

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2022-215

Anonymous Referee #2

Referee comment on "Application of a new distributed hydrological model based on soil-gravel structure in the Niyang River Basin, Qinghai-Tibet Plateau" by Pengxiang Wang et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-215-RC2>, 2022

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.

Specific comments:

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.

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

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.

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...

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

Line 143: to avoid

Line 154: what is "and0"?

Line 181-182: this is repeated...

Line 349-350: how were they determined?

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...

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...

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.

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.

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.