

The manuscript “Application of a new distributed hydrological model based on soil-gravel structure in the Niyang River Basin, Qinghai-Tibet Plateau” applied a new model which considered the impact of gravel on water and heat transfer, as well as the snow cover in the study region. This work was necessary for the study region which has soils with large portion of gravel to a certain depth. However, when I read the manuscript, I felt that the work needs some major changes in order to make it clearer. The authors developed both the soil and snow processes, but they did not show how each process have improved the results. Besides, the description on the results are quite subjective and I did not see confident quantitative descriptions in multiple places, I will put the specific comments below. In general, I suggest a substantial revision to make this work more attractive and interesting to readers.

Dear Reviewer:

We appreciate the detailed and valuable comments, which have considerably improved the quality of our manuscript. Our responses to your comments are provided below.

Specific comments:

1. Line 63-70: I like this paragraph about the soil formation of QTP, but the position of this paragraph can be moved upward before introducing the gravel content impacts on soil heat transfer.

Reply: Thank you for this suggestion. We will revise this paragraph as you suggest.

2. Line 76-79: I think this sentence is repeated as it was already mentioned above.

Reply: Thanks for pointing this out, we'll revise this section by remove the repetition.

3. Line 79: how to adjust parameters? I think you mean calibration, but the calibration always depends on the function as the goal, i.e. soil temperature from surface or from lower layer, and/or soil water simulation accuracy.

Reply: Yes, it is parameter calibration. For the water and heat simulation of the underlying surface in the hydrological model, with the goal of improving the accuracy of soil water and soil temperature simulation, the hydrothermal parameters of the underlying surface were calibrated. In the revised version, we will clarify this part.

4. So I would suggest the authors to be more direct on demonstration of what you focus and why it is important. This would connect the whole storyline of this manuscript. Otherwise, I would not think you tell a good story on your work and the importance of it would be heavily lowered.

Reply: Thank you for your constructive feedback. We will modify the structure of the Introduction in the revised version, as you suggested, to help readers better understand the importance of our work.

5. Line 84: I did not see the impacts of rain intensity in your introduction.

Reply: We are sorry that we failed to make this clear enough in the previously submitted version. The runoff generation mechanism is different between the non-heavy and heavy rain scenarios. The non-heavy rain scenario indicates saturation-excess, and the heavy rain scenario indicates infiltration-excess. Therefore, in the WEP model, the runoff generation process was divided into two scenarios. In the revised version, we will clarify this part.

6. Line 143: to avoid

Reply: Thanks, we will correct it and check for other potential errors.

7. Line 154: what is “and0”?

Reply: The “0” was redundant. We will remove it and check for other potential errors.

8. Line 181-182: this is repeated.

Reply: Thanks for pointing this out, we'll remove the repetition.

9. Line 349-350: how were they determined?

Reply: These parameters were calibrated with actual measurement.

10. Line 420: Figure 8, again, interestingly, the model was good for frozen period because liquid water was very low, but during the thawing period, i.e. March and April, the model starts showing large discrepancy from observations, which I am curious about the possible causes.

Reply: The soil moisture simulation results of the WEP-QTP model in March and April were indeed not satisfactory, but there is still a clear advantage compared to the WEP-COR model. The lower accuracy in simulation can be attributed to soil inclusions within the gravel layers. Gravel layers are not homogeneous, but they were generalized in the model as a set of unified parameters; hence, there is a discrepancy in the model simulation results.

11. I just wonder the improvement is more about the hydraulic process than the thermal process? I can not agree that soil temperature was obviously improved from Figure 7 as the dynamics is still not well represented by the WEP-QTP model.

Reply: The improvement of the heat transfer process includes two aspects: one is to consider the thermal insulation effect of snow, and the other is to consider the thermodynamic parameters difference between gravel and soil. The addition of snow mainly reduces the temperature fluctuation in the surface layer, while the addition of gravel affects the heat conduction in the underlying layer. Impacted by the heterogeneity of the gravel layer, the improved model shows a discrepancy from the observations, but it is closer to the observations than the WEP-COR model.

12. Section 3.3: these are not quantitative and a little subjective. I would like to see how the new model is better and with some quantitative results.

Reply: Thanks for the comment and suggestion. In the revised version, we will add the discussion of the quantitative results in this section 3.3 to help readers understand the model improvements.

13. Line 457: I am wondering, how you compare the new model with the old model if it was changed in different processes, as you have developed the gravel related processes, and now you have a new snow model. I will be confused on what caused the differences in results.

Reply: By analyzing the sensitivity of snow and gravel layers to temperature simulation, we found that when only considering the influence of the thermodynamic parameters of gravel, the average RE was -4.38%, which is close to the average RE of WEP-COR (-3.6%). When only the insulation effect of snow was considered, the average RE was 0.70%, which is close to the average RE of WEP-QTP (0.08%). Therefore, snow is the main factor affecting the temporal and spatial variation of permafrost.

14. Conclusions: I am not so confident on the results for frozen period in the whole manuscript as I mentioned above, the snow and soil processes were both developed and I did not see the impact from one single process. I therefore additional sensitivity work should be done to quantitatively show how each process affects the results.

Reply: Thank you for your professional comment. As mentioned in the reply to question 13, we will add additional sensitivity analysis in the revised version to quantitatively show how each process impacts the simulation results. In addition, we will focus on revising the discussion section based on your comments above to enhance the persuasiveness of our conclusions.