBMOD, as described in Sect. 2.1. Specifically, the infiltration water is allocated first according to Eq. (1) if there is precipitation or irrigation. When there is residual infiltration water across the water table in the j-th layer, the amount of residual infiltration is denoted as q_I. *Then the advective flow* q_A *across the water table driven by gravitational potential is calculated by Eq. (2). The direction of these two terms is downward. The* q_S *term is caused by evapotranspiration. When the critical depth of evapotranspiration is shallower than the groundwater table depth, the groundwater can be consumed by evapotranspiration and it causes an upward* q_S *term. A virtual layer is needed when calculating the diffusive movement driven by matric potential across the water table based on Eq. (4). As shown in Fig. 2 (a), the virtual layer will be added under water table, numbered as layer j+1. The thickness,* M_{j+1} *[L], of the layer is set as,*

$$M_{j+1} = z_{j+1} - d_u, \quad (7)$$

where z_{j+1} *is the bottom depth of layer j+1 [L];* d_u *is the thickness of unsaturated zone [L]. The amount of the upward flux between the virtual layer and layer j is denoted as* q_D. *Then, the net recharge matrix* ***R*** *for the whole area is obtained and used for the Recharge (RCH) package of MODFLOW."* Please see Lines in 286~308.

The correction factor mentioned in lines 200-201 is one of the major highlights of

UBMOD. The diffusive movement driven by the matric potential under homogeneous and heterogeneous soils can be expressed as,

for homogeneous soil,

$$\frac{\partial \theta}{\partial t}=\frac{\partial}{\partial z}\left(D(\theta)\frac{\partial \theta}{\partial z}\right), \tag{2}$$

and for heterogeneous soil,

$$\frac{\partial \theta}{\partial t}=\frac{\partial}{\partial z}\left(D(\theta)\left(\frac{\partial \theta}{\partial z}-\left(\frac{\partial \theta}{\partial \theta_s}\frac{\partial \theta_s}{\partial z}+\frac{\partial \theta}{\partial \theta_r}\frac{\partial \theta_r}{\partial z}+\frac{\partial \theta}{\partial \alpha}\frac{\partial \alpha}{\partial z}+\frac{\partial \theta}{\partial n}\frac{\partial n}{\partial z}\right)\right)\right), \tag{3}$$

where $D(\theta)$ is the hydraulic diffusivity [L^2T^{-1}]; θ_s is the saturated soil water content [L^3L^{-3}]; θ_r is the residual soil water content [L^3L^{-3}]; α and n are two parameters in van Genuchten model. The extra term on the right side of Eq. (3) is the correction term under heterogeneous conditions. We add an appendix to introduce the details that how UBMOD handles this correction term. Please see the Appendix.

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 (R^2) are added to evaluate the misfit between the coupled model and other models. Three tables (Table 2, Table 3 and Table 5) are added to list the statistical indexes calculated by the cases in the manuscript. The discussions of the calculated results are expanded. Please see section 3.2.

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 add more statistical index in the revised manuscript to show the model ability for unsaturated-saturated flow modeling as listed in Table 2, Table 3 and Table 5. The corresponding discussion can be found in Lines 419-455. For case 1, we add the groundwater consumption as an additional estimator, as "*Moreover, the net groundwater consumption at the end of the simulation period was compared, which is 0.132 m calculated by the coupled model, and it is the same with that from HYDRUS-1D*." Please see Lines 427-429. For case 2, the calculated recharge in the steady state is compared with the real value, as "*The calculated recharge is 3.55 m/d per unit area when the flow becomes steady, which equals to the input flux.*" Please see Lines 452~454.

We agree that the results presented by case 1 and case 2 have deviations. The reasons causing the deviations are different for these two cases. The reasons causing the deviations of case 1 are added as *"The deviations of groundwater table depth and soil water content from the coupled model and HYDRUS-1D can also be observed in Fig. 5. The deviations are caused by the different model structures of the coupled model and HYDRUS-1D. The HYDRUS-1D solves the saturated-unsaturated flow together, and the groundwater table is determined at the depth with the matric potential equaling to zero. The soil water content of the capillary fringe above the groundwater table is almost saturated. However, the UBMOD model cannot simulate the capillary fringe.*

And there is a parameter the field capacity used to calculate the downward movement of soil water, which is defined under a free drainage condition. So, the coupled model could lead to the lower soil water content in the capillary fringe and higher groundwater table as shown in Fig. 5. And there is another parameter specific yield used in the coupled model to determine the groundwater table, which also attributes to the deviation of groundwater table." Please see Lines 432-443.

For case 2, as the constant flux only applied to a part of the upper boundary, there is a significant lateral flux happening in the initial time. Since our model ignores the lateral flow in the unsaturated zone, it will cause more water flow downward to recharge groundwater and lead to a higher water table. The similar deviations were also found by Xu et al. (2012) and Shen and Phanikumar (2010) in their Quasi-3D models. 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. We add more discussion about the results calculated by Quasi-3D models. Please see Lines 452~455 and Lines 489~492.

Shen, C. and Phanikumar, M.: A process-based, distributed hydrologic model based on a large-scale method for surface-subsurface coupling, Adv. Water Resour., 33(12), 1524-1541, doi:10.1016/j.advwatres.2010.09.002, 2010.

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.

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:

Thanks for reminding this. As inspired by the reviewer, we carefully think about the misfit of groundwater table depth shown in the previous Fig. 9 (it is Fig. 10 in the revised manuscript). In our opinion, the reason is caused by the inappropriate upper boundary conditions for village and bared land. In the previous version, the simulated groundwater table for the farm land fits the observations well, while the results for village and the bared soil are poor. That is because that a very small evapotranspiration rate was given for village and bared soil in the previous version, and we ignored that there are natural plants growing in the bared soil and village. Then in the revised manuscript, we recalculate the case with more appropriate upper boundary conditions. The actual soil evaporation and crop transpiration of village and bared land are recalculated according to the study of Yu et al. (2019) in this area. The recalculated groundwater table is shown in the revised manuscript as Fig. 10. The *ARE* and *RMSE* for the regional average groundwater table decrease from 17.1% and 0.306 m to 9.9% and 0.203 m. The calculation results are improved significantly for village and bared land, as shown in Fig. 10 (d)(e)(f). The corresponding description about the results are modified in the revised manuscript. Please see Lines 566-578.

What's more, the calculation results of the coupled model at smaller time scales are discussed to show that the model can calculate the recharge rate. The descriptions are added in the revise manuscript as "*The recharge during short-term was calculated for further checking the results by comparing the results with those from reference papers. The calculated recharge in farm land during the autumn irrigation (from Oct 16 to Oct 31) is 93.3 mm, and the coefficient of recharge from the autumn irrigation is 0.37. Zhang (2011) proposed the coefficient of recharge from the autumn irrigation is approximately 0.3. Yang (2016) proposed that the coefficient of the recharge from the autumn irrigation is between 0.36 and 0.4. Yu (2017) used the coefficient of recharge from autumn irrigation as 0.33 for the district. The calculated result is consistent with* *the previous studies. The phreatic evaporation coefficient was estimated during the period from Sep 15 to Sep 30 with no precipitation or irrigation. The quantity of the recharge from saturated zone to unsaturated zone is 10.1 mm during the period in the farm land. The phreatic evaporation coefficient is 0.179, and the averaged water table depth is 1.51 m during the period. The phreatic evaporation coefficient measured by Wang (2002) is 0.172 at the depth of 1.5 m. The short-term results indicate the validity of the simulating results.*". Please see Line 579~592.

Yu, B., Shang, S., Zhu, W., Gentine, P., Cheng, Y.: Mapping daily evapotranspiration over a large irrigation district from MODIS data using a novel hybrid dual-source coupling model, Agr. Forst. Meteorol., 276-277, 2019.

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 suggestion. We agree that it is worthy to discuss the impact of the stress period ΔT and the spatial and temporal discretization on the simulation accuracy and computational cost. In the revised manuscript, 7 scenarios with different temporal and spatial resolution and stress period are added in case 1. The influence of the temporal and spatial discretization and stress period on the simulation accuracy is first discussed and added in a new section as "*section 3.2.2 Influence of the temporal and spatial discretization as well as the stress period on simulation results*". Please see Lines 456~476.

The influence of the temporal and spatial discretization and stress period on the computational cost is then discussed and added "*The computational cost of different scenarios in case 1 of the coupled model ranges from 49 s to 63 s as listed in Table 2. It is 1.4 s by HYDRUS-1D. The temporal and spatial discretization has slight influence on computational cost, while the stress period has significant influence on the computation cost. The iteration and information exchange are responsible for the high computational cost.*". Please see Line 498~503.

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:

Thank for the suggestion. The abstract is rewritten to make it more accurate and concise. The other parts on the results and findings are also modified. Please see the following response.

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 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 at 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. In the revised manuscript, we declare the fine vertical resolution should be used to solve properly the Richards' equation. Please see Lines 60~63.

In the revised manuscript, this paragraph is rewritten and last confused sentence ": : : the latter are commonly based on coarse discretization." is deleted.

Downer, C. and Ogden, F.: Appropriate vertical discretization of Richards' equation for two - dimensional watershed - scale modelling, Hydrol. Process., 18(1), 1-22, doi:10.1002/hyp.1306, 2004.

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 delete this reference.

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 iterative coupling scheme has already been used to couple two hydrodynamic models, both of which calculate the hydraulic head, and use the hydraulic head as the exchange information of the two models. However, we couple a soil water balance model (UBMOD) with a hydrodynamic model (MODFLOW). The soil water content (θ) is the variable calculated by UBMOD other than the hydraulic head (h). Therefore, the traditional implementation method of the iterative scheme is inapplicability. In this study, a specific implementation method of the iterative scheme is developed to couple the soil water balance model and the hydrodynamic groundwater model.

The reviewer is right that we do not develop “a new iterative scheme”, while a “new implementation method of iterative scheme” suitable for coupling a soil water balance model and a groundwater hydrodynamic model is developed. In the revised manuscript, we have rephrased this sentence to make it clear. Please see Line 124~132.

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 have rephased the sentence to make it clear. Please see Line 135~138.

Comment 12:

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

Response 12:

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

Comment 13:

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

Response 13:

The details about boundary conditions are added as *“The recharge at the bottom* *boundary calculated by UBMOD is treated as the upper boundary condition of MODFLOW. …… The lower boundary condition of the whole region is set in MODFLOW. As the soil water movement is reduced to 1-D 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 practical.”* Please see Lines 260~262 and 268~271.

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 delete 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 (Δt_u) and MODFLOW (Δt_s) are the calculating time step for the unsaturated flow and groundwater flow respectively. It is not necessary to set the Δt_u and Δt_s to be equal. The time step for UBMOD (Δt_u) is a priori value and cannot be changed during the calculation. The specific time step for MODFLOW (Δt_s) 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). The description about the three-time levels is added. Please see Line 309~317.

As recommended by the reviewer, the impact of the stress period ΔT and the spatial and temporal discretization on the simulation accuracy and computational cost are discussed in the revised manuscript. 7 scenarios with different temporal and spatial resolution and stress period are added in case 1. The influence of the temporal and spatial discretization and stress period on the simulation accuracy is first discussed and added in a new section as "*3.2.2 Influence of the temporal and spatial discretization as well as the stress period on simulation results*". Please see Lines 456~476.

The influence of the temporal and spatial discretization and stress period on the computational cost is then discussed and added "*The computational cost of different scenarios in case 1 of the coupled model ranges from 49 s to 63 s as listed in Table 2. It is 1.4 s by HYDRUS-1D. The temporal and spatial discretization has slight influence on computational cost, while the stress period has significant influence on the computation cost. The iteration and information exchange are responsible for the high computational cost.*". Please see Line 498~503.

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.

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 with a finite element method, 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 coupled model will be more obvious. In the revised manuscript, we add the calculation time of the real-world application. The simulation time of the real-world case is 120 s, which is efficient considering the scale of the problem.

The computational cost for case 1 is now listed in Table 2. The description about the computational cost comparison results with other models is added in the revised manuscript. Please see Lines 498~509. The description about the computational cost in the real-world application is in Lines 604-605.

Comment 17:

Line 464-465: the way ET_0 is computed for 2004 is not clear for me.

Response 17:

The description about ET_0 for 2004 is added as "*As lack of the weather data in 2004, the potential evapotranspiration* ET_0 *was calculated by the measured evaporation data from the 20 cm pan* (ET_{20})*, multiplying by an empirical conversion coefficient. The empirical coefficient is 0.55, which was recommended by Hao (2016) by comparing monthly* ET_0 *and* ET_{20} *with 8 years' data in this area.*". Please see Lines 534~538.

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. The subsurface system keeps changing. Therefore, it is hard to give a stress period which is linked to the time for the rainfall/irrigation water to reach the water table. So, in this case, we set a small stress period 5 d to eliminate its impact when considering the changing upper boundary condition.

Comments from Lele Shu:

Specific comments

Comment 1:

L362, Typo "Vaulin" should be "Vauclin".

Response 1:

We have corrected the error in the revised manuscript.

Comment 2:

L498-L502: "Figure 10 further shows... water table depth ... The increase trend is obviously found from Fig. 10(a) to Fig. 10(c) in the farm land, during which the groundwater was consumed by crop transpiration and soil evaporation".

Firstly, I suggest rephrasing to "... The increasing trend is obviously found in Fig. 10(a) to Fig. 10(c) in the farm land...", if what I discussed below is of misunderstanding.

The trend is not very obvious via the three maps. You may add the "maps of spatial GTD change" to make it more intuitive.

You explain that the "increasing" trend of farmland GTD resulting from crop consumption. Figure 9 shows the GTD increasing trend between 30d to 180d for all landuse types in observational data; water tables of farmland, village and bared land become deeper and deeper between 30d and 150d (the period from Fig 10(a) to Fig 10(c)) at the very similar magnitude, while the simulated results show different magnitude of decreasing water table. The water table of three landuse types increased after the autumn irrigation sharply, the model did not capture this trend accordingly. So I think the representative of landuse in the model is not competent to represent the characteristics of landuse, or issues from ET of different landuse, or the model configuration in MODFLOW did not capture the horizontal groundwater flow.

So I think the words in L504-505 "These results indicate that our model can reasonably simulate the saturated water table depth in space and time" is too strong.

I suggest the authors rephrasing these explanations.

Response 2:

Thanks for all the suggestions. There are 10 groundwater monitoring wells in this district, as shown in Fig. 8(a) in the current manuscript. Five wells are located in the farm land, two wells in the village, and three wells in the bared soil. In the previous version, the calculated results at the monitoring wells are averaged by the land type. As noted by the reviewer, it is not suitable because the averaged water table depth at the monitoring wells cannot represent the water table depth for different land use type due to different topography conditions. In the revised manuscript, we give the comparison results of the single well. Please see Fig. 10.

As inspired by the reviewer, the previous simulation failed to capture the increasing water table depth from 30d and 150d, which is caused by ET, especially in the bared soil and village. That is because that a very small evapotranspiration rate was given for village and bared land in the previous version, and we ignore that there are natural plants growing in the bared soil and village. Then in the revised manuscript, we recalculate the case with more appropriate upper boundary conditions. The actual soil evaporation and crop transpiration of village and bared land are recalculated according to the study of Yu et al. (2019) in this area. The simulation results fit the observations better in the revised manuscript. The description is added in Lines 561~578.

What's more, the calculation results of the coupled model at smaller time scales are discussed to show that the model can calculate the recharge rate. The descriptions are added in the revise manuscript as "*The recharge during short-term was calculated for further checking the results by comparing the results with those from reference papers. The calculated recharge in farm land during the autumn irrigation (from Oct 16 to Oct 31) is 93.3 mm, and the coefficient of recharge from the autumn irrigation is 0.37. Zhang (2011) proposed the coefficient of recharge from the autumn irrigation is approximately 0.3. Yang (2016) proposed that the coefficient of the recharge from the autumn irrigation is between 0.36 and 0.4. Yu (2017) used the coefficient of recharge from autumn irrigation as 0.33 for the district. The calculated result is consistent with the previous studies. The phreatic evaporation coefficient was estimated during the period from Sep 15 to Sep 30 with no precipitation or irrigation. The quantity of the recharge from saturated zone to unsaturated zone is 10.1 mm during the period in the farm land. The phreatic evaporation coefficient is 0.179, and the averaged water table* *depth is 1.51 m during the period. The phreatic evaporation coefficient measured by Wang (2002) is 0.172 at the depth of 1.5 m. The short-term results indicate the validity of the simulating results.*". Please see Line 579~592.

Yu, B., Shang, S., Zhu, W., Gentine, P., Cheng, Y.: Mapping daily evapotranspiration over a large irrigation district from MODIS data using a novel hybrid dual-source coupling model, Agr. Forst. Meteorol., 276-277, 2019.