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Interactive comment on "Eco-hydrological effects of stream-aquifer water interaction: A case study of the Heihe River Basin, northwestern China" *by* Y. Zeng et al.

Anonymous Referee #2

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Title: Eco-hydrological effects of stream-aquifer water interaction: A case study of the Heihe River Basin, northwestern China Authors: Zeng et al.

Summary: This work presents the impact of river network-aquifer interaction on both sides of the river network using 1D lateral groundwater model. On both sides of the river network groundwater exchange is simulated over a region of 3 km using a pixel resolution of 60 m. For each pixel the vertical column response is simulated using the CLM4.5 model. Results show that the river network has an impact on the saturated and unsaturated zone dynamics in close vicinity of the river network. These variations have an impact on the water, energy and ecological properties of these grid cell.

Overall quality: Reading the title and abstract of this manuscript I was quite enthusiastic

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about the content of this work. However, after thoroughly reading the rest of this work I ended up feeling rather disappointed. The authors basically show that incorporating river-groundwater interactions has an impact on the water table and unsaturated zone dynamics. And these variations have impact on the carbon and energy fluxes. As such, the message presented in the abstract does not correspond well with the content of the manuscript. In my comments below I have tried to provide some more detailed information on how to improve this discrepancy. Furthermore, I need to stress the important equations as given by Eqs. 6-8 seem mathematically incorrect (see below). I would like to ask the authors to make sure that these are just typos and that the model was correctly implemented. If this is not the case, the simulations performed in this work need to be redone. That being said, the overall results presented in this work are fine and fit within the scope of HESS. Therefore, in its current form I recommend major changes. These changes are mainly related to the textual content of the manuscript.

General comments:

1) Page 1, lines 15-16 and page 16, line 1 states that stream aquifer interaction processes were incorporated into CLM4.5. I do not agree with this statement. From what I understand from the modelling set up, based on reading the paper, the authors have simulated the hydrological response of 50 pixels of each 60 m wide on both sides of the river and simulated the vertical response of each pixel using CLM 4.5. Furthermore, the response of the river network is not explicitly simulated using CLM4.5 but is externally forced in the model. In the current version for of the manuscript, the authors give the impression as if a major addition was added to the model. I do not believe that this the case while reading the paper. The authors only present 1 dimensional lateral groundwater exchange model, which obtains its water level estimate from CLM4.5.

2) Page 4, lines 11. Generally CLM4.5 is use for large-scale simulation (global/continental) using relatively coarse grid resolution (about 0.1-1 degree). Furthermore, these simulations usually make use of a 2D lateral grid structure, even though the official version of CLM4.5 does not explicitly represent lateral groundwater

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flow, but instead the lateral groundwater flux (as estimated using a non-linear reservoir model) is directly moved into the river network. Given this difference in the official version set up and the set up used here (see also previous point) I would suggest to add a section between 2.1 and 2.2 which shows the 1D lateral grid set up up (on both sides of the river network using a high pixel resolution) used here. This will really help improve the readability of the manuscript. E.g. it will then become much easier to understand page 4, lines 9-18.

3) Page 3-4, Eqs. 1-4. The authors present here presents the 1-dimensional lateral groundwater flow equation here with a flexible downstream head boundary condition (i.e. the river network). This model is used to simulated the groundwater response on CLM. In the original version of CLM4.5 a non-linear groundwater reservoir model is used. However, in the manuscript no information is provided, whether this original model was removed in the setup of the authors? Please provide some additional details here (see also comments below).

4) Page 5, line 6 Change "i.e. water . . . 3.8m" to "i.e. water table lies within 3.8m from surface."

5) Page 5, Eq. 6. There is know information provided on what T1 and T2 indicate?

6) Page 5, Eq. 7. Mathematically this is incorrect as the transmissivity is obtained from the groundwater level up to the depth of the bedrock. The summation should therefore not include all 10 layers. Instead if the groundwater level lies within layer i: (z_wt – z_(i,bot))*K_i + summation from layer j=i+1 till layer j=10 of (delta z_j*K_j). Where z_wt is the depth of the groundwater table (Eq. 5) and z_(i,bot) is the bottom level of layer i.

7) Page 5, Eq. 8. After this equation please add: "where, z' = z-3.8.

8) Page 8, line 4-5. The manuscript states that an initial spin-up of 700 years was conducted using the original CLM4.5 model. So without groundwater exchange. This looks very impressive but seems very redundant as well.. Given the resolution of the CLDAS

dataset (0.0625 degrees corresponding to 7.5km), means that all 50 cells on each side of the river receive the same type of input. Without accounting for lateral exchange, basically means that they all give the same results, indicating that the simulations can be performed using a single pixel. I cannot believe that one needs 700 years of spin-up simulations to reach some kind of equilibrium groundwater level. Please provide more information here why this was performed.

9) Page 8, line 15. Please add a line indicating that for the river cell in the middle, no simulations with CLM4.5 were performed. But instead a boundary condition was enforced here.

10) Page 8, lines 12-20. For each of the 50 pixels on both sides of the channel network, did the authors consider elevation variations between the pixels?

11) Page 8, lines 12-20. It is not clear how the control simulations where implemented? Lines 13-15 state the these do not take stream-aquifer interaction into account. It is not clear whether these simulations do account fo lateral groundwater exchange (Eq. 1-4) and how groundwater is removed into the river network (was there some additional boundary condition used?). The results in this manuscript show that there are considerable differences between the CTL and TEST simulations. However, as the current manuscript does not provide much info on how CTL was implemented, it is currently unknown whether how important these difference are (or whether is it just related to the set up of the model).

12) Page 8, line 19. What is the resolution of the MICLCover land cover map used here?

13) Page 8-9, sensitivity experiment 1 and 2.. On page 9, lines 12-13 it is mention that the groundwater table variations are not sensitive to k_r. By directly comparing the chosen values of k_r with those of the saturated lateral hydraulic conductivity value of the surrounding soils Ksoil, one could already made a first impression whether this would have a impact. In case k_r is much larger than Ksoil (as I expect to be the case

here), I do not see a reason why to perform this experiment. As these results were to be expected. Therefore, I would remove these results from the manuscript (this will also reduce the number of figures presented in this work, which is rather large).

14) Page 9, lines 1-2. See comment #8.

15) Page 10, lines 1-3. In my opinion these results just show that the model correctly adjust to changes in the observed surface temperature.

16) Page 10, line 6. Change "good ability" to "reasonable ability".

17) Page 10, lines 9-18 and Fig. 6. The results presented in this figure are heavily depend on the local surface elevation enforced in the model. I would therefore suggest the rescale and plot the difference as with respect to the local surface elevation (y-axes) as function of distance from the channel network (x-axis). This helps to improve the interpretation of this figure.

18) Fig. 7. Note that the legend is a dashed line, while this is not shown in any of the panels.

19) Page 10, lines 17-18. Please mention that the quality of these results are directly influenced by the chosen saturated hydraulic conductivity values, which in this study were chosen a priori and as such not optimized in any kind of manner.

20) Page 12, line 1. Please add ", see Section 4.2.2." after "from a stream".

21) Page 17, lines 14-15. Please remove this statement from the manuscript. This work presents a theoretical study using an extremely high pixel resolution in the direction perpendicular to the river network. Such, resolutions at large scale are infeasible. Even if this would be possible, such an implementation would need many additional model changes not accounted for in the model set up presented in this work.

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