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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "A Unique Vadose Zone Model for Shallow Aquifers: the Hetao Irrigation District, China" *by* Zhongyi Liu et al.

Anonymous Referee #1

Received and published: 17 December 2018

The manuscript presents a new modeling approach for simulating soil moisture in the Hetao irrigation district, China, based on the soil matric potential and height of the soil layer in relation to the groundwater table. The paper fits well within the scope of a journal like HESS but there are too many issues that need to better consider by authors before it can be published. My recommendation is that the paper be sent for Major Revision, with details given below.

Major comments: 1. The introduction needs to be revised. Authors divide models based on whether they are capable of solving the full Darcy's law or whether they follow only a simplified and regionalized solution. In my opinion, such classification is not very practical making the introduction section quite confusing. On one hand, authors group very distinct models such as fully distributed catchment models, plot scale va-

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dose zone models, and groundwater models as those based on the full solution of the Darcy's law (L82-84). On the other hand, semi-distributed catchment models are given as examples of those using simplified and regionalized solutions of the Darcy's law (L89-90). Authors should review the introduction section to focus only on similar models as theirs using comparable or alternative approaches for simulating soil moisture. 2. As a result of a confusing introduction section, it is not clear whether authors are trying to develop a model to be applied at the plot scale (which they are) or at the regional scale. Nothing is said about that in L114-118. 3. This is a clear misunderstanding of the evapotranspiration process throughout the paper, with authors referring many times simply as evaporation. Another example is given in L391 where authors refer to crop evapotranspiration (because then they refer to crop coefficients) as reference evaporation (?). 3. Soil water dynamics is pretty much dependent on soil evapotranspiration rates. However, there is nothing in the Material and Methods section describing how crop evapotranspiration is computed in the model or given as input. 4. The Material and Methods section does not detail about the approach used for calibrating/validating the model except for some vague sentence in L282-283. This information is critical and needs to be given. Not later in the results section (L385-387) when readers already gave up understanding what was done in the paper. 5. Authors apparently believe that groundwater dynamics is solely dependent on irrigation and evapotranspiration, and that groundwater flow and river connectivity are not relevant processes. This assumption seems to explain statements such as those in L328-336 which are obviously incorrect. The fact is that groundwater depth cannot be modeled using a 1D approach as in this paper, but only by considering the regional scale. Groundwater depth can only be considered as boundary condition for 1D simulations. 6. Authors assume an equilibrium between soil moisture and groundwater which does not happen in reality as themselves observed in L357-364. 7. The Conclusions section shows a brief summary of the paper, not its conclusions.

Additional comments: L49: Authors should explain why they feel water scarcity was ignored before in many parts of the world. By whom? Certainly not by population

living in those areas that have to deal daily with that problem; certainly not by the scientific community that has been addressing that problem for decades. L52: Authors give an estimate of 5100 m3 of available fresh water per capita by the year 2025. How much is it now? There is no point in advancing numbers for the future if they cannot be compared with some baseline. L56: Are these SI units? What does the "a" in "m3 a-1" stands for? Please check also other lines throughout the text (e.g. L127) L62-64: Authors should refer the environmental problems that resulted from the shallow irrigation water in Hetao, namely soil salinization risks and land degradation. L69-73: Authors should likely state that better management practices (new irrigation scheduling, alternative irrigation methods, and so on) are needed in the region. Otherwise, why the need for field trials and modeling? L74-77: One sentence does not make a paragraph. L83-84: The references for the HYDRUS and SWAP models were not given correctly. I'm sure authors of those models would appreciate seeing their work being recognized. If authors' intentions were to give applications in the Hetao region, they can be given below in the text. L92: What is the point of referring the computation method here? Are authors referring later to models using, for example, the finite volume method later? L93: The same as before. The correct reference of the HYDRUS-1D model was not given. Authors need to reword the text if their intention is to cite a modeling application. L94-96: I don't understand what authors are trying to say here. Apparently all models can be applied regardless the depth of the groundwater. L96-100: Models cited here apparently use a water bucket approach to simulate soil moisture. Is it correct? How do these fit in the model classification used in L78-79. L101-103: Why are those models not valid? Usually, water bucket approaches use empirical solutions to consider capillary rise. Couldn't those models be adapted by considering similar solutions? Apparently research in the region is quite extensive to be simply put aside. L103-107: I don't understand how the two models given here fit in the general scope of modeling research in the region. Some additional explanation should be given. L163: This should be "-33 kPa". L180: The particle size distribution is usually presented as percentage values, not fractions. L192: Equation 1 needs to

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be revised. Where is θ (volumetric moisture content) and θs (volumetric saturated soil moisture content)? This text seems to be extra here. L197: The text should say "For cases... when the flow is assumed to stop..." since flow never actually stops. L201: Please revise text as it makes little sense. L237-244: Authors intention here is likely to describe the role of evapotranspiration on model computation, not evaporation. Otherwise, the assumptions are completely wrong as evaporation rates are not maximum when the plant canopy is closed. Soil evaporation is limited by the amount of energy available at the soil surface during that period in conjunction with the energy consumed by transpiration. L238-239: How is the osmotic stress considered in the model? L288: I have some doubts on whether Ren et al. (2016) is the most appropriate reference for citing statistical indicators. Did those authors develop those indicators or at least elaborated on them? Or did they simply used them like here? Please revise. L290-293: Usually, the Nash and Sutcliff modeling efficiency test is also used to assess model performance. This test allows to understand whether the residuals variance is much smaller than the observed data variance, hence that the model predictions are good. Please include it in the analysis. L30-305: This text should likely be moved to the Material and Methods section. What is the relevance of including it here to the analysis of the results? L316: Figure 4 and 5 present something defined as additional irrigation. Please explain. It does not correspond to the irrigation events given in Table 2. Also, why is it not possible to distinguish between irrigation and rainfall? Both represented by green color and during the same day. Rainfall in Figure 4 does not seem to rainfall in Figure 2. L365: I'm not sure what authors are trying to say here. Please revise. L393: Which were the salinity levels in the field? L394-395: Allen et al. (1998) does not give Kc values for soils with median salinity. Please revise. L466-467: The EPIC model was already applied to simulate crop growth in the Hetao region. Those studies should be cited.

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