**Reviewer #2:**

**We thank the reviewer 2 for the helpful comments and suggestions, which are in plain text below. Our response is in bold text.**

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 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.

**Response: Thanks for the comments. As the suggestion, we changed the way of expression from “incorporating stream-aquifer interaction scheme to CLM and model development” to “combining the two models to investigate the effects of stream-aquifer water interaction” in the abstract, introduction and throughout the manuscript.**

**In fact, the two models are two-way coupled. That means, besides the presenting of one-dimensional lateral groundwater exchange model which obtains its water level (and some other parameters) estimate from CLM4.5, we also modified the simulated groundwater table and aquifer water storage of CLM4.5 based on the output of the lateral groundwater exchange model. We took advantages of both the models. It is not a major modification to CLM but can be seen as a convenient and effective way to achieve our scientific goals.**

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 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.

**Response: Thanks for the comments. As the suggestion, we added a section of “Configuration of CLM4.5 for simulation over riverbank” (page 4, line 8-page 5, line 2) to introduce how we set up the model and prepared the surface dataset which is the most important in riverbank simulation. Furthermore, the subsurface runoff scheme in CLM4.5 was turned off because it was not suitable in the fine-scale modeling and replaced by the groundwater lateral flow in stream-aquifer interaction scheme, which was the explicit representation of the subsurface process (page 4, line 23-page 5, line 2).**

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).

**Response: Thanks for the comments. The flexible downstream head boundary condition** **was only used when running the stream-aquifer water interaction module and did not directly connect to CLM4.5.** **All the vertical biogeophysical and biogeochemical processes of CLM4.5 was retained because they were not scale-dependent and could be used in any resolution if the corresponding surface dataset was set properly. To the non-linear groundwater reservoir model of original CLM4.5, the vertical water exchange scheme between soil and aquifer was not modified. However, as referred above, the subsurface runoff of the original CLM4.5 was turned off in the model because it was not fit for the fine-scale modeling and was replaced by our lateral groundwater exchange model. The Relevant discussions were added in the new section 2.2 of “Configuration of CLM4.5 for simulation over riverbank” in the manuscript (page 4, line 8-page 5, line 2).**

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

**Response: Thanks for the comments. We modified the sentence as the suggestion.**

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

**Response: Thanks for the comments. We added related information about *T1* and *T2* in the appropriate position (page 7, lines 2-3).**

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.

**Response: Thanks for the comments. We are sorry for this mistake and corrected it in the manuscript (page 8, line 9). We ensure that it is only a slip of typing. The model code is correct.**

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

**Response: Thanks for the comments. We added the sentence as the suggestion.**

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.

**Response: Thanks for the comments. Although the resolution of atmospheric forcing dataset is coarse, the topographic, land cover, soil datasets for making CLM surface dataset are fine (ASTER Dem Dataset with 30-m, MICLCover with 1-km, HiWATER Land Cover Map with 30-m and China Soil Characteristics Dataset with 1-km). So we think it is necessary for the spin-up over all grids. The choice of 700 “spin-up” years was based on the user’s guide of CLM (Chapter 4 of Kluzek 2013) showing that when the biogeochemistry carbon-nitrogen module of CLM is turned on (it is the case of this study), the model should be at least run for 700 years to get a steady state because the magnitudes of carbon and nitrogen fluxes are very small (Oleson et al. 2013). The discussion above was added in the manuscript (page 10, lines 15-18; page 11, lines 11-19).**

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.

**Response: Thanks for the comments. We added the sentence as the suggestion.**

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?

**Response: Thanks for the comments. Certainly we took the elevation variations into consideration, for it is the major control of the groundwater lateral flow. We got the high-resolution elevation data from ASTER Dem Dataset with 30-m resolution.**

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 for 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).

**Response: Thanks for the comments. The control simulations took the groundwater lateral flow into account because in this study we focused on the effects of stream-aquifer water interaction, but not the groundwater lateral flow. The only difference of CTL from TEST is that the water exchange between stream and aquifer was set to zero (flexible boundary condition). We added this information into the manuscript (page 11, lines 5-7).**

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

**Response: Thanks for the comments. The resolution of MICLCover land cover is 1-km. The divide of land cover types of MICLCover is similar to CLM. However we also referred the HiWATER Land Cover Map (30-m resolution) when making the surface dataset. We added this information in the manuscript (page 11, lines 11-19).**

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).

**Response: Thanks for the comments. It is right that *k\_r* is several times larger than *Ksoil*. However, the *k\_r* is still matter when the simulation time is short. To show this (as well as to meet the comments from another reviewer), we added the results from short-term (7 days) sensitivity experiments in the Figure 4a-4d and 4i-4l. They revealed that the river bed water conductivity is more important in the controlling of short-term water table variation than the controlling of long-term water table equilibrium. The discussion was added in the manuscript (page 12, lines 5-25).**

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

**Response: Thanks for the comments. The reason of 700 years spin-up was explained in the response to 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.

**Response: Thanks for the comments. As the suggestion, we changed the sentence of the explanation for Figure 6a.**

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

**Response: Thanks for the comments. We changed the words as the suggestion.**

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 (yaxes) as function of distance from the channel network (x-axis). This helps to improve the interpretation of this figure.

**Response: Thanks for the comments. We plotted the figure (Figure 7c) as the review’s suggestion over the Gaotai Bridge where most water wells were displayed.**

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

**Response: Thanks for the comments. We modified the Figure 8 to make it clearer.**

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.

**Response: Thanks for the comments. As the suggestion, we added this statement in the manuscript (page 14, lines 11-14).**

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

**Response: Thanks for the comments. As the suggestion, we changed the words as the suggestion.**

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.

**Response: Thanks for the comments. We deleted these statements as the suggestion. Maybe in the future we can summarize the findings over the high resolution pixels and come up with some parameterizations to make CLM being able to simulate the effects of stream-aquifer water interaction over large-scale. Thank you!**