Reviewer 1:

General comment

This third round of review is much more satisfying than the previous one regarding my two main concerns (e.g. the stratigraphy and the model description). I think that now the authors have convincing arguments to say that their stratigraphy deserves to be considered over the QTP even though they should still avoid to say that the whole plateau presents it since, as we talked about it before, it does not make much sense (e.g. where you have bedrock, fluvial deposits, moraine…). It does not need to be 100% of the plateau to be scientifically significant. I saw that so far the authors have implemented a lot of my suggestions in the appendix rather than in the main text but I think putting several of their last arguments to support the relevance of this stratigraphy in the main text will vastly improve the paper.

Dear Reviewer:

We thank you for your thoughtful suggestions and insights, which have enriched the manuscript and produced a better and more balanced account of the research. From the comments presented from prior discussion, it has come to our attention that the information presented on the geological stratification based on the QTP was not careful enough. Therefore, we have revised the introduction by adding arguments from the appendix to support the relevance of this stratigraphy.

Change from

"In addition, in the cold alpine regions of the QTP, the decomposition of biomass occurs mostly in the surface layers of Quaternary sediments owing to the low temperatures, resulting in the formation of a thin soil layer that is more highly developed and accumulates more organic matter than deeper layers (Sun, 1996). This soil stratification is particularly evident in alpine meadows (Yang et al., 2009; Pan et al., 2017)."

To:

"In addition, due to environmental constraints, physical weathering dominates the soil formation process on the QTP, resulting in a low level of soil mineral decomposition and slow soil development. Although the QTP has a variety of landscapes and surface processes, the grasslands occupy the largest proportion of the land, followed by bare land, the sum of which exceeds 80% (Zhang and Zhou, 2021). In these areas, the decomposition of biomass occurs mostly in the surface layers of Quaternary sediments because of the low temperatures, resulting in the formation of a thin soil layer that is highly developed and accumulates more organic matter than deeper layers (Sun, 1996). This soil stratification is widely spread on the QTP (Yang et al., 2009; Pan et al., 2017). The topsoil (typically 0 – 20 cm) in these areas is generally a sandy loam with a mixture of sand and gravel below it (Chen et al., 2015)." (Lines 64–71).

Regarding the model description, I am still not fully sure but I think we went through an imbroglio because in the first place, the model description included the equation for surface energy balance calculation (as well as reference to the effect of radiations on snow) even though the model does not perform SEB. I think it is terribly misleading and a bad practice in general. The model description (and the equations inside) should describe what is inside the model. So this needs to be fixed (more precisions below) and it is important for a cryo-hydrology paper to discuss the fact that latent heat fluxes between the atmosphere and the surface are ignored in the model. This should also be part of the main text, not in the appendix.

Reply: We apologize for the confusion. Considering that the upper boundary of the study object is the atmosphere, we did not use SEB in the calculation of the heat flux conducted into ground (G), but used the forcing recovery method directly through meteorological forcing. The SEB was included in the model, but instead of calculating G, it was used to calculate sensible heat and then give the breakdown of energy. This is indeed very misleading to the reader. Therefore, we removed the

sentences related to SEB that may mislead readers and introduced the calculation method of *G* as per your suggestion. Additionally, the implications of this current simplified calculation method for model simulations has been stated in the results and discussion section. Please refer to Comment 4 for more details.

I have a few smaller points apart from these 2 and some phrasing suggestions.

Comment 1

Answer to points 1 and 2

The author have addressed my concerns about how the stratigraphy of the QTP is handled in a much more complete and convincing way than previously. I am now satisfied by the provided references and I thank the author for this detailed explanation. I think part of these explanations should be included in the main text to explain the motivation for these developments, because to me (and likely to other readers), until today, this stratigraphic choices did not sound motivated enough. I like Table 1 of the answer, I think it really helps to understand the point of this study. Yet, I miss the source of the data. Where does it come from ?

Reply: We are pleased to hear that our explanation has addressed your concerns. Thank you for your suggestion, we have added the explanations in the Appendix to the Introduction accordingly. The revised information has been presented under the general comment.

Furthermore, we would like to apologize to the reviewer for the missing citation for the source data here, the data in Table 1 was calculated statistically from the Multi-Period Land Use Land Cover Remote Sensing Monitoring Dataset in China (CNLUCC) (Xu et al., 2018).

[1] Xu X L, Liu J Y, Zhang S W, et al. Multi-Period Land Use Land Cover Remote Sensing Monitoring Dataset in China (CNLUCC)[J]. Resource and Environmental Science Data Registration and Publication System. Available online: http://www. resdc. cn/DOI, 2018.

Comment 2

Answer to point 3

I understand better what the author means. But I still think saying that in unsaturated conditions "the macropores do not work" is a bit strange and maybe not very appropriate. If this is the conveyed idea, I would recommend something more standard like "In unsaturated conditions, the hydraulic conductivity is lower and this decreases the drainage ability of the soil". Same for "and the gravel will hinder the movement of water" if a higher gravel content implies a lower matrix suction (and higher hydraulic conductivity, cf discussions on point 11 below), isn't it rather the opposite, that gravels promote the drainage ability of the soil?

Reply: The two sentences are in response to comment 8 of the first round and we apologize that our initial colloquial explanation has brought about some confusion based on the concepts of hydraulic conductivity, drainage capacity, and water transport fluxes in the soil.

Hydraulic conductivity (K) is the water flux per unit water potential gradient in soil, the higher the soil hydraulic conductivity, the better the soil drainage capacity. In both saturated and unsaturated conditions, the gravel on the QTP are conducive to hydraulic conductivity (drainage capacity).

The soil water transport flux q is the actual water flux between soil layers (not the soil drainage capacity) and is influenced by both the water potential gradient (∇H) and the K (obtained by Darcy's law: $q = K(\theta_l)\nabla H$). Under unsaturated conditions, the gravel reduces the water potential gradient by reducing the matrix potential thereby affecting the water transport flux.

In short, gravel is a promoter of K and a reducer of ∇H. Whether it increases or decreases the soil water transport flux, it is dependent on the actual water content.

Comment 3

Answer to point 4

Thanks for the explanation. The authors also deleted "When the snow thickness difference between two calculation units exceeded this threshold, snow meltdown occurred" and that was an important thing to do as well.

Reply: We thank the reviewers for the acceptance of our explanation. This error was removed when the sentence was revised.

Comment 4

Answer to point 5

I am amazed that every time I point a problem in the model description a new equation appears. But I think I see what the problem has been since the beginning now. The thing is WEB-COR does not do surface energy balance calculation neither in Li et al. (2019) nor in the present draft. In their paper, Li et al. (2019) presents a surface energy balance equation (Eq. 1) that they do not use in their model. And the author of the present draft did the same in their initial submission… Why would you put in your model description an equation that your model ignores? This is extremely misleading, because it is normal to expect that your model includes SEB if you put the SEB equation in the model description. But I think the blame goes to Li et al. (2019) as I can tell the authors of the present draft largely got inspired from Li et al. (2019) to write their model description.

So I think it is time to make this point clear and to add in the model description (not in the appendix) that, consistently with Li et al. (2019):

- the model does not perform surface energy balance

- the climatic forcing affecting the surface and driving heat conduction in the ground only result from temperature difference between the surface and the atmosphere

- radiation are thus not explicitly taken into account

- the evaporation values that the model computes are purely hydrological and have no impact on the surface thermal regime, which is an important lack regarding the given objective to "develop a modeling framework representing coupled water and heat transfer in the ground" (cf introduction).

The discussion should also include a section discussing the consequences of not accounting for the thermal effect of evaporation on their results.

Reply: We apologise for any misleading information. After several rounds of reviewing and revision, all misleading sentences relating to energy balance and radiation have been removed from the manuscript. To make this part clearer, we have made a supplementary modification to the introduction of the *G* calculation as per the reviewer's suggestion by explicitly stating that the temperature difference between the surface and the atmosphere is the only source of heat conduction.

Change from

"For the heat transfer process, we assumed that the upper boundary of the heat transfer system is the atmosphere, which controls the input and output of energy in the system. The temperature difference between the atmosphere and the surface is the source of heat conduction."

To:

"Due to the zonal variability in altitude, land surface features, and vegetation characteristics, spatial differences exist in meteorological elements and aerodynamic parameters on the QTP. The use of the energy balance method, which involves multiple meteorological elements and aerodynamic parameters to calculate the heat flux of each contour band, can lead to extensive calculations and an unstable solution process. Therefore, we assumed that the upper boundary of the heat transfer system is the atmosphere, which controls the input and output of energy in the system. The temperature difference between the atmosphere and the surface is the only source of heat conduction" (Lines 278 – 283).

In addition, since the objective of the study is "to develop a modeling framework representing coupled water and heat transfer in the ground based on the

snow-soil-gravel layer structure", the focus is on the water and heat transfer in the snow-soil-gravel layer. Therefore, the calculation of the energy input to the upper boundary is simplified and the energy balance formula was not used. As per your suggestion, we added the following discussion on this defect in Section 3.2 accordingly:

"It should be pointed out that, the model improvement is mainly concerned with the water and heat transfer within the seasonal thaw layer. Coupled with the fact that the amount of evaporation during the freeze–thaw period is generally less and the latent heat of evaporation accounts for a small proportion of the net radiation. Therefore, the model simplified the calculation of the energy input to the upper boundary of the seasonal thaw layer by using the temperature difference between the atmosphere and the surface as the only source of heat conduction, without quantitatively considering the influence of sensible heat and latent heat of evaporation on the heat flux conducted into the ground. The model needs to be further improved in subsequent studies by systematically considering the influence of the radiation and climate characteristics of the QTP on each energy component." (Lines 440–447).

Comment 5

Answer to point 9

I appreciate the deletion but I still don't manage to understand why this was written in the first place, it was again very misleading, and gave the impression that the model included surface energy balance calculation even though it is not the case.

Reply: As per the response under the general comment, the surface energy balance was included in the model, but instead of calculating *G*, it was used to calculate sensible heat and then give the breakdown of energy. The manuscript has been revised and all misleading information have been removed.

Comment 6

Answer to point 11

I see the first 2 quotes of my initial remarks are now out of the manuscript, removing the ambiguity.

Reply: We thank you for your review and suggestions, as it has helped us to improve the manuscript.

Comment 7

I went through the draft again (since it is the 3rd time, I ignored the part for which we did not discuss anything recently)

L40 (and 78): I would replace "unique" by "characteristic" or "typical"

L75: I would replace "hidden" by "compensated"

L82: "(when the temperature of the calculation…)"

L188 "a key link" between what and what ? maybe a key feature

Reply: Thank you for your suggestions, these have been amended accordingly.

Comment 8

L199: the Wiktionary says that a continuum is: "A continuous series or whole, no part of which is noticeably different from its adjacent parts, although the ends or extremes of it are very different from each other." and I am not sure it applies well to the superposition of 3 different media separated by interfaces.

Reply: We modified the "snow–soil–gravel layer continuum" to "snow–soil–gravel layer structure" by reason of rigor, this change also allows for uniformity with "dualistic soil–gravel structure".

Comment 9

L252: "only moisture simulation was performed during this period" what is the ground temperature profile when the freeze thaw period restarts if it was not calculated during fully thawed period?

Reply: The initial ground temperature profile for a new freeze-thaw period set in the current model was inherited from the ground temperature profile at the end of the previous freeze-thaw period.

Comment 10

L257: "Therefore, avalanches are common in this region" common in steep/mountainous regions. As for the soil structure, the author should be, once more, more careful and avoid saying "this process is like this for the whole QTP". A plateau presents by definitions some areas with flat or gentle slope that does not trigger avalanches.

Reply: Thank you for pointing this out. We have revised it accordingly and have checked the full text for rigour.

Comment 11

L288: the fundamental frequency of what? The frequency of the sinusoidal signal used to establish this theory I guess.

Reply: Yes, it is the angular frequency ($\omega = \frac{2\pi}{86400}$).

Comment 12

L295: why not directly give the values rather than give how they compare to 0, it would be more informative. Is it different for each contour band?

Reply: Yes it is different, the topography of many places on the QTP is undulated. Our calculation unit is based on the contour band, the bottom boundary of each contour band is not at the same altitude and maintains a different constant temperature under the influence of geothermal and climatic forcing. The higher the altitude, the lower the bottom temperature.

Comment 13

L359: I would not use "considerably" if you don't give numbers to support that it is considerable.

L387: "Owing to" -> because of

Reply: Thank you for your suggestions, these have been amended accordingly.

Comment 14

L412-413: "During the freezing period (December–March), the liquid water content of the upper layer first decreased owing to the decrease in temperature". I am not sure I fully understand. The freezing process converts liquid water in ice and once freezing is completed, temperature can decrease again. Is that what is implied here? Because it is different from saying that because the liquid wc decrease, the temperature decrease.

Reply: We apologize for the misunderstanding; here we simply wanted to describe the reduction of the water content in the soil profile layer by layer as the atmospheric temperature decreases. To reduce ambiguity, we rewrote this sentence as:

"After entering the freeze–thaw period, the water in the soil started to freeze from the top layer to the bottom as the atmospheric temperature dropped, thereby decreasing the liquid water content layer by layer until February" (Lines 417–419).

Comment 15

L415: "melt" -> Thaw, unless you say "the ice in the upper layer began to melt".

Same L416

L455-457: no need for decimal number in the percentage.

Reply: Thank you for your suggestions. these have been amended accordingly.

Comment 16

L478: "observed", earlier in the review processed I pointed out that on Fig 10 you show differences between QTP and COR but without the reference point given by some observations and I remember saying "different does not necessarily implies better". The answer was to say that it was not the point of the figure to compare with observations. But now the paragraph presenting the figure ends up mentioning that the QTP version shows a feature that is "observed". So either there are observations and there should be on the graph or there are not. Or is it some kind of general statement ? Please clarify this point.

Reply: We thank the reviewer for bringing up this point, to clarify, this is a general statement referring to a slow flow reduction process which is in accordance with the observed flow changes in Figure 10 (original Fig. 9). To reduce ambiguity, we added the definition of "tailing" process (a slow flow reduction process consistent with observed flows, Lines 458-459) and rewrote this sentence as follows:

"Water in the WEP–QTP model had more time to complete groundwater and river recharge, and thus exhibited a better 'tailing' process than that in the WEP–COR model (Fig. 10)" (Lines 490–492).

Comment 17

L484: "In cold regions, climate change affects high and low flows in different ways" Which flow are we talking about here? River flow I assume that because you cite Song et al., 2021 which seems to talk about river runoff? If so precise it in the sentence.

Reply: We apologize to the reviewer for not clarifying this. This refers to river flow. We have amended the sentence accordingly to improve clarity:
"In cold regions, climate change affects high and low river flows in different ways." (Line 497).

Comment 18

L489-490: "Our study demonstrates that the soil–gravel layer structure on the QTP is different from the soil structure in other cold regions."
This is not exact, you assume it is different based on other studies and then you demonstrate its impact on the cryo-hydrology with your model.

Reply: Thank you for pointing this out and we have amended this sentence to: "When simulating the water cycle process in the QTP, it is not appropriate to simply generalize the active layer as a homogeneous soil structure and ignore the influence of g-layers beneath the soil-layers. This study shows that..." (Lines 502–503).

Comment 19

L509: I would also remove decimal numbers to the % values

Reply: Thank you for your suggestion and we have amended this accordingly.

Comment 20

L519: "the unique geological structure and climatic characteristics of the QTP". This is again a statement that is not careful enough "the widely spread geological structure" would be a bit more reasonable. Even though this structure might be often present, it still makes no sense to claim that it covers 100% of the QTP.

Reply: Thank you for your suggestion. We have revised it accordingly and have checked the full text for rigour.

Reviewer 2:

The authors made substantial improvement to the paper. The WEP–QTP model seems to be an effective tool to simulate water-heat balances in the dualistic soil–gravel structure in QTP. I recommend the paper could be acceptable after minor revision.

I understand this study improved the hydrological model by considering freeze–thaw processes in the dualistic soil–gravel structure, but the authors should address uncertainties regarding the model structure and the newly-introduced parameters. For example, how the parameters or the degree-day factor and the critical temperature were prescribed or estimated in the modeling. The authors should give a brief discussion on the uncertainties.

Dear Reviewer:

We are glad that our improvements to this paper have been recognized by you.

How to estimate the parameters involved in the new formula has been introduced in the parameter description following the formula.

The new parameters added due to model improvement, including the degree-day factor, the critical temperature, and the critical value of non-heavy versus heavy rain periods, are all sensitive parameters for the improved model and have clear physical significance. The degree-day factor and critical temperature values were estimated by the modelling of snow thickness, while the critical value of non-heavy versus heavy rain periods was determined by the parametric calibration of the flow process.

We made the following supplements in section 3.1:

"The new sensitive parameters for the model improvement included the degree-day factor of snow, the critical temperature of snow and the critical value of non-heavy versus heavy rain periods, where the degree-day factor and the critical temperature of snow were estimated by the modelling of snow thickness, and the critical value of non-heavy versus heavy rain periods was determined by the

parametric calibration of the flow process. The values of these parameters were: 4 mm/[°C·day], −1 °C, 15 mm/day respectively.” (Lines 356–360).

Thank you for your advice and suggestions, we look forward to hearing from you and would be happy to make further changes, if required.