AUTHOR’S RESPONSE TO RC3:

Manuscript hess-2018-626 by Martinez-de la Torre & Miguez-Macho: “**Groundwater influence on soil moisture memory and land–atmosphere interactions in the Iberian Peninsula**”

This paper discusses the role the water table plays in the terrestrial water cycle through the provision of vertical fluxes it provides for crops to evapo-transpire. The authors apply a Land Surface Model LEAFHYDRO that also simulates the dynamics of water table. They present results that show the difference between the simulated soil moisture values with and without the inclusion of the water table. I think this paper addresses relevant scientific questions within the scope of HESS, it represents interesting tools and ideas; however, the presented methodology and data fall short from supporting the reached conclusions.
It is clear that significant work has been undertaken to produce the results; however, I think because the authors are dealing with many processes including land surface, unsaturated zone, and saturated processes, the paper as it stands lacks a lot of information that are necessary to convince the reader with the applied methodology and possibly the repeatability of the experiment. In addition, there are concerns related to the structure of the paper where introduction, results, and discussions are all mixed together.

Authors: Thanks for the reviewer’s complete assessment. We understand the issues pointed out by the reviewer and have introduced substantial editions and changes to the manuscript to address them. We discuss such changes in response to the reviewer’s specific comments below.

My points below make these comments clear:

At the beginning of the introduction, the authors state that “groundwater exchanges with the land surface occur via vertical fluxes through the water table surface, and horizontal water redistribution via gravity driven lateral flow”. The authors must be specific regarding the type of the lateral flows. Are these flows in the saturated zones only? Are they in the main aquifers or perched aquifers? Or do they also include what is called through flows, i.e. lateral movement of infiltrated due to the existence of low permeability materials above the water table?

Authors: In the introduction and the rest of the paper, we mean lateral flow *within the saturated zone,* as explained in the methodology section. We have added this to the text pointed out by the reviewer. The model does not represent lateral transport in the unsaturated zone.

The scale the authors are dealing with is a national scale. It is expected that many types of hydro-geological conditions will be met at this scale. It is not expected that they will deal with all possible hydro-geological settings, however, the paper must clearly state the selected hydro-geological condition the model is applied to. A diagram showing a conceptual model of this hydrogeological setting is needed. All the results to be presented and discussed has to be put always within the context of this conceptual model.

Authors: Yes, agreed, thank you. The original submission did not go into enough detail about the methodologies of the model. After this and the rest of reviewer´s comments, we have edited substantially the methodology section 2.1, adding information about how the model represents the hydro-geological conditions using the conductivity parameters. Please see the revised manuscript.

The introduction must be more focused. The paper states the aim of the paper in the first paragraph of the paper. The introduction then tries to explain the reasons for undertaking the work afterwards. I think the argument should be built the other way round. In addition the introduction includes description of the methodology applied (Page 3 Lines 5 to 12) and site description (paragraph starting from Line 20 on Page 3). I have difficulties with some of the definitions and terminology used. For example, on Line 34 Page 3, the authors write “reflecting the importance of groundwater memory”. Why do they need to call it memory? It is the groundwater storage that reduces the impact of extreme weather events. The use of positive and negative recharge is also confusing (although clearly defined) and not intuitive.

Authors: We fully agree and have edited the introduction section, stating the research questions and the particularities of our approach (Page 3 L5-12 of the original submission) at the end. The discussion about the Iberian Peninsula and its hydrological characteristics has been slightly modified, but we still think it should be part of the Introduction as it focuses the reader on the problems that the paper has to deal with.

Yes, the reviewer is right, we have rephrased "reflecting the importance of groundwater memory", it now reads "reflecting the importance of groundwater influence on surface hydrology".

About the use of positive and negative recharge, we acknowledge that the groundwater recharge is often referred to as the positive flux into the groundwater reservoir. In this work, we have followed the model signs for fluxes, as in upward is positive (like evapotranspiration from the surface) and downward is negative (like the water flux through the soil layers and then into the groundwater). We have changed the name of the flux to "net recharge" in the revised manuscript in order to clarify this point at different instances. This is now clarified the first time the net recharged is referred to in the manuscript in Section 2.1: "*The water flux through the water table or net recharge R is the sum of gravitational downward groundwater recharge and capillary flux, and depending on soil wetness and atmospheric demand, it can be downwards, causing the water table to rise, or upwards, causing the water table to deepen*"

Section 2 must be split into two sections one describing the study area including the information that are presented in the "Introduction", in addition to the conceptual model. The other section must be dedicated to the Methodology, which must include a lot more information than what is already presented. For example:

Equation 1 shows the temporal variations of groundwater storage as a response to recharge. What about the soil moisture temporal variations?

Authors: Information has been added in Section 2.1 about the flux calculations within the unsaturated zone, following the Richards' equation.

How does the model calculate evapotranspiration? Does it calculate runoff? Does it account for overland routing? Is overland water added to the groundwater flows emerging in the rivers to calculate total flows at the gauging station?

Authors: Information has been added at the end of Section 2.1 about the ET methodology.

Details on the river routing scheme and a sketch on the river parameters calculation have been added in Section 2.2.

The model does not calculate overland routing, but it does calculates surface runoff as infiltration excess, which is added to groundwater baseflow in the cell to calculate streamflow. Further details are part of the original model LEAF, described in the reference given as Walko et al., 2000.

How is the capillary flux calculated? Is it dependent on the position of the water table? (It is clear it is but at least it must be described in the methodology)

Authors: Details on this have been added to Section 2.1.

How capillary forces are presented in the model? When a water table exists, the water is available to evapo-transpire wherever the water table depth is?

Authors: Yes, regardless of the water table position, the vegetation has access to the soil water within the root zone depth. Of course if the water table is there, this means higher water availability for the plants.

It is not clear how the high resolution steady state simulation results are used in the low resolution time variant results (This is explained later, but what is mentioned in Section 2 is not enough to clarify this approach.

Authors: Yes, agreed. We have realized that Section 2.4 was not completely clear in the original submission. In the revised manuscript we have rewritten Section 2.4 to make it more explanatory.

It is stated that the shallow water table slows down drainage. If the soil is not fully saturated and the water does not pond on the surface, how the shallow water slows down drainage?

Authors: If soil moisture increases with depth when approaching the water table, as it is usually the case, capillary fluxes are upward. The always downward gravitational flux may dominate the net flux, but the latter is certainly smaller than when there is no groundwater. In the FD run, the net flux at the bottom of the soil columns is just the gravitational flux, with no upward capillary flux to counteract it, at least partially, thus drainage is faster. Furthermore, when the water table is within the resolved layers, drainage at 4m is zero, and if the water table reaches the surface, infiltration ceases altogether. In the FD run, drainage at 4m is always occurring when the bottom layer is above field capacity.

It must be explained here that rivers could be influent and effluent

Authors: It was very briefly explained in the original submission, refering to "gaining" and "losing" streams. After the reviewer suggestion, we have added the following information in Section 2.1:

"*This flux can occur as groundwater discharge (subsurface runoff) into gaining streams when the water table is above the river, sustaining stream baseflow, or as river infiltration into the groundwater reservoir in losing streams when the water table is below river bed. For gaining streams, LEAFHYDRO approach combines the physically based parameters of Darcy's law into a parameter called river conductance, commonly used in groundwater modeling literature, like the MODFLOW model (Harbaugh et al., 2000). Even though the river conductance is physically based and observable, detailed data on river geometry and bed sediments are lacking for the region studied, hence it needs to be parametrized. Such parametrization consists in a representation of the river conductance that includes two contributions; an equilibrium part, and a dynamic part that depends on the water table deviation from equilibrium at the time. Further details on this dynamic river conductance parametrization and discussion on its choice are found in Miguez-Macho et al. (2007). For losing streams, the distance of flow or river bed thickness in Eq. 10 is the same as the water table minus riverbed elevation difference (third parenthesis in Eq. 10, only with negative sign provided that wth < z_{r}), and hence these factors cancel out one another, leaving the flux calculation to be given by*

(new Eq. 11).

Therefore, the losing stream flux Q_{r} in the model is not dependant on the water table position, once the latter is below riverbed, but on the groundwater-rivers hydraulic connection."

Are the groundwater flows also driven using Darcy's law or is it based on hydraulic gradient only? What is the calibration procedure used to find the spatially distributed hydraulic conduct values?

Authors: Further explained in the revised version of the manuscript (Section 2.1)

Section 2.2 provides information about the source of data but no information about the data are provided. For example information about the spatial distribution of landuse is important to understand the amount of water extracted by evapo-transpiration from the soil store. Nothing is mentioned about the hydrogeological data used in the model such as the values of the hydraulic conductivity and storage coefficient of the aquifer, river bed conductance values, etc.

Authors: Thanks. We believe that with the inclusion of Equations 1 and 2 and the last paragraph about ET and PFTs in Section 2.1 , the model approach is clearer now. We have edited also the first paragraph in Section 2.2 as follows:

"*The 11 soil textural classes used in LEAFHYDRO, necessary to derive soil parameters in Eq. 2 controlling the vertical water fluxes, are defined by the United States Department of Agriculture (USDA) from fractions of silt, clay and sand. The data for top (0-0.30 m depth) and bottom (0.30-4 m depth) soil layers comes originally from the Food and Agricultural Organization of the United Nations (FAO) world database (http://fao.org/soils-portal/soil-survey). Other processes in the model, such as evapotranspiration, need parameters dependent on the vegetation type (PFTs) at the land surface. For vegetation type we usethe COordination of INformation on the Environment (CORINE) Land Cover Project database (EEA, 1994)*"

Details about lateral groundwater flow calculations and aquifer properties are also included as follows:

"Lateral groundwater flow Q_n is determined by the slope of the water table surface, applying Darcy's law the water flux from the n^{th} neighbour into a model cell is given by

$$Q_n = cT(wtd_n - wtd)/l$$

where c (m) is the flow cross-section connecting the cells, T ($m^2\ s^{-1}$) is the flow transmissivity between the cells, wtd and 30 wtd_n (m) are the water table depths for the centre cell and the n^{th} neighbour cell, respectively, and l (m) is the distance between cells. T is calculated as an integration of the lateral hydraulic conductivity at saturation, for which the model uses observedvalues of the anisotropy ratio relating vertical and lateral conductivities (Fan et al., 2007), and assumes exponential decay of the vertical hydraulic conductivity at saturation KV_f with depth, as

$$KV_f = K_0 exp(-z'/f)$$

where K_0 ($m\ s^{-1}$) is the known value at 1.5 m deep, z' (m) is the depth below 1.5 m and f (m) is the e-folding depth, 5 calculated as a function of terrain slope β as f = 75/(1 + 150β), where f is limited to 4 m when β ≥ 0.118. "

In Section 2.4, can you state please which groundwater model is used with the Mosaic LSM recharge model to calculate the initial EWTD? On Lines 10 to 18 (Page 6) it is unclear which model has the high resolution and which one has the low resolution. A diagram that shows the steps followed in methodology will be helpful. Text from Line 18 onward in this section are results. Why are they included in this section?

Authors: Section 2.4 has been edited as pointed out before. Please see the revised manuscript. Even though we agree that our EWTD is a result, we decided to include it at this point in the methodology section, since it is used as an initial condition for the main experiment.

In Section 3 the authors dip into discussing the validation of a model while no infor- mation about the hydraulic parameters used in the model are provided. These include parameters controlling overland, subsurface, and unsaturated flows as well as soil and landuse data. They claim that the temporal variabilities are reproduced. However, with the lack of the parameter values and the definition of the context (assumptions and conceptual model) within which the model is built, this conclusion is easily challenged.

Authors: Thanks. We again refer to the revised and more detailed new Section 2 that now includes discussions about all the required parameters in calculations.

In Section 4.1 (Lines 25 to 30 on Page 9), the authors define positive and negative recharge in an unintuitive way since in groundwater, recharge is referred to as inflow to the groundwater reservoir and the opposite is a discharge from the water store and that could be in any direction (like the upward capillary fluxes). The sentences on Lines 10 to 14 on Page 10 are not very well formulated and together with the comment above, it is difficult to understand the point the authors are trying to make. On Line 15, the argument "this cycle is more pronounced the shallower the water table" is not very strong since Figures 6c to f all show seasonal variations across the whole peninsula.

Authors: Yes, we have responded to this concern about the signs of the recharge flux above. The point we try to make in the referred lines is to differentiate between large areas of low positive flux and river valleys with high positive flux. We have slightly edited the sentences and we believe the point is clearer now:

"*However, in river valleys where steep slopes in the water table head drive strong local lateral groundwater flow convergence, groundwater-fed ET can exceed precipitation by large amounts, resulting in higher values for the positive recharge. This is apparent n Fig. 7a along the main river valleys crisscrossing the dry Mediterranean areas of the Iberian Peninsula.*"

We agree with the reviewer in that the point we made in Page 6 line 15 (original submission) is not sufficiently supported by the figure. We have deleted the sentence. The point about seasonality and the influence from shallow water tables is made in the following sentences. Thanks

In Section 4.3: can you please state how annual anomalies are calculated? Is it a difference from a long term average value or the difference from an average calculated on the day the anomaly is determined?

Authors: Anomalies are differences between the given year values and annual means in the simulations. It has been clarified in the revised manuscript.

Line 28 Page 11: are anomalies in precipitation and anomalies in soil moisture corre- lated or are the anomalies in soil moisture correlated with precipitation. Please clarify

Authors: The correlations are calculated between anomalies. In this case, anomalies in precipitation and anomalies in soil moisture. It has been clarified in the revised manuscript.

Section 4.4 Line 21: "water table depth (red lines)" are observed or simulated? If simulated is it from the model with water table or with free drainage?

Authors: It is the water table depth simulated in the WT run. The FD run does not simulate any water table. It has been clarified in the revised manuscript.

Figure 9: Please correct the caption for the left figure which should be related to the free drainage (FD)

Authors: Corrected. Thanks for spotting this.

In Figure 11, I expect the soil moisture anomalies calculated from the simulation with a water table to be lower in absolute value than those calculated from the simulation with free drainage. This appears to hold true for all hydrological years except Years 8 and 9 (Compare row 3 to row 2). Why?

Authors: The soil moisture anomalies when the water table is considered do not necessarily have to be smaller than in the FD simulation. In areas where the water table is shallow, while this is the case, it is true that variations are buffered. However, if a shallow water table deepens as a result of a prolonged drought, so that the connection with the top soil is lost, soil moisture anomalies are going to be larger than in a FD simulation. Soil moisture values in both runs would be similar, but the anomaly is going be larger in the WT run where the soil is typically wetter due to the presence of a shallow water table. This is what happens in years 8 and 9, after the drought. In the FD run, soil moisture anomalies rapidly follow those in climate. In the WT run, however, the water table has not fully recovered, and soils are still much more anomalously dry.

Finally, I think the paper has to include a Discussion section where the analysis of the results has to be aligned with the assumptions listed in the conceptual model together with the hydraulic characteristics of the studied domain and the landuse controlling the amount of evapotranspiration from the soil zone. While the amount of work that has been taken and presented must be recognised and appreciated, I think the addition of a discussion section and rewriting the conclusion section to address the main findings concisely will greatly improve the presentation of this work.

Authors: Yes, agreed. We have re-structured the paper, including a Discussion section after the results and a shorter conclusion section at the end. Please check the revised manuscript.