General comment

I am not convinced by the answers brought to my points. To summarize, I had and still have three main complaints (that I also detail below the general comment).

First, the revised abstract still says : “The Qinghai-Tibet Plateau (QTP) has a thin soil layer on top of a thick soil-gravel mixture (SGM) layer.” And this still makes no sense to me and I am still waiting for references which prove that I am wrong and that a significant portion of the QTP has indeed this stratigraphy. As developed below, it seems more reasonable to me to say that it is a relevant simplification for the present watershed based on the field observations of the authors. The conclusion of the manuscript is more cautious in this regard and I like better how it presents the study content than the abstract.

Second, the model description is still very puzzling. The authors confused avalanches and snow melt but most importantly, the surface energy balance description disappeared to the benefit of a peculiar new equation 7 that only considers sensible heat fluxes and in a very strange way that is not supported by the provided references. How come the first version was so wrong about the atmosphere-surface energy fluxes ? As I said before, I suspect the model is fine and only the description has problems but the whole process of deleting the surface energy balance part to replace it by this odd equation leaves me with a weird feeling. Clarifications are needed.

Finally, when answering to me, the authors did not really address my concern about the fact that the demonstration of the improvements brought by the new model needs to be improved (because I did not develop it enough in my detailed comments I guess). But I saw that other reviewers were more thorough on this point. So I’ll leave it to be fixed based on their input.

So in the end, I still think the study is interesting but the problems that bother me still need to be addressed. Also I realize now that the title of the study mentions a new model whereas it would be more accurate to mention new improvements brought to an existing model (which is different from creating a new model from scratch). Below are my comments to specific answers from the authors.

Comment 1.

I think that the answer to comment 1 is off. My point was to say that there is no reason such a variety of landscapes and surface processes leads to a uniform stratigraphy at the scale of a catchment and even less at the scale of the QTP. It is no problem to simplify reality if it is acknowledged and framed. Explaining the model class does not address this point.

Comment 2.

If so, can the author provide a proportion of the QTP area for which this stratigraphy applies ? I am not convinced by the references provided. We are discussing real world observations that can assess the validity of the proposed stratigraphy and the authors suggest two papers describing modelling works (Chen et al. 2015 and Yang et al. 2009). Among the 2 others, one I did not find (Sun et al. 1996), so I checked the other one (Deng et al. 2019). Maybe I missed it but I did not find anything about the ubiquity or widespread occurrence of a gravel layer below a thin soil layer over the whole QTP. The paper discusses Pliocene and Pleistocene deposits in the eastern QTP and their connection with tectonics. Figures 6 to 8 of this paper summarize the stratigraphy in different areas, figure 6 shows a lot of lateral variability as a consequence of the activity of a fault, figure 7 shows gravel on top of sand (for the upper part of the stratigraphy), and figure 8 shows humus on top of clay with limestone fragment. This last one could fit the theory of the authors but nowhere Deng et al. claim that this is ubiquitous. The word fragments do not appear anywhere else in the article. And Deng et al. use the word gravel in its common meaning and not as a rock fragment. So to say, I am still waiting for the proof that this stratigraphy is widespread across the QTP. Again I have no problem with simplifications but then it needs to be presented as such. I would largely prefer to read that it is a relevant simplification for the present watershed based on the field observations of the author. Either this or, as I was saying earlier, then the author should provide the order of magnitude of the coverage of this stratigraphy, a reference that says if it is e.g. 0.8%, 8% or 80% of the plateau that correspond to this stratigraphy, based on relevant references so that we know what we are discussing.

Comment 8.

“In a saturated state, the macropores form a fast channel for transporting water. However, when the SGM layer is in an unsaturated state, the water mainly moves under the actions of the matrix potential and gravitational potential. Thus, in an unsaturated state, the macropores do not work, and the gravel will hinder the movement of water.”

Conductivity is known to evolve with saturation but this explanation is a bit puzzling to me. How strong is the matrix potential in a soil with high gravel content ? And how come this matrix potential does not also ampere gravitational drainage ?

Comment 10.

Confusion between snow melt and avalanche is very surprising to me, but now I understand the corrected sentence. Can the author elaborate on the importance of accounting for avalanches for ground thermo-hydrological regime ? It is surprising to me that avalanches play a big role in this regard but I might be wrong.

By the way L258-259 of the revised manuscript still say:

“When the snow thickness difference between two calculation units exceeded this threshold, snow meltdown occurred. The snow in the higher-altitude calculation unit slides into the next unit until the two units have the same snow thickness.”

And line L264-265 say:

“when the difference in snow thickness between two adjacent contour bands exceeds this threshold, an avalanche occurs between those contour bands. The snow in the higher-altitude contour band slides into the lower band until the two bands equalize in snow thickness.”

This model description is still confusing and it should not be the case at this stage.

Comment 13.

This is extremely weird. In the initial version of the manuscript, the energy fluxes between the atmosphere and the surface was based on surface energy balance calculation with incoming and outgoing radiations, latent and sensible heat fluxes… And now all of this is replaced by this new equation 7 ! What happened to initial equations 7, 8 and 9 ? And how could the first model description be so wrong ? Such a difference implies a massive difference regarding the forcing data that are used. This now comes after the confusion between snow melt and avalanches and gives the impression that all these parts were written with very little knowledge of the model. It is the first time I see something like this and I do not know what to think of that. I still want to believe that only the model description is off.

Finally, the physics of the new equations look questionable to me. First, now it seems that the only energy exchange between the atmosphere and the surface corresponds to sensible heat fluxes so what about radiations ? What about evaporation ? Neither radiations nor evaporation are going to impact the soil thermal regime ? What about the claimed water-heat coupling if evaporation does not impact the energy fluxes between the surface and the atmosphere ? The consequences of such a choice need to be developed and discussed. Also the initial version of WEB COR included a surface energy balance calculation so if this is the new calculation is it a downgrade of WEB COR regarding physical processes and it should be mentioned.

Second, this flux does not depend on wind speed or not even on a bulk parameter such as a convection coefficient or the aerodynamic impedance that was present in the initial draft. This equation should describe a process happening at an interface and looks like something based on the energy variations of a volume of ground (C x V x dT, but in this case, dT would have a sense if it was a transient variation not an instantaneous potential). I am confused and considering what is happening here, I have a hard time believing this equation was used. Third, what value does the “du” parameter take ? Are we talking about several centimeters ? meters ? How is it established ? Because the energy change will vary linearly with this value.

Finally, I checked Jia et al. (2001) and the new equation 7 has nothing to do with Jia et al. (2001), even equation 61 from Jia et al. (2001) is very different. Jia et al. (2001) actually includes surface energy balance, as initially submitted here. I checked Hu and Islam (1995) (the new draft says Hu et al., 2001, I assume it is a typo) but new equation 7 is nowhere to be found either. I have the feeling new equation 7 is not physically valid regarding sensible fluxes for the aforementioned reasons (not counting that radiations and latent fluxes are for now on ignored) so I would need proof that I am wrong (i.e. a reference that established its validity).

Comment 34.

The answer is very nebulous and imprecise and following the answer on Comment 13, it shows a limited knowledge of the model. I am surprised that in a study aiming at bringing model developments, knowledge about the relationship between negative temperature (or energy) and liquid water content is so hard to find. Basically the appendix B14 told me to check Li et al. 2019, which I did. And Li et al. 2019 says “The water–heat continuous equation of frozen soil is solved numerically based on the soil freezing status and empirical formulas.” I tried to dig and went from Li et al. 2019 to Wang et al. 2014 and then to Niu et al. 2006, and there I actually found relationships between liquid water content and negative temperatures that are in WEB COR if understood correctly.

Comment 35.

If so please explain where and how you use the riverbed conductivity. And please do so when the model setup is described.

Comment 39.

"… Figure 10 have no measured values. Figure 10 was provided to compare the effect of model improvement on the hydrological cycle flux." How can we know that it is an improvement if there is no field value to compare to ? Unless I missed something, the fact that it is different does not imply that it is better no ? I don't follow this reasoning.

I went through the new draft:

Line 197

"… its higher reflectivity to shortwave solar radiation were also considered"

When talking about snow. So here again I wonder: are the authors using surface energy balance calculation (including radiations) or not ? Because if it is just the new equation 7, radiations are not accounted for in the model…

Line 227

I think it would be nice to have the values of the empirical parameters.

Several lines:

Line 63-64: "the saturated hydraulic conductivity of SGM decreases as the gravel content increases"

Line 222: "since gravel can neither conduct nor store water"

Line 242: "The gravel increases the porosity in the SGM layers"

Line 347-348: "The saturated hydraulic conductivity of the soil layer was 0.648 m/d, that of the SGM layer was 4.32 m/d"

These assertions don't work together or if they do please explain.

Line 279-280

"For the heat transfer process, assuming that the upper boundary of the system is the atmosphere, which controls the input and output of the system energy."

This sentence has neither subject nor conjugated verb relating to the subject.

Line 300-301

Former equation 11 is now equation 9.

Line 373-377

Please explain how you got discontinuous but millimetric values of the snow cover for your observations. Explain also how the comparison was made. The contour bands are 20 km2 in average and we are talking here about a point-wise measurement.

Line 379

If I understood correctly there is just one experimental site, so no S at site.

Line 395

The legend of the figure was cropped (visible on the initial submission). The graphs are left without legend, and cannot be understood.

Line 418

As for figure 7, there is no longer a legend on Figure 8 and the reader cannot know what is observation, QTP and COR.