AUTHOR'S RESPONSE TO RC1:

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.

Authors: Thanks for this 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.

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 mem- ory 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.

Authors: Agreed. We have included the suggested references and others in the reviewed manuscript where they were relevant. Other references included:

Ying Fan, Gonzalo Miguez-Macho, Esteban G. Jobbágy, Robert B. Jackson, and Carlos Otero-Casal: Hydrologic regulation of plant rooting depth, PNAS 114 (40) 10572-10577, 2017 https://doi.org/10.1073/pnas.1712381114

Sobrino, J., Gómez, M., Jiménez-Muñoz, J., and Olioso, A.: Application of a simple algorithm to estimate daily evapotranspiration from NOAA-AVHRR images for the Iberian Peninsula, Remote Sensing of Environment, 110, 139–148, 2007.
https://doi.org/https://doi.org/10.1016/j.rse.2007.02.017

Westerhoff, R., White, P., and Miguez-Macho, G.: Application of an improved global-scale groundwater model for water table estimation across New Zealand, Hydrol. Earth Syst. Sci., 22, 6449-6472, https://doi.org/10.5194/hess-22-6449-2018, 2018.
https://www.hydrol-earth-syst-sci.net/22/6449/2018/

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.

Authors: Thanks. We agree that the characteristics of the region should be introduced first and then presenting what we have done for the Iberian Peninsula at the end of the introduction. We have deleted the mentioned sentences and included them as the last paragraph of the introduction:

"*In this paper, we present a modelling study linking groundwater to soil moisture, land-atmosphere interactions and surface water at the regional scale in the Iberian Peninsula. We investigate the role of groundwater in the hydrology of the region, focusing first, on its impact on soil moisture spatial variability, dynamics and long-term memory, second, on its effects on land-atmosphere ET fluxes, and third, on its direct impact on river flow.*"

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 MOD- FLOW? Are river heights variables (using Manning's Formula) or prescribed? How are the river conductances determined?

Authors: Thanks. Our initial approach was to not include too many details on the model formulations, but rather refer the reader to relevant literature where such formulations are described and focus on the results. But given this one and another reviewer's comment, we have realized that the manuscript needs some of these model details to be consistent, and therefore we have edited substantially the Methodology section in the reviewed manuscript, adding information that responds to the reviewer concerns.

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.

Authors: Thanks. The textural classes are needed to derive the parameters governing the vertical water flux through the soil layers. They also appear in the calculation of transmissivities for lateral groundwater flow. We have edited section 2.2 to clarify this and the rest of the reviewer's concerns, including a description of the methodology used to calculate the river parameters and a new Figure to follow this methodology.

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.

Authors: Yes, we have clarified this point in the reviewed version as follows:

"Once the daily precipitation is read and interpolated into the model grid, the model temporally disaggregates the daily values throughout the day using 3-hourly ERA-Interim precipitation distribution. Hence, the model uses the IB02 daily analysis data for bias-correction of daily totals and ERA-Interim data for precipitation distribution throughout the day."

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

Authors: This mention refers to the IB02 data resolution. After the new edition in section 2.2, the model grid resolution has been defined before this point.

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.

Authors: We agree that the explanation on how we calculated our initial EWTD was not clear enough. We have edited the text from lines 11 to 17. The second paragraph in section 2.4 answers the reviewer's questions now and reads as follows:

"We used topography data at high spatial resolution (9 arc seconds) in the EWTD calculation to properly capture topographic variability and local hillslope gradients (Gestal-Souto et al., 2010) A three-step process was followed, where first, a low resolution (1º) global climatic recharge from the Mosaic LSM was used to calculate a first estimate of EWTD by ingesting it to the 2D model using the high resolution topography; second, the resulting first high-resolution estimate of EWTD is simply aggregated to a grid of 2.5km to serve as initial water table condition for LEAFHYDRO full LSM 10-year test run (1989-1998), and third, a new high resolution EWTD was recalculated forcing the 2D model with the groundwater net recharge obtained with the LEAFHYDRO test run at 2.5 km and the high resolution topography. The test

run uses precipitation analysis and other forcings (see section 2.3) at higher resolution than the 1º climatic recharge from MOSAIC initially feeding the EWTD model, and produces a much more realistic recharge, totally compatible with our simulation settings"

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.

Authors: We have now added details about this in the first paragraph of section 2.1.

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?

Authors: The 10 year choice was a compromise between the computational capabilities at our disposal and the science issue of choosing a period long enough to include wet and dry years in order to study soil moisture and water table memory. The time resolution for resolving heat and water fluxes in the soil and at the land surface was 60 s. The time step for groundwater-stream exchange, groundwater mass balance and water table adjustment in the WT run is 900 s. We have included this information in Section 2.5.

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.

Authors: We used water table depth data from the IGME (*Institute of Geology and Mining of Spain*), several *Confederaciones Hidrológicas* (Spanish agencies managing
the main watersheds within the country) and the SNIRH (*National Information System for Hydrological Resources of Portugal*). We had no information about the confinement of the aquifers, hence we decided to neglect those stations with observations with wtd lower than 100 m.

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?

Authors: Yes, exactly. We have added the word "mean" in the reviewed manuscript to clarify this point.

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.

Authors: That is true, thanks for the remark. LEAFHYDRO can only resolve one water table per grid cell. If observations at one point come from different aquifers within the same column, as the reviewer point out, the "vertical design of the model" would be the difficulty we were trying to point out in this paragraph, as well as the coarse resolution. We have added this point to the manuscript.

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

Authors: Agreed. We have tried to clarify, as:

"*Approximately one third of the stations present a shallow mean wtd (< 8 m), and 66.0 % of them are also found to have shallow mean water table by the model.*

In terms of mean wtd error, 14.0 % of stations present less than 2 m difference between simulated and observed mean wtd at the available observation times *(red points in Fig. X. If we only consider shallow water table observations (mean wtd < 8 m), 33.0 % of them present less than 2 m difference with the mean simulated wtd.*"

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

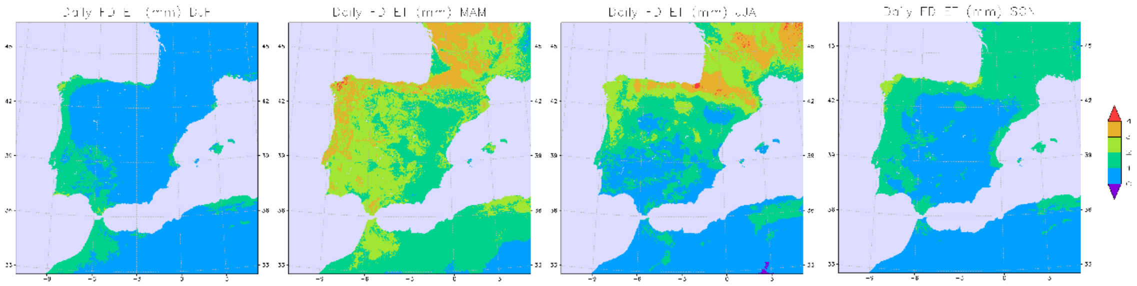
Authors: We meant "capturing the mean water table depth" here, for points 1-12. Then in the next sentences we talk about time evolution and mention points 1-14, therefore points 1-12 are in both red (capturing mean wtd) and green (capturing time evolution) categories.

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

Authors: The following results section focused on the shallower water table points or regions, where the groundwater is connected to the top-soil hydrology. With this statement we aimed to summarize some of the figures presented in the previous paragraphs, particularly the percentages that were discussed two responses above.

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.

Authors: We have seasonal plots of the evapotranspiration fluxes in the model:



However, we decided not to include them, as we did not intend to include too many figures. We have added some text about the ET cycle in the manuscript: "*The seasonal character of ET in the Iberian Peninsula (Sobrino et al, 2007) is induced by water availability and incoming radiation; maximum values and higher spatial variability are found in spring and summer, whereas minimum values and variability appear in autumn and winter, when the incoming radiation is lower and the leaf area index decreases*". The Sobrino (2007) reference presents ET seasonal patterns using NOAA satellite images for the Iberian Peninsula, in agreement with the LEAFHYDRO patterns shown here. Of course we can add the figures as supplementary material if the reviewer see it necessary.

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.

Authors: Yes, thanks for spotting this. We meant "Where the water table is deeper (Fig. 2)", and have corrected it in the reviewed manuscript.

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

Authors: See response 2 comments above.

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.

Authors: With respect to the annual mean. It has been clarified in the revised manuscript.

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

Authors: Done in revised manuscript. Thank you.

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.

Authors: We meant that drainage is slowed down in comparison with the FD run, as there are no upward capillary fluxes from below the top soil column or shallow water tables in free-drain approach. Drainage should then be faster than with the presence of a water table, at least in the regions where the water table is shallow. We have edited slightly the sentence as:

"During the wet season (autumn-winter) the water table rises due to precipitation infiltration, but since drainage is slowed down as compared with the free-draining FD run, the soil moisture difference between both experiments also follows an upward trend"

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.

Authors: Yes, of course. Thanks for pointing this out. Decadal timescale results are not relevant in this analysis. We have deleted the comment.

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?

Authors: We were referring to the cycle apparent in the previous Fig. 13 (on ET and soil moisture). We have added this reference in the revised manuscript.

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.

Authors: The statement is still true for most basins at lower frequencies, particularly the Mediterranean basins at around 3yr frequencies.

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.

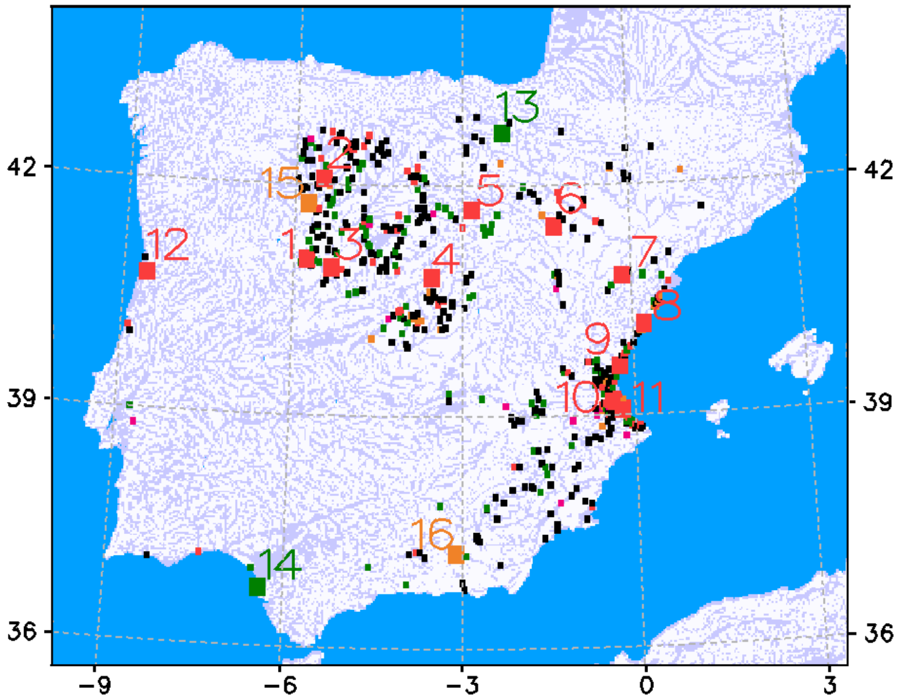
Authors: We believe after consideration that having the 2 figures makes it easier for the reader. We present in Fig 5 the streamflow comparison for WT run only as this is the flow we obtain as output of our main simulation we extract conclusions from. We then present fig. 15 (now 16) when we study the main basins, focusing on the differences between WT and FD representations of river flow.

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.

Authors: The topography data was used to calculate EWTD as a balance between the climatic recharge and the lateral flow driven by topography, using the 2-D model in Fan et al (2007), as explained in section 2.4. Therefore, the original resolution of the EWTD data shown in the figure is the resolution of the used topography, we have clarified this point in the caption in the revised manuscript. We have also widen the figure and added the units to the colourbar.

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.

Authors: Yes, the portions of France and North Africa in the figure are also part of the domain. But the reviewer is right in that the study is not about them, and particularly for this figure, their inclusion makes the information harder to read. We have cropped the figure to focus it on the peninsula, and made it wider in the manuscript, so that the information can be clearer seen. Still, we have decided to keep the colour code to avoid presenting too many maps. We believe that it can be better understood now:



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.

Authors: We have included in the revised text that at these very shallow water table points the amplitude of the wtd variations are larger in the model than in the observations.

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.

Authors: There are two important issues related to streamflow in these simulations. They are discussed in section 3.2 in the paper, but perhaps they need further clarification. The first one is related to the precipitation forcing data. From figure 5 it is obvious that there is a large amount of missing water in the model results. Only basin 5 (Guadalquivir) shows less streamflow in the observations than in the model, but this is because in this strongly regulated basin, water is heavily used for irrigation. While there can be some errors due to evaporation biases, we have evidence from local independent observation networks that this missing water is more related to the precipitation forcing (please, see the discussion about the same bias in the IB02 dataset in Rios-Entenza and Miguez-Macho, 2014). In the mountains, especially in the north, the IB02 dataset does not properly capture orographic enhancement, since it was obtained using simple interpolation algorithms.

The second problem is due to model parameterizations and is also commented in the paper. Surface runoff from excess saturation in thin soil or in subgrid near saturated areas is unrepresented. Due to unresolving hillslope hydrologic gradients at the 2.5km resolution, the connection between rivers and groundwater in cells where the mean water table is deep does not produce a good result either.

Since both forcing and model problems affect mostly mountain areas where terrain is complex, we are confident that the main conclusions in our work about groundwater and soil moisture are sound. However, we cannot say the same about riverflow, since the contribution from the mountainous areas to their total water budget is very important.

We have now calculated other skill scores for both experiments, as suggested by the reviewers. In the FD simulation, the lack of surface runoff is compensated by the fact that infiltration is readily incorporated into the rivers. Because precipitation amounts are biased low, winter peaks may look better in this FD simulation and some skill scores are better than in the WT simulation, but this does not mean that the result is physically correct.

Station - Basin	Basin Catchment Area (km^2)	E WT	r WT	r_{mm} WT	E FD	r FD	r_{mm} FD
Foz de Mouro - MIÑO	15.407	-0.13	0.89	0.98	0.55	0.93	0.95
Puentepino - DUERO	63.160	-0.52	0.73	0.96	0.18	0.72	0.69
Almourol - TAJO	67.482	0.28	0.91	0.93	0.80	0.91	0.89
Pulo do Lobo - GUADIANA	61.885	0.07	0.65	0.71	0.21	0.54	0.66
Cantillana - GUADALQUIVIR	44.871	0.44	0.76	0.66	0.15	0.56	0.67
Tortosa - EBRO	84.230	0.36	0.74	0.93	0.55	0.82	0.87

For all the aforementioned reasons, we purposely wanted to limit our discussion about streamflow in the paper and just show the WT results. The only point that we wanted to make with the FD simulation is that in the Mediterranean climate of the Iberian Peninsula, summer stream flow is sustained by groundwater and, without it, in a simulation with a free drain approach, rivers dry out. We are confident that this result holds true, despite all the problems in the forcing and model parameterizations. We show the Ebro basin to illustrate this point because it is the one showing less streamflow total annual underestimation and annual cycle better matching observations in the control WT run, especially in winter. It so happens that it is also the largest basin the Peninsula, so the example is significant.

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.

Authors: Ok. Done in revised manuscript.

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

Authors: Replied earlier. Thanks.

Page 34, Figure 12: add a title.

Authors: Ok. Done in revised manuscript.

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

Authors: Responded above

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

Authors: All technical corrections have been included in the revised manuscript. Thanks.