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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Groundwater influence on soil moisture memory and land–atmosphere interactions in the Iberian Peninsula" *by* Alberto Martínez-de la Torre and Gonzalo Miguez-Macho

Anonymous Referee #1

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General comments

This manuscript assesses the groundwater influence on soil moisture memory and land-atmosphere interactions in the Iberian Peninsula by using the LEAFHYDRO model. The simulation was performed at 2.5-km over the Iberian Peninsula for a 10 year period. The authors found significantly wetter soil and enhanced ET over shallow water table regions suggesting that groundwater might have an impact on climate over the Iberian Peninsula.

This study follows two previous studies carried out in United Sates (Miguez-Macho et al., 2007) and over the Amazon basin (Miguez-Macho and Fan, 2012) where the

same model was used to depict the influence of groundwater on soil moisture and atmospheric variables. The methodology and science questions of the present paper are very similar to these two previous papers but applied for the Iberian Peninsula. Regarding the main conclusion obtained from the present paper, most of them are consistent and confirm the findings in numerous previous studies, including the two previous studies using the LEAFHYDRO model. However, no significant new findings can be drawn from this modeling, hence the novelty cannot be said to be high. In my opinion, this is the first major issue of the paper. The authors should consider to better highlight what is the interest of using such high-resolution model over Spain with respect to the previous study using the same model (in United Sates and Amazonia). A reorganization of the introduction may help to better define the novelty introduced by the use of LEAFHYDRO over the Iberian Peninsula.

The paper is articulated in five sections: introduction, methodology, validation, results, and conclusion. Regarding this structure, I identified two general remarks that need to be solved. First, the methodology section do not give enough details on the model description and the data used. In my opinion, while the main purpose of the paper is related to groundwater-surface land relationships, this part is not enough detailed in the paper. Secondly, the results section introduces some elements of discussion that are not at all linked with the literature. No references are cited, neither in this results section, nor in the conclusion. Regarding the bunch of paper related to this subject (i.e. groundwater-soil moisture influence), the paper lacks of references. This is the second major issue.

Besides these general comments, I identified specific comments and technical errors in the text and in the figures that I put in comment in the subsequent sections. In particular, I wish to see all the figures wider, regarding the size of the simulated area.

Based on the above statement, I think major revisions are needed to solve the two previous major issues and the below specific comments and errors before the paper can be eventually published in HESS.

Specific comments

Generally speaking, and regarding the bunch of papers on the subject, the Introduction part lacks of references on land surface-atmosphere coupling and soil moisture memory influences on groundwater and atmosphere. As an example the following papers should be considered:

Maxwell, R. M., Lundquist, J. K., Mirocha, J. D., Smith, S. G., Woodward, C. S. and Tompson, A. F. B.: Development of a Coupled Groundwater–Atmosphere Model, Mon. Wea. Rev., 139(1), 96–116, doi:10.1175/2010MWR3392.1, 2010.

Vergnes, J.-P., Decharme, B. and Habets, F.: Introduction of groundwater capillary rises using subgrid spatial variability of topography into the ISBA land surface model, J. Geophys. Res. Atmos., 119(19), 2014JD021573, doi:10.1002/2014JD021573, 2014.

Page 2, line 4 to 5: This is the purpose of the paper. I suggest to move this part near the end of the introduction, after the definition of the science questions.

Page 3 line 21 to the end : “Here, we present a modelling study linking groundwater to soil moisture, land-atmosphere interactions and surface water” : You introduce the purpose of the paper in the first sentence and then explain why you chose your case study. To better highlight the subject of the paper and enhance the problematics that occurred in Spain and the opportunity to simulate groundwater and soil moisture at this scale, you should consider to move all this part before introducing the purpose of the paper.

Page 4 line 9: “Model description and settings”. This part describes the model and data used in the study. Most of the formulation of the model’s equations are described in (Miguez-Macho et al., 2007) and (Fan et al., 2007), so only the mass balance of the dynamic groundwater reservoir is given here. However, it could have been useful to have a description of how the water table head is calculated since it the main variable that is evaluated in the following sections. Information on how hydrodynamic parameters

(transmissivity and porosity) are taken into account in the model could be added.

Page 4 Line 27 – Page 5 line 7: The coupling of the water table and the soil layers is unclear. Why the layer B is added? This part needs more details on how the water content is computed.

Page 5 line 7-14 : This part lacks of details about the calculation of the river-groundwater exchanges. It is the so-called river conductance model used in MODFLOW? Are river heights variables (using Manning’s Formula) or prescribed? How are the river conductances determined?

Page 5, line 15 “2.2 Initial land and river parameters” Regarding the title, should it be “Land-surface and river parameters”? Generally speaking, this part lacks of many details about the parameters used for developing the model over the Iberian Peninsula. The authors should consider the following remarks and maybe add a Figure depicting the case study.

Page 5 line 16-20: Why the soil textural classes are needed in LEAFHYDRO? What is the dominant soil type/vegetation type?

Page 5 line 21-24: How does the river flow scheme work ? Does it used Manning’s Formula? How are the river widths determined? This part lacks of details for the Iberian Peninsula.

Page 6 line 4: Could you add details about the method used to disaggregate the IB02 data using the ERA-Interim precipitation data? This is not clear how the link between the two of them is described.

Page 6 line 3: You speak about the model grid without having define his resolution before (0.2° ?).

Page 6 line 12-17: What is the resolution of the model grid? How was the global climatic recharge at low resolution used? Was it disaggregated at a higher resolution? Is it an annual mean average over a period? How was the test run aggregated to the

model grid? This part is not clear.

Page 6 line 25-28: Much details are needed on how soil moisture is calculated in LEAFHYDRO. This remark is linked to the model description in section 2.1. Some details could be added to illustrate soil moisture.

Page 6, line 29: 10 year is a rather short period to validate the model. I know some water table characterized by multi-year annual cycles of 20 years. Could you explained why you choose this time period? What is the time step?

Page 7 line 24: “in order to rule out measurements in confined aquifers as much as possible”: does it means that you used some observations of confined aquifers to validate unconfined aquifers? It should be clarified.

Page 8 line 3 : “With regard to the observations, 203 of the studied stations present a shallow water table (wtd < 8 m) during the simulation period”: does it mean that the mean water table depth is lower than 8 m?

Page 8 line 10 : Do these 3 different observation sites in Point 15 grid cell belong to the same aquifer or maybe to different layers? Coarse spatial resolution is a factor that could explain these differences, but the different piezometers can also monitored different aquifers. It should be verified. The same remarks applied for the other points with several observations.

Page 8 line 14-line 17: The presentation of these percentage need to be clarified.

Page 8 line 17: “capturing the mean water table depth” : is it rather “capturing the water table depth time evolution”?

Page 8, line 26-27: this statement should be better connected with the results.

Page 10, line 14-16: Recharge mean annual cycles is linked to ET and precipitation mean annual cycles, but Figure 6 only shows the climatology of the recharge variable. Results for ET and precipitation should be mentioned here, maybe in the Figure, or

with some details in the text.

Page 10, line 19-23: “As the water table gets deeper”: does it correspond to the EWTD of Figure 2 ? Or a time evolution ? It must be clarified.

Page 10, line 22: ET evolution is mentioned but no Figure show it.

Page 11, line 24: Anomalies are computed with respect to the annual mean or the mean annual cycle ? It must be clarified in this subsection.

Page 12, line 29: The authors should add a sentence on the location of this region (reference to Figure 10).

Page 13 line 24: “but drainage is slowed down”. This result need to be reinforced with further results, maybe with a water budget or a time evolution of the recharge.

Page 14 line 15: “one year frequencies and at decadal timescales”. Decadal timescale appear on these power spectrum analysis, but I wonder the pertinence of finding decadal timescale with a 10-year time series. A period of at least 20 years would have been more appropriate.

Page 14, line 15-16: “The annual cycle, linked to that of the surface water balance”: Could you better explain this statement ? Maybe by linking it with previous results ?

Page 14, line 24-25 : “The higher weight of longer timescales of variation in the WT soil moisture series”: same remark as above. A 10-year simulation appear rather short to establish this result.

Page 14, line 28: For this section, Figure15 is not necessary since it is the same as Figure 5. You should consider to had the FD simulation in Figure 5 directly. Figure 15 could be replaced by a Figure showing the stream-groundwater exchanges in order to discuss this flux.

Page 23, Figure 2: The legend refers to EWTD and topographic data, but only one map is shown that corresponds to EWTD. Why describing topographic data here ?

This Figure could be wider and extend to the full wide of the page. Add a unit to the colorbar and a title.

Page 24, Figure 3: The authors describe a grid centered in the Iberian Peninsula. Figure 3 shows this peninsula, but also parts of France and North Africa. Could you add the limits of the simulated domain ? Are the France and North Africa part also simulated ? Generally speaking, Figure 3 should be reorganized to better highlight the results. A wider map centered on Spain could improve the reading. Using different color points for different information on the same map is confusing. I suggest to use different maps for the different informations (wtd, correlation, steep, number of station par cells) and grouping them into a single figure.

Page 25, Figure 4: Point 8 and Point 11: the model seems to overestimate the amplitude of the piezometric head evolution. It should be mentioned and explained.

Page 26, Figure 5: only correlation scores are given. The Nash-Sutcliffe (Nash and Sutcliffe, 1970) score could be used and commented in the text to quantify the quality of the simulation.

Page 29, Figure 7: add units for the maps and the y-axis of the bars. The (a), (b) and (c) letters need to be added to the titles.

Page 30, Figure 8: the authors should think to show the seasonality of ET in Figure 8, or elsewhere, as said earlier.

Page 34, Figure 12: add a title.

Page 37, Figure 15: suppress this Figure and add the FD simulation to Figure 5.

Technical corrections

Page 1, line 2 : “a key role” not “an key role”

Page 8, line 19 : “simulated time series” instead of “simulated series”

Page 13, line 1 : “for the large” instead of “for the the large”

References

Fan, Y., Miguez-Macho, G., Weaver, C. P., Walko, R. and Robock, A.: Incorporating water table dynamics in climate modeling: 1. Water table observations and equilibrium water table simulations, J. Geophys. Res., 112(D10), D10125, doi:10.1029/2006JD008111, 2007.

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