We would like to thank Reviewer 1 for the extensive and thoughtful comments. It is obvious from the review that we have a lot of work to do in clarifying the ideas in the manuscript. In this response, we will only touch on the major comments of the reviewer. We will provide at a later date a detailed response to all the comments. (The numbers below are the same as the numbers in the comment of the Reviewer 1)

1. The reviewer finds our introduction confusing and does not like our division of models in those who use Darcy's law and those that do not. In the revised manuscript we will amend the divisions of the models.

   However, in this initial response, we would like to clarify the background of our division of models. In our past modelling efforts, we have noted that that making the model more complex not necessarily gives a more accurate fit to the observed data (e.g., Hoang et al., 2018, Moges et al., 2017; Steenhuis et al., 2013; Johnson et al., 2003). We show in this research that for rolling and steep terrains during periods when the precipitation exceeds the potential evaporation (and the landscape wets up), the complexities in flow patterns organize themselves in a predictable pattern of moisture contents in the landscape. The wetness pattern is a function of the amount of water stored in the watershed. The current debate in the literature is whether Darcy's law and the conservation of mass (small scale physics according to Kirchner et al., 2006) can predict these wetness pattern or we simply can use these recurring patterns to predict runoff. We show (Steenhuis et al. 1993) for example that the runoff is linearly related with the precipitation after a threshold moisture content is exceeded. In more recent research in the Ethiopian highlands that dividing the watershed up in the periodically wet valley bottoms, degraded lands and permeable hillsides and keeping a water balance for each, we can predict the outflow more accurately than more complex models such as SWAT and HBV (Moges et al., 2017).

   In the present paper we found something similar where the moisture content distribution after a large irrigation event depends on the ground water depth until the groundwater cannot supply the water the evaporative demand of the plants. In the present manuscript we find that we do not need the conductivity of the soil to simulate the observed moisture contents. Hence also in this case only a general form of Darcy's law is needed to model the upward movement of water.

   We agree with the reviewer that we need improve the manuscript to clarify the model division and description. Thank you for your suggestion.

2. The reviewer asks if we are developing a field or plot scale model. We are developing a field scale model that is tested in a small part of the field. We do not have the sufficient data to the do the whole field. One of the interesting results is that the soil characteristic curves determine for a large part the moisture contents in the soil as a function of the ground water depth. Soil layering varies in the field and

so will be the moisture contents. Since our model uses the hydrologic equilibrium principle, our model remains valid at the field scale but will need information about the soil composition with depth to predict the moisture contents. Precise measurements of the moisture contents will require additional sampling. For any model to predict the spatial distribution of the moisture content with different depth will require these measurements. When average properties are taken, our and other model will predict average conditions. The next logical step in this research is to measure the soil characteristic curves with depth (and beyond the 90 cm in our current manuscript) at many locations in the fields and observe the moisture content and water table depth in the field.

3. The reviewer writes that there is "a clear misunderstanding of the evapotranspiration process throughout the paper, with authors referring many times simply as evaporation". The misunderstanding is not caused by faulty modeling of evaporation processes (some of us are modeling water balances for over 40 years!), but more likely related to the fact that we used the word "evaporation" instead of "evapotranspiration". In the current manuscript we have followed the recommendation of Savenije (2004) who points out shortcomings in measuring transpiration due to interception and dew forming of the plants. He writes in the conclusion of his paper

> "It may be clear that I would like the word evapotranspiration to disappear from the hydrological jargon. I propose that we use the much simpler and more correct word evaporation instead. I hope that my fellow hydrologists find these arguments convincing. If not, then I look forward to a continued debate."

   It looks like that we are continuing the debate. In the rewrite we will better define what we mean with evaporation and provide in Material and Methods part of the revised manuscript a detailed account of the method that was used to calculate the evapo(transpi)ration. We will be more precise with evapo(transpi)ration terminology in the revised manuscript.

4. The reviewer points that the approach used for calibrating and validating is not detailed in the Material and Method part. We agree with the reviewer and we will give more details about the calibrating and validating process in the revised manuscript. One year was uses for calibration and one year was used for validation. To make sure that sensible representation of the moisture content was obtained we calibrated the various part of the model separately. Thanks.

5. The reviewer writes that

> "the 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".

The reviewer is correct that the groundwater is a regional phenomenon. However, the regional flows might not be the main component of the groundwater flow since the experiment takes place in a plain with a hydrologic gradient between 0.1 and 0.25% (line 124). Assuming the hydraulic conductivity is 10 m/day (It is certainly less than that since the all the soils have a high clay and silt content). This would mean a water velocity less than 5 cm/day (assuming a porosity of 0.4). The field dimensions are approximately 40 by 90 m. Consequently, it will take much longer than a year (800 days) to travel across the shortest distance, Hence, our assumption that the dynamics in the vadose zone determines the groundwater depth seems reasonable.

In spite of the argument above, we write that irrigation in a nearby field affected the groundwater table in the beginning of growing season (lines 328-336).

> "In general, groundwater rose during an irrigation event and then decreased slowly due to upward movement of water to the plant roots to meet the transpiration demand. However, in the beginning of the growing season, we can see that the water table increased without an irrigation event. This occurred on Field A on June 24, 2016 and Fields C and D on June 20, 2017 (Fig. 5). This is curious and could be due to water originating from irrigation in a nearby field."

Our hypothesis is that early in the season the cracks in the structured clays were not fully closed and these could have transported some of the water across the field. These cracks close once the field is irrigated. It is not something that can be predicted by a standard finite difference or element model since the conductivity is so small for this site. So it is unexpected (or curious).

6. The reviewer writes that

> "Authors assume an equilibrium between soil moisture and groundwater which does not happen in reality as themselves observed in L357-364."

The reviewer's comment is really helpful because the text was wrong since we did not specify that we expect equilibrium between soil moisture and groundwater after an irrigation event that causes the groundwater to rise and thus the soil is above field capacity and the hydraulic conductivity is not limiting. This equilibrium will be maintained as long as the potential upward flux is greater than the evapo(transpi)ration demand of the atmosphere. Once the calculated upward flux is less than the root function determines from what layer the "unmet" evaporation is subtracted. Our apologies for the confusion.

Reaching equilibrium (or close to it) takes one or two days according to measurement moisture content data when the soil is wet. When the soil dries out reaching equilibrium will take much longer because the unsaturated hydraulic conductivity becomes very small but not zero.

7. The reviewer points that "The Conclusions section shows a brief summary of the paper, not its conclusions". We are grateful for this useful suggestion and we will modify this part in the revised manuscript.

We will address the remaining helpful comments of the reviewer at a later date since they do not fundamentally challenge to the conceptual and theoretical part of the manuscript. We are looking forward further discussion about these excellent and major comments of reviewer 1 and other concepts in the model.

References

Hoang L., Mukundan R., Moore KEB., Owens EM and Steenhuis TS: The effect of input data resolution and complexity on the uncertainty of hydrological predictions in a humid, vegetated watershed. Hydrol. Earth Syst. Sc., 22: 5947-5965. 2018. https:// doi.org/ 10.5194/hess-22-5947-2018.

Johnson, MS., Coon, WF, Mehta, VK., Steenhuis, TS., Brooks, ES., and Boll, J.: Application of Two Hydrologic Models with Different Runoff Mechanisms to a Hillslope Dominated Watershed in the Northeastern U.S.: A Comparison of HSPF and SMR. J. Hydrol. 284:57-76. 2003. https://doi.org/10.1016/j.jhydrol.2003.07.005.

Kirchner, JW.: Getting the right answers for the right reasons: linking measurements, analyses, and models to advance the science of hydrology. Water Resour. Res., 42: W03S04. 2006. https://doi.org/10.1029/2005WR004362.

Moges, MA., Schmitter, P., Tilahun, SA., Langan, S., Dagnew, DC., Akale, AT., Steenhuis, TS.: Suitability of Watershed Models to Predict Distributed Hydrologic Response in the Awramba Watershed in the Lake Tana basin. Land Degrad. Dev, 28 (4): 1386-1397. 2017. https://doi.org/2017. 10.1002/ldr.2608.

Savenije, HHG.: The importance of interception and why we should delete the term evapotranspiration from our vocabulary. Hydrol. Process.,18: 1507–1511. https://doi.org/2004. 10.1002/hyp.5563.

Steenhuis, TS., Hrncir, M., Poteau, D., Luna, EJR., Tilahun, SA., Caballero, LA., Guzman, CD., Stoof, CR ., Sanda, M., Yitaferu, B., Cislerova, M.: A saturated excess runoff pedotransfer function for vegetated watersheds. Vadose Zone J., 12 (4). 2013. https://doi.org/10.2136/vzj2013.03.0060.

Responders: Tammo Steenhuis, Zhongyi Liu and Zailin Huo