Reviewer 1

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.

Dear Reviewer:

Firstly, thank you very much for your detailed and professional comments during the two reviews. In the abstract, we have made the following change to make our description more precise:

"The Qinghai-Tibet Plateau (QTP) has a thin soil layer on top of a thick soil-gravel mixture (SGM) layer."

To:

"Owing to plate movements and climatic effects, the surface soils of bare lands and grasslands on the Qinghai–Tibet Plateau (QTP) are thin, and the soil below the surface contains abundant gravel."

Secondly, we have also provided additional information about the distribution of this geological structure on the Tibet Plateau based on relevant literature, as detailed in the answers to Comments 1 and 2 below. I hope this will help you better understand the distribution of this structure on the QTP.

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.

Reply: We apologize for the confusion. The initial manuscript originally intended to present the specific calculation of each energy flux in the energy balance equation of the model, but it did not introduce the calculation method of G clearly. In this study, the upper boundary of the object of study is the atmosphere, and the temperature difference between the atmosphere and the surface is the source of heat transfer. Therefore, the added equation 7 is used to calculate G. In the revised manuscript, we have supplemented the derivation procedure of the equation used to solve G and the corresponding references. This is clarified in detail in the responses to Comment 5 below.

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.

Reply: Thanks to your suggestion, we have changed the title accordingly, as follows:

“Application of an improved distributed hydrological model based on soil–gravel structure in the Niyang River Basin, Qinghai–Tibet Plateau”

Thank you very much for your careful review of our manuscript. Your comments have helped us to greatly improve the quality of the manuscript. For the comments that were not clearly explained in the previous reply, we will provide additional detailed responses below.

1. ***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.

Reply: The QTP has a variety of landscapes and surface processes, but grassland occupies the largest proportion of land use, followed by bare land, and the sum of the two accounts for up to 81.64% (Table 1).

Table 1. Proportion of land use in Qinghai-Tibet Plateau

Land use	grassland	bare land	forest	water body	cultivated land	built-up land
Proportion (%)	48.60	33.04	12.05	5.18	1.02	0.11

In these areas, physical weathering is dominant due to long-term low temperatures, mineral decomposition is low, and clay content gradually decreases from top to bottom. In the central and eastern plateau meadow areas of the QTP, due to the slow decomposition of biomass in the soil, the topsoil (typically 0–20 cm) in this region accumulates much denser grassroots and more soil organic matter than does the deep soil. The soil stratification in these areas is very significant compared to that observed in other regions (Yang et al., 2009). In the reply to Comment 2, we provide specific literature evidence and a detailed reply. Accordingly, we have revised the abstract and the introduction regarding the distribution of this soil stratification structure to make our discussion more rigorous.

In the abstract, change from

"The Qinghai-Tibet Plateau (QTP) has a thin soil layer on top of a thick soil-gravel mixture (SGM) layer."

To:

"Owing to plate movements and climatic effects, the surface soils of bare lands and grasslands on the Qinghai–Tibet Plateau (QTP) are thin, and the soil below the surface contains abundant gravel."

In the introduction, change from

"In addition, under strong freeze-thaw conditions in the cold plateau region, the humus accumulation of herbaceous plants is slow, while the decomposition of minerals is weak, resulting in slow soil development on the surface of Quaternary deposits and a thin soil layer above the SGM (Deng et al., 2019; Yang et al., 2009; Chen et al., 2015; Sun, 1996)."

To:

"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)."

2. 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.

Reply: In the manuscript, we provide four references (Deng et al., 2019; Yang et al., 2009; Chen et al., 2015; Sun, 1996), two of which you may have missed (Yang et al., 2009; Chen et al., 2015).

Reference 1: Yang K, Chen Y Y, Qin J. Some practical notes on the land surface modeling in the Tibetan Plateau[J]. Hydrology and Earth System Sciences, 2009, 13(5): 687-701.

In section 4.1 of this paper the authors write: “The decomposition of the biomass in the soil is slow due to low temperature over the Plateau, and therefore, the topsoil (~typically 0–20 cm) in the CE-TP region accumulates much denser grassroots and more soil organic matters (SOM) (not shown) than the deep soil does. This soil stratification in the CE-TP should be addressed for the following reasons. First, the soil stratification in the CE-TP is very significant compared to that observed in other regions."

This paper also provides the soil texture and parameters obtained from laboratory experiments of soil samples taken at Anduo sites (Table 3).

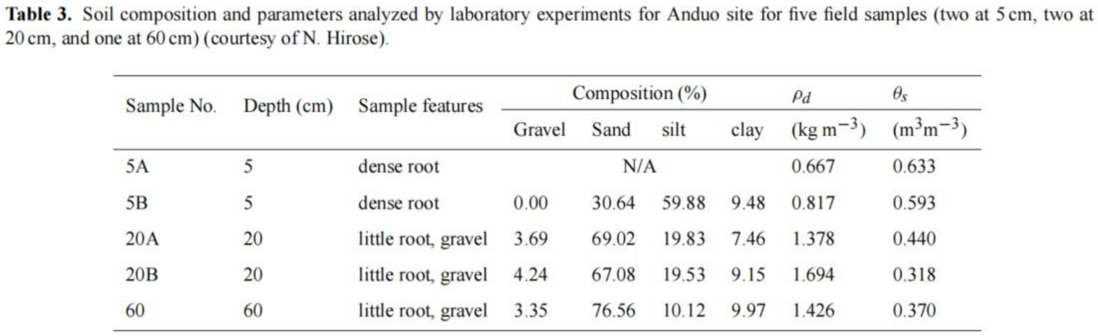


Table 3. Soil composition and parameters analyzed by laboratory experiments for Anduo site for five field samples (two at 5 cm, two at 20 cm, and one at 60 cm) (courtesy of N. Hirose).

Sample No.	Depth (cm)	Sample features	Composition (%)				ρ_d	θ_s
			Gravel	Sand	silt	clay	(kg m^{-3})	(m^{3}m^{-3})
5A	5	dense root	N/A				0.667	0.633
5B	5	dense root	0.00	30.64	59.88	9.48	0.817	0.593
20A	20	little root, gravel	3.69	69.02	19.83	7.46	1.378	0.440
20B	20	little root, gravel	4.24	67.08	19.53	9.15	1.694	0.318
60	60	little root, gravel	3.35	76.56	10.12	9.97	1.426	0.370

From Table 3, it can be find that the proportion of gravel is 0% and the proportion of sand is 30.64% when the depth $<$ 20 cm. When the depth $\geq$ 20 cm, gravel appears, and the proportion of sand increases to more than twice that of the surface soil. A clear soil stratification can be observed.

Reference 2: Chen H, Nan Z, Zhao L, et al. Noah modelling of the permafrost distribution and characteristics in the West Kunlun area, Qinghai‐Tibet Plateau, China[J]. Permafrost and Periglacial Processes, 2015, 26(2): 160-174.

This paper notes the influence of soil characteristics of the West Kunlun region, located on the QTP, on the simulation of permafrost distribution. Sensitive soil parameters of 0~1 m soil layers were provided by the laboratory soil particle size distribution analysis and the empirical model of soil parameters based on soil texture (Table 1).

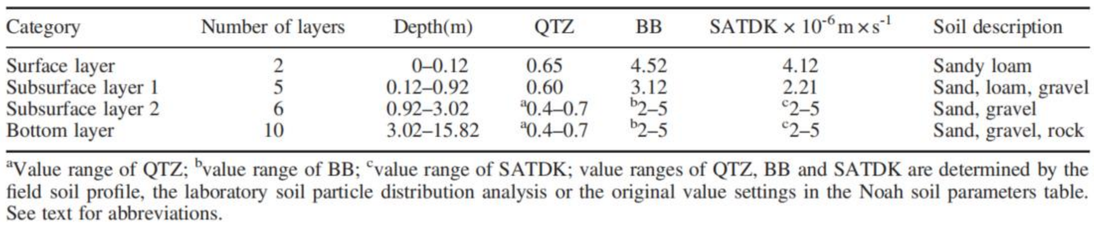


Table 1 Soil layers and parameters at the TGL station.

Category	Number of layers	Depth(m)	QTZ	BB	SATDK × 10^{-6} m × s^{-1}	Soil description
Surface layer	2	0–0.12	0.65	4.52	4.12	Sandy loam
Subsurface layer 1	5	0.12–0.92	0.60	3.12	2.21	Sand, loam, gravel
Subsurface layer 2	6	0.92–3.02	[a]0.4–0.7	[b]2–5	[c]2–5	Sand, gravel
Bottom layer	10	3.02–15.82	[a]0.4–0.7	[b]2–5	[c]2–5	Sand, gravel, rock

[a]Value range of QTZ; [b]value range of BB; [c]value range of SATDK; value ranges of QTZ, BB and SATDK are determined by the field soil profile, the laboratory soil particle distribution analysis or the original value settings in the Noah soil parameters table. See text for abbreviations.

As can be seen from this table, the soil is sandy loam at 0–0.12 m, but below 0.12 m, the soil becomes a mixed layer with sand and gravel.

Also in another paper:

Reference 3: Pan Y, Lyu S, Li S, Gao Y, Meng X, Ao Y, and Wang S: Simulating the role of gravel in freeze－thaw process on the Qinghai－Tibet Plateau, Theoretical and applied climatology, 127, 1011-1022, 2017.

This paper pays attention to the gravel in the soil of the QTP. Through the sampling results in Madoi (Table 1) and Nagqu (Table 2), it can be find that the content of gravel in the surface soil is relatively low (≤ 10%), and the content of gravel under the surface soil is higher (around 30%). The soil also has an obvious stratified structure.

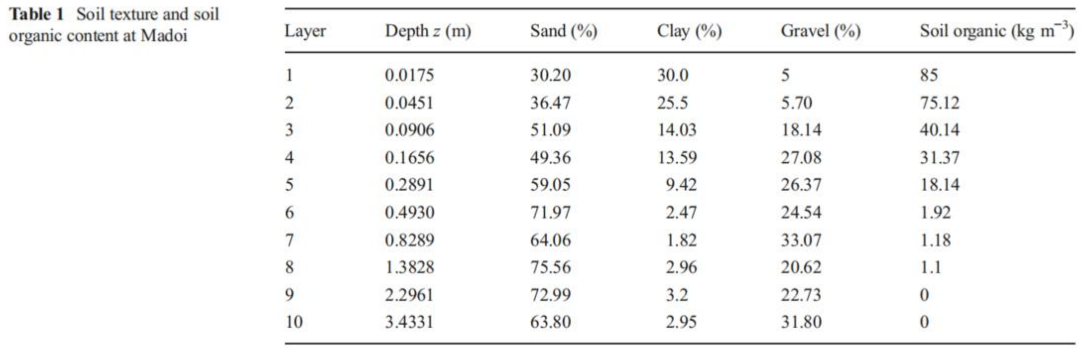


Table 1 Soil texture and soil organic content at Madoi

Layer	Depth z (m)	Sand (%)	Clay (%)	Gravel (%)	Soil organic (kg m^{-3})
1	0.0175	30.20	30.0	5	85
2	0.0451	36.47	25.5	5.70	75.12
3	0.0906	51.09	14.03	18.14	40.14
4	0.1656	49.36	13.59	27.08	31.37
5	0.2891	59.05	9.42	26.37	18.14
6	0.4930	71.97	2.47	24.54	1.92
7	0.8289	64.06	1.82	33.07	1.18
8	1.3828	75.56	2.96	20.62	1.1
9	2.2961	72.99	3.2	22.73	0
10	3.4331	63.80	2.95	31.80	0

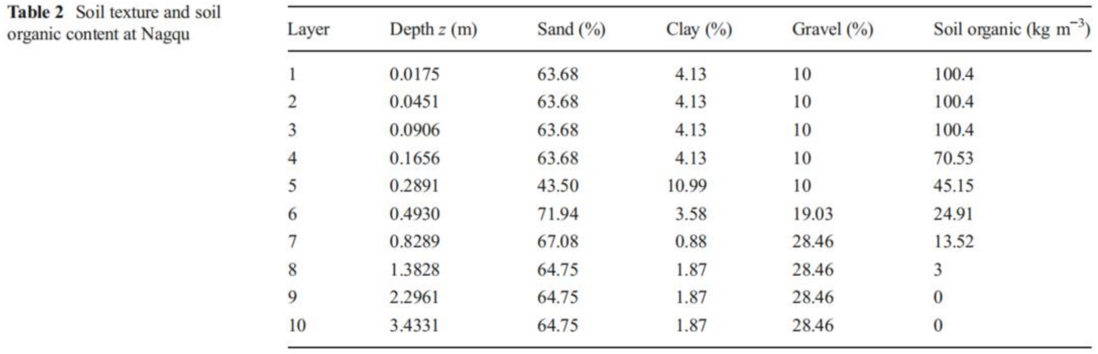
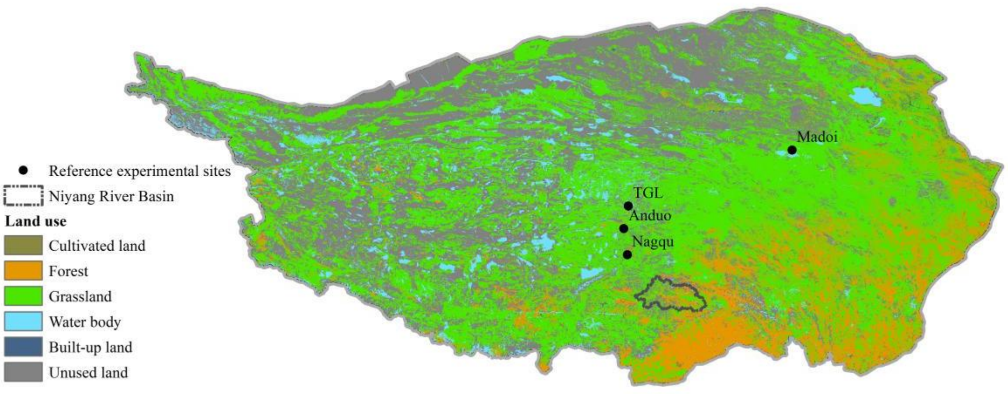


Table 2 Soil texture and soil organic content at Nagqu

Layer	Depth z (m)	Sand (%)	Clay (%)	Gravel (%)	Soil organic (kg m^{-3})
1	0.0175	63.68	4.13	10	100.4
2	0.0451	63.68	4.13	10	100.4
3	0.0906	63.68	4.13	10	100.4
4	0.1656	63.68	4.13	10	70.53
5	0.2891	43.50	10.99	10	45.15
6	0.4930	71.94	3.58	19.03	24.91
7	0.8289	67.08	0.88	28.46	13.52
8	1.3828	64.75	1.87	28.46	3
9	2.2961	64.75	1.87	28.46	0
10	3.4331	64.75	1.87	28.46	0

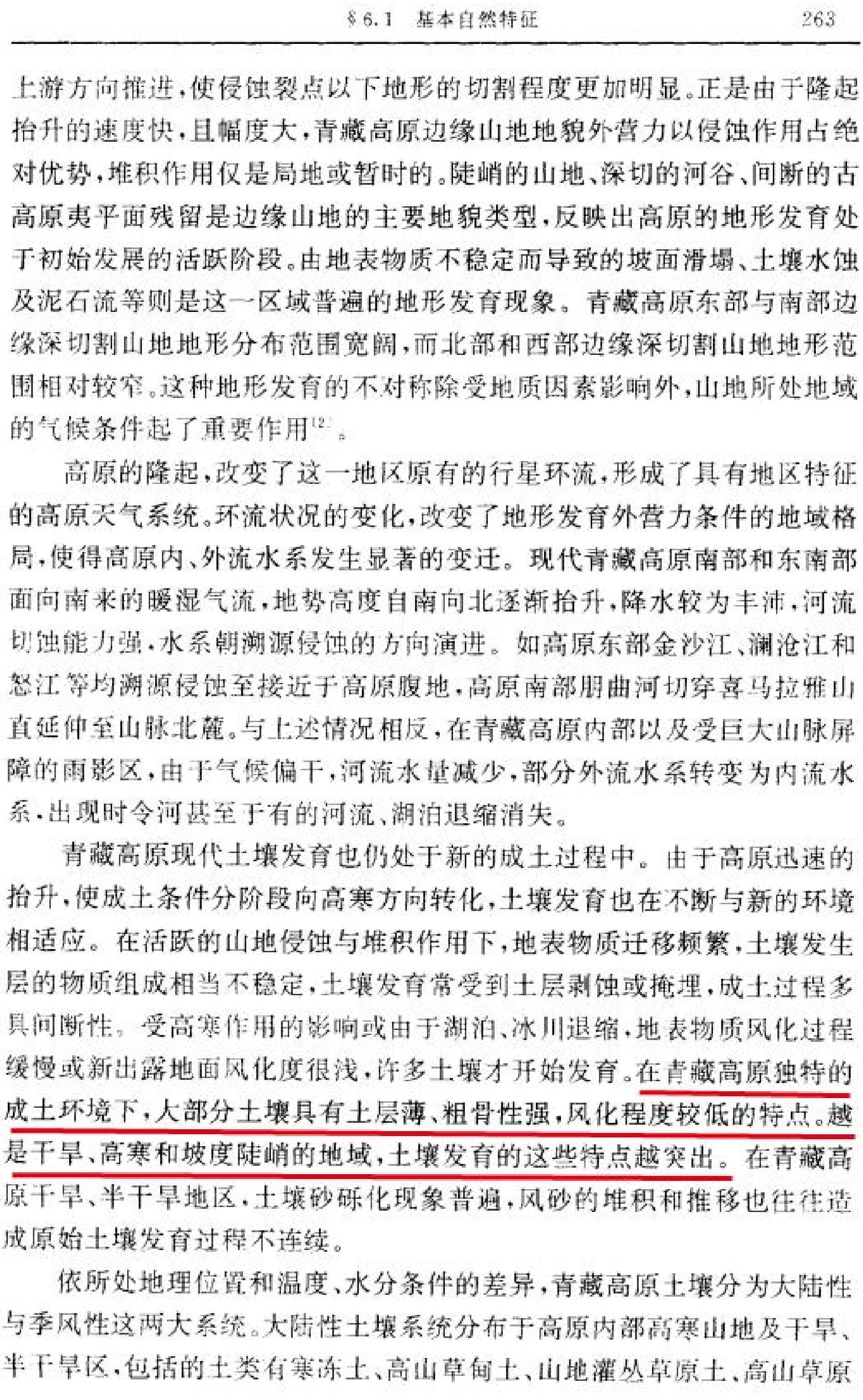
We plotted the sampling sites of these studies and our study areas in the figure below so that you can better understand the distribution of the current stratified research on the QTP.



Reference 4: Sun, H., The formation and evolution of the Qinghai-Tibet Plateau. Shanghai Science and Technology Press, 1996, ISBN:9787532340231.

The reference you didn't find (Sun et al. 1996) is a book written by Sun Honglie, an academician of the Chinese Academy of Engineering, who was the leader of the first Qinghai–Tibet scientific expedition. This book introduced the formation mechanism of the QTP, the uplift process, and its impact on the natural environment and human activities.

On page 263, when introducing the basic natural features of the QTP, it states that, "In the unique soil-forming environment of the QTP, most soils are characterized by thin soil layer...":



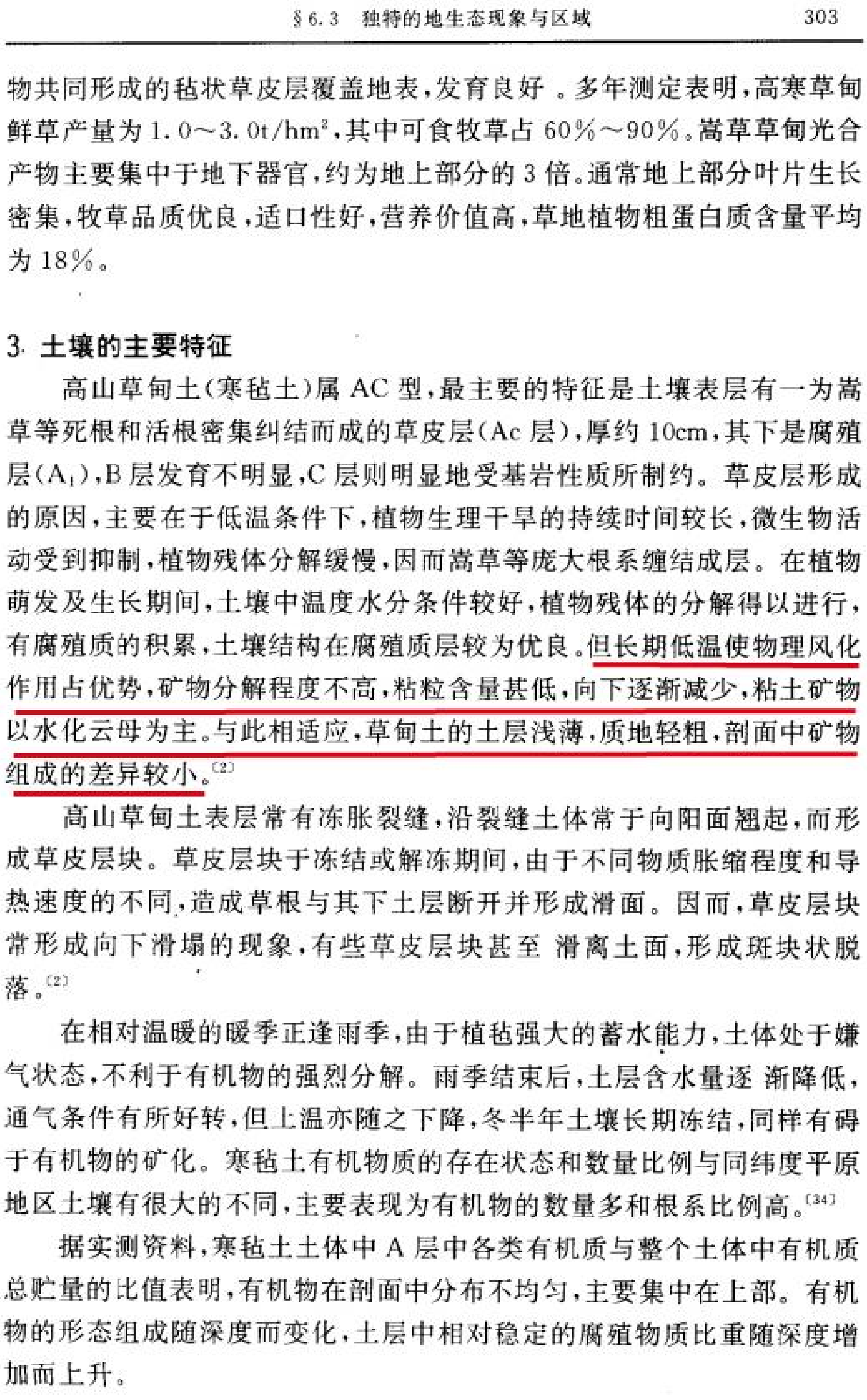
上游方向推进，使侵蚀裂点以下地形的切割程度更加明显。正是由于隆起抬升的速度快，且幅度大，青藏高原边缘山地地貌外营力以侵蚀作用占绝对优势，堆积作用仅是局地或暂时的。陡峭的山地、深切的河谷、间断的古高原夷平面残留是边缘山地的主要地貌类型，反映出高原的地形发育处于初始发展的活跃阶段。由地表物质不稳定而导致的坡面滑塌、土壤水蚀及泥石流等则是这一区域普遍的地形发育现象。青藏高原东部与南部边缘深切割山地地形分布范围宽阔，而北部和西部边缘深切割山地地形范围相对较窄。这种地形发育的不对称除受地质因素影响外，山地所处地域的气候条件起了重要作用[2]。

高原的隆起，改变了这一地区原有的行星环流，形成了具有地区特征的高原天气系统。环流状况的变化，改变了地形发育外营力条件的地域格局，使得高原内、外流水系发生显著的变迁。现代青藏高原南部和东南部面向南来的暖湿气流，地势高度自南向北逐渐抬升，降水较为丰沛，河流切蚀能力强，水系朝溯源侵蚀的方向演进。如高原东部金沙江、澜沧江和怒江等均溯源侵蚀至接近于高原腹地，高原南部朋曲河切穿喜马拉雅山直延伸至山脉北麓。与上述情况相反，在青藏高原内部以及受巨大山脉屏障的雨影区，由于气候偏干，河流水量减少，部分外流水系转变为内流水系，出现时令河甚至于有的河流、湖泊退缩消失。

青藏高原现代土壤发育也仍处于新的成土过程中。由于高原迅速的抬升，使成土条件分阶段向高寒方向转化，土壤发育也在不断与新的环境相适应。在活跃的山地侵蚀与堆积作用下，地表物质迁移频繁，土壤发生层的物质组成相当不稳定，土壤发育常受到土层剥蚀或掩埋，成土过程多具间断性。受高寒作用的影响或由于湖泊、冰川退缩，地表物质风化过程缓慢或新出露地面风化度很浅，许多土壤才开始发育。在青藏高原独特的成土环境下，大部分土壤具有土层薄、粗骨性强，风化程度较低的特点。越是干旱、高寒和坡度陡峭的地域，土壤发育的这些特点越突出。在青藏高原干旱、半干旱地区，土壤砂砾化现象普遍，风砂的堆积和推移也往往造成原始土壤发育过程不连续。

依所处地理位置和温度、水分条件的差异，青藏高原土壤分为大陆性与季风性这两大系统。大陆性土壤系统分布于高原内部高寒山地及干旱、半干旱区，包括的土类有寒冻土、高山草甸土、山地灌丛草原土、高山草原

On page 303, when describing the soil characteristics of the alpine meadow with the largest proportion of the QTP, it also states that, "The long-term low temperature makes the physical weathering dominant, the degree of mineral decomposition is not high. The content of clay is very low, and gradually decreases from top to bottom. In line with this, the soil layer of meadow is shallow, and coarse in texture.....":



§6.3　独特的地生态现象与区域　303

物共同形成的毡状草皮层覆盖地表，发育良好。多年测定表明，高寒草甸鲜草产量为1.0～3.0t/hm^2，其中可食牧草占60%～90%。嵩草草甸光合产物主要集中于地下器官，约为地上部分的3倍。通常地上部分叶片生长密集，牧草品质优良，适口性好，营养价值高，草地植物粗蛋白质含量平均为18%。

3. 土壤的主要特征

高山草甸土（寒毡土）属AC型，最主要的特征是土壤表层有一为嵩草等死根和活根密集纠结而成的草皮层（Ac层），厚约10cm，其下是腐殖层（A_1），B层发育不明显，C层则明显地受基岩性质所制约。草皮层形成的原因，主要在于低温条件下，植物生理干旱的持续时间较长，微生物活动受到抑制，植物残体分解缓慢，因而嵩草等庞大根系缠结成层。在植物萌发及生长期间，土壤中温度水分条件较好，植物残体的分解得以进行，有腐殖质的积累，土壤结构在腐殖质层较为优良。但长期低温使物理风化作用占优势，矿物分解程度不高，粘粒含量甚低，向下逐渐减少，粘土矿物以水化云母为主。与此相适应，草甸土的土层浅薄，质地轻粗，剖面中矿物组成的差异较小。[2]

高山草甸土表层常有冻胀裂缝，沿裂缝土体常于向阳面翘起，而形成草皮层块。草皮层块于冻结或解冻期间，由于不同物质胀缩程度和导热速度的不同，造成草根与其下土层断开并形成滑面。因而，草皮层块常形成向下滑塌的现象，有些草皮层块甚至滑离土面，形成斑块状脱落。[2]

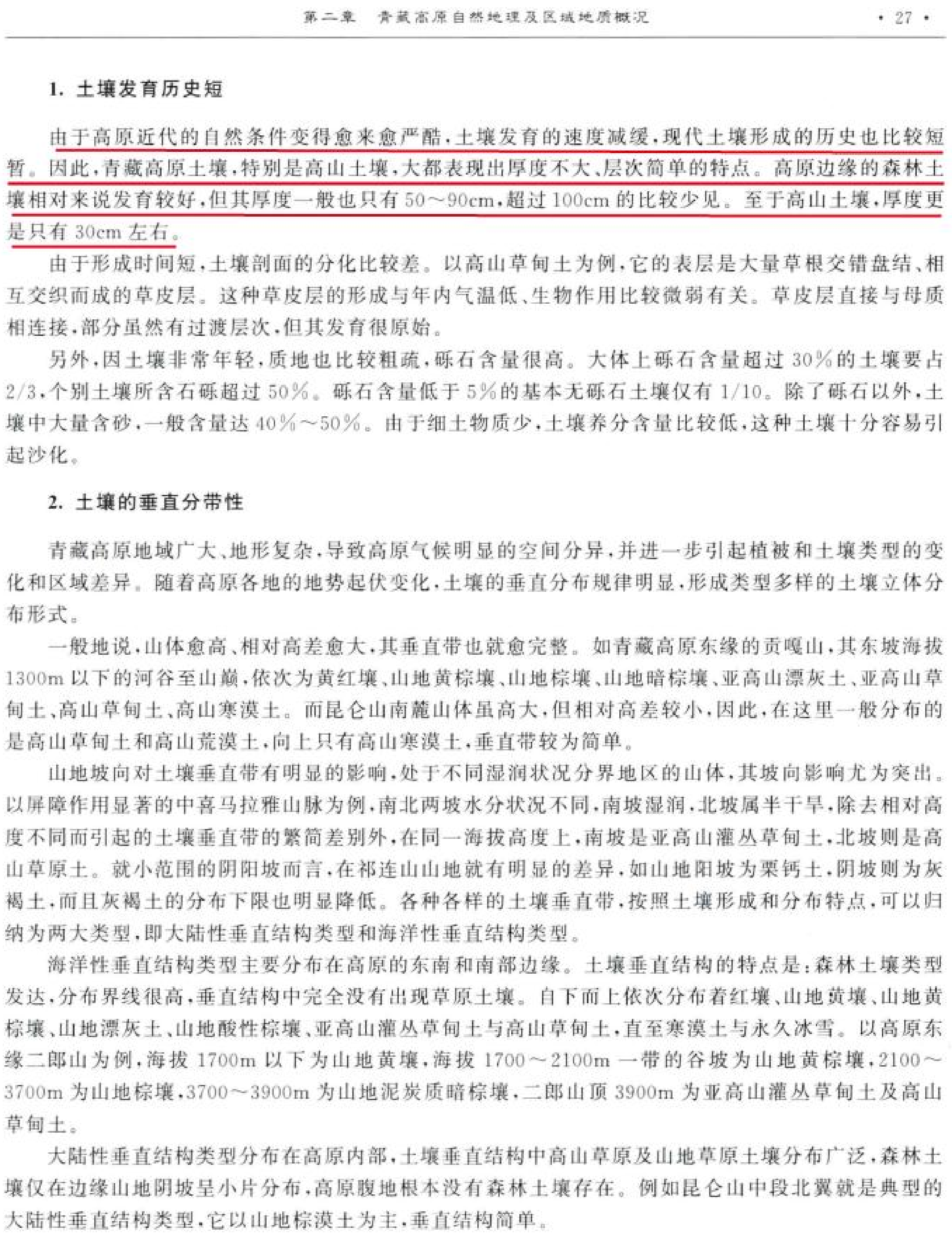
在相对温暖的暖季正逢雨季，由于植毡强大的蓄水能力，土体处于嫌气状态，不利于有机物的强烈分解。雨季结束后，土层含水量逐渐降低，通气条件有所好转，但上温亦随之下降，冬半年土壤长期冻结，同样有碍于有机物的矿化。寒毡土有机物质的存在状态和数量比例与同纬度平原地区土壤有很大的不同，主要表现为有机物的数量多和根系比例高。[34]

据实测资料，寒毡土土体中A层中各类有机质与整个土体中有机质总贮量的比值表明，有机物在剖面中分布不均匀，主要集中在上部。有机物的形态组成随深度而变化，土层中相对稳定的腐殖物质比重随深度增加而上升。

In addition, during our review we also found, in another book, a statement consistent with the findings of our study.

Reference 5: Sun Z Y, Zhou A G, Bu J W, et al., Research on geological environmental carrying capacity evaluation method for mineral resources development in Qinghai-Tibet Plateau, China University of Geosciences Press, 2016, ISBN：9787562539940

On page 27, when introducing the soil of the QTP, the authors state that, "The soil of the Qinghai-Tibet Plateau, especially the alpine soil, mostly shows the characteristics of small thickness and simple layers. The forest soil at the edge of the plateau is relatively well developed, but its thickness is generally only 50 to 90cm, and it is relatively rare to see more than 100cm. As for alpine soils, the thickness is even only about 30cm."



第二章 青藏高原自然地理及区域地质概况 • 27 •

1. 土壤发育历史短

由于高原近代的自然条件变得愈来愈严酷，土壤发育的速度减缓，现代土壤形成的历史也比较短暂。因此，青藏高原土壤，特别是高山土壤，大都表现出厚度不大、层次简单的特点。高原边缘的森林土壤相对来说发育较好，但其厚度一般也只有 50～90cm，超过 100cm 的比较少见。至于高山土壤，厚度更是只有 30cm 左右。

由于形成时间短，土壤剖面的分化比较差。以高山草甸土为例，它的表层是大量草根交错盘结、相互交织而成的草皮层。这种草皮层的形成与年内气温低、生物作用比较微弱有关。草皮层直接与母质相连接，部分虽然有过渡层次，但其发育很原始。

另外，因土壤非常年轻，质地也比较粗疏，砾石含量很高。大体上砾石含量超过 30%的土壤要占 2/3，个别土壤所含石砾超过 50%。砾石含量低于 5%的基本无砾石土壤仅有 1/10。除了砾石以外，土壤中大量含砂，一般含量达 40%～50%。由于细土物质少，土壤养分含量比较低，这种土壤十分容易引起沙化。

2. 土壤的垂直分带性

青藏高原地域广大、地形复杂，导致高原气候明显的空间分异，并进一步引起植被和土壤类型的变化和区域差异。随着高原各地的地势起伏变化，土壤的垂直分布规律明显，形成类型多样的土壤立体分布形式。

一般地说，山体愈高、相对高差愈大，其垂直带也就愈完整。如青藏高原东缘的贡嘎山，其东坡海拔 1300m 以下的河谷至山巅，依次为黄红壤、山地黄棕壤、山地棕壤、山地暗棕壤、亚高山漂灰土、亚高山草甸土、高山草甸土、高山寒漠土。而昆仑山南麓山体虽高大，但相对高差较小，因此，在这里一般分布的是高山草甸土和高山荒漠土，向上只有高山寒漠土，垂直带较为简单。

山地坡向对土壤垂直带有明显的影响，处于不同湿润状况分界地区的山体，其坡向影响尤为突出。以屏障作用显著的中喜马拉雅山脉为例，南北两坡水分状况不同，南坡湿润，北坡属半干旱，除去相对高度不同而引起的土壤垂直带的繁简差别外，在同一海拔高度上，南坡是亚高山灌丛草甸土，北坡则是高山草原土。就小范围的阴阳坡而言，在祁连山山地就有明显的差异，如山地阳坡为栗钙土，阴坡则为灰褐土，而且灰褐土的分布下限也明显降低。各种各样的土壤垂直带，按照土壤形成和分布特点，可以归纳为两大类型，即大陆性垂直结构类型和海洋性垂直结构类型。

海洋性垂直结构类型主要分布在高原的东南和南部边缘。土壤垂直结构的特点是：森林土壤类型发达，分布界线很高，垂直结构中完全没有出现草原土壤。自下而上依次分布着红壤、山地黄壤、山地黄棕壤、山地漂灰土、山地酸性棕壤、亚高山灌丛草甸土与高山草甸土，直至寒漠土与永久冰雪。以高原东缘二郎山为例，海拔 1700m 以下为山地黄壤，海拔 1700～2100m 一带的谷坡为山地黄棕壤，2100～3700m 为山地棕壤，3700～3900m 为山地泥炭质暗棕壤，二郎山顶 3900m 为亚高山灌丛草甸土及高山草甸土。

大陆性垂直结构类型分布在高原内部，土壤垂直结构中高山草原及山地草原土壤分布广泛，森林土壤仅在边缘山地阴坡呈小片分布，高原腹地根本没有森林土壤存在。例如昆仑山中段北翼就是典型的大陆性垂直结构类型，它以山地棕漠土为主，垂直结构简单。

The limited experiments we have conducted on the QTP do not yet allow us to give a definite percentage. However, from the above literature, we conclude that this soil stratification structure is not unique to the Niyang River basin and, at least in the meadow area accounting for 48.60% of the QTP, this kind of soil stratification structure is very significant. In the bare land, which accounts for 33.04% of the QTP, the stratification of the soil was not as significant as that in the meadows due to the thinner surface soil, but there is also abundant gravel beneath the surface soil.

3. ***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 ?

Reply: According to Darcy's law, the amount of water transported in the soil is:

$$q = K(\theta_l)\nabla H$$

where $K(\theta_l)$ is the hydraulic conductivity (cm/s) of the soil when the liquid water content is θ_l; ∇H is the gradient of water potential (containing gravitational potential and matrix potential); and q is the amount of water transported in the soil (cm/s).

Hydraulic conductivity and water potential gradients together affect the amount of water transported in the soil.

In the unsaturated state, the matrix potential and gravity potential work together to drive water transport. The effect of gravel content on the soil matrix potential can be specified in Equation 1:

$$\frac{\theta_l - \theta_r}{\theta_s - \theta_r} = A_m h^{-\lambda}\left(1 - B_m \omega_{gravel}\right)$$

In our study, for every 10% increase in gravel percentage with constant value of $\frac{\theta_l-\theta_r}{\theta_s-\theta_r}$, the matric suction decreases by approximately 10%.

In the saturated state, the matrix potential is 0, and water movement is driven only by the gravitational potential.

4. ***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.

Reply: As can be seen from figures A1 and A2 in Appendix A, in the study area, precipitation is greater and temperature is lower at higher elevations, where the snow accumulation rate far exceeds its melting rate. If there were no avalanches, precipitation would be stored more and more as snow or ice at the top of the mountain, which does not correspond to the actual situation. In the real world, avalanches allow a portion of the snow to first collapse to a lower, warmer elevation and then gradually melt.

Specifically, if avalanches are not taken into account, the effects are as follows: 1) simulated runoff will be reduced (snowmelt runoff is an important source of runoff in this region); 2) the insulation effect of snow on permafrost at lower elevations will be reduced and the area of seasonally frozen soil will increase; and 3) soil moisture and temperature at lower elevations will also be affected by reduced snow cover during freezing-thawing periods.

We rewrote the description of the avalanche and retained the following corrected sentence:

"On the QTP, variations in temperature and precipitation caused by altitude differences result in more snow accumulation and less melting at higher altitudes. Therefore, avalanches are common in this region. In this model, we established a snow thickness threshold. When the difference in snow thickness between two adjacent calculation units (the contour bands) exceeds this threshold, an avalanche will occur. The snow in the higher-altitude calculation unit slides into the next unit until the two units have the same snow thickness.”

5. 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).

Reply: In the original manuscript, we simply wanted to show that the heat conduction into soil (*G*) and the sensible heat flux (*H*) can be solved using the joint solution of equations. However, *H* is much larger than *G* and the calculation method of *H* is relatively crude, which makes this solution method unstable. Therefore, a simplified method was used in this model to obtain *G* by climate forcing first. In the revised manuscript, we found that this statement was easily misunderstood by the reader, so we deleted this part and reintroduced the method of calculating *G*.

In this study, the upper boundary of our study object 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. For periodic forcing, the heat flux into the soil could be parameterized by the sum of a temperature-derivative term and the difference between ground surface and deep soil temperature (Hu and Islam, 1995). By integrating the thermal diffusion equation, equation 11 from Hu and Islam (1995) can be obtained:

$$C_{Vu}\delta\frac{dT_u}{dt} = G(0,t) - G(\delta,t)$$

where C_{Vu} is the volumetric heat capacity of the underlying surface (MJ/m^3/°C); T_u is the temperature of the underlying surface; δ is the thickness of the underlying surface; and $G(\delta,t)$ is the heat flux at the depth δ at the moment *t*.

When δ is equal to the damping depth of the diurnal temperature wave (d_u) ($d_u = (2k/\omega)^{1/2}$ (Hu and Islam, 1995), where *k* is the thermal diffusivity of the underlying surface (m^2/s), ω is the fundamental frequency), $G(d_u,t)$ can be neglected, and the daily average heat flux conduction into the underlying surface (*G*) can be obtained by discretizing equation 11 from Hu and Islam (1995) as follows:

$$G = C_{Vu}d_u\Delta T_u$$

where ΔT_u is the daily temperature variation of the underlying surface (°C), which is approximated by the difference in temperature between the atmosphere and the underlying surface.

This is not a downgrade of the original model. The same approach was used in the WEP-COR model. It can be found in Li et al. (2019):

"The force-restore method (FRD) (Hu & Islam 1995) is used to solve *G* and the surface temperature of different land covers."

In the new revised manuscript, we have rewritten this section by adding the derivation procedure for the equations used to solve *G* and the corresponding references (Lines 278-293).

6. 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.

Reply: We apologize for the lack of clarity. We have added the exact reference in the Appendix B, Equation B14.

7. Comment 35.

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

Reply: This parameter was used in the calculation of groundwater outflow. Groundwater outflow is calculated according to the hydraulic conductivity k_b of riverbed material and the difference between river water stage H_r and groundwater level h_u (Jia et al., 2001):

$$RG = \begin{cases} k_b A_b (h_u - H_r)/d_b & h_u \geq H_r \\ -k_b A_b [1 + (H_r - Z_b)/d_b] & h_u < H_r \end{cases}$$

where A_b is the seepage area of the riverbed, Z_b the elevation of the riverbed, and d_b is the thickness of the riverbed material.

We have made the following supplements in the model structure section of Appendix B:

"The groundwater outflow was calculated according to the hydraulic conductivity of the riverbed material and difference between the river water stage and groundwater level (Jia et al., 2001)."

8. 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.

Reply: We did not intend here to say which water cycle flux change process is better.

The model performance has been discussed in Section 3.1 through the comparison of flow processes. In Section 3.3, the comparison of the changes in water cycle fluxes between the two models was only intended to explore the influence of gravel on the water cycle process.

Therefore, in order to avoid ambiguity, we change the title of Section 3.3 from:

“Simulation and comparison of watershed flow process”

To:

“Analysis of the snow–soil–gravel layer continuum effects on the process of water cycling”

Additionally, the following sentences were added before the comparative analysis:

“By comparing the hydrological cycle fluxes simulated by the two models, the influence of gravel on hydrological processes and the contribution of gravel to enhancing the simulation can be revealed, to some extent. Figure 10 shows the comparison and analysis of hydrological cycle flux changes across the basin simulated using the WEP–QTP and WEP–COR models.”

9. 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…

Reply: The surface energy balance equation was not used in the calculation of *G*, and we deleted this sentence accordingly.

10. Line 227

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

Reply: Flowing the introduction of the equation parameters, we supplemented the parameter taking values accordingly, as follows:

“Wang et al. (2013) provided a full description of the factors and parameters used in

Equation (1) (A_m = 1.45, B_m = 0.2, λ = 0.18) in this study.”

11. 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.

Reply: Lines 63-64 refer to the case of low gravel content, but the soil of the QTP generally has a high gravel content. The abundance of gravel allows the formation of many interconnected pore channels within these sediments, thus increasing their saturated hydraulic conductivity. Therefore, the result of parameter calibration was that the saturation water conductivity of the g-layer (the lower gravel and soil mixed layer) is greater than that of the s-layer (the upper soil layer).

In order to avoid ambiguity, we have rewritten this part in the introduction as follows: “As a result of the collision of the Indian and Eurasian plates, there are many gravel and rock fragments within QTP Quaternary sediments (Chen et al., 2015; Deng et al., 2019). The abundance of gravel allows the formation of many interconnected pore channels within these sediments, thus increasing their saturated hydraulic conductivity (Beibei et al., 2009).”

We also removed sentences that were likely to cause misunderstanding (Lines 63-64 and Line 222).

12. 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.

Reply: Thank you for pointing out this mistake, we have made the following corrections to this sentence:

"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 the system energy."

13. Line 300-301

Former equation 11 is now equation 9.

Reply: We have corrected this error and checked all equation references.

14. 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.

Reply: These snow thicknesses were manually measured in the field during each inspection of the experimental site, so they were discontinuous. The snow thickness of the experimental site was calculated according to the precipitation and temperature of the site through equations 4, 5, and 10, and then compared with the measured values. After validation of the actual measurements, these equations were then applied to the model to calculate the average thickness of snow in the contour band.

In Section 2.1.1, we added the method of snow thickness measurement:

"The monitoring instruments were inspected regularly during the experiment, along with manual measurement of snow thickness."

15. Line 379

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

Reply: Thank you for pointing this out. We have corrected this in the revised manuscript.

16. Line 395

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

Reply: Thank you for pointing this out. We have replaced it with new figure in the revised manuscript.

17 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.

Reply: Thank you for pointing this out. We have replaced it with new figure in the revised manuscript.

References

[1] Hu, Z., Islam, S.: Prediction of ground surface temperature and soil moisture content by the force - restore method. Water Resources Research, 31(10): 2531-2539, 1995.

[2] Li, Z., Yu, G., Xu, M., Hu, X., Yang, H., and Hu, S.: Progress in studies on river morphodynamics in Qinghai-Tibet Plateau. Advances in Water Science, 27(4): 617-628, 2016

[3] Yang K, Chen Y Y, Qin J. Some practical notes on the land surface modeling in the Tibetan Plateau[J]. Hydrology and Earth System Sciences, 2009, 13(5): 687-701.

Reviewer 2

This study developed a hydrological model (WEP-QTP) to consider soil–gravel structure (SGS) for areas in Qinghai-Tibet Plateau (QTP). The model employed different infiltration approaches to represent the impact of the dualistic SGS under fully thawed conditions, and it coupled heat and water transfer process for the snow soil–gravel layer continuum system. The model was well evaluated in a watershed in QTP regarding streamflow, snow thickness, soil-gravel temperature, and moisture. This model may be applicable in QTP to represent heat and water fluxes. However, large uncertainties exist from the forcing data, the model parameters. It is not a difficult task to obtain acceptable performance if the model was evaluated in a few sites of observations. The paper is readable, but language editing is strongly recommended to make the paper concise and professional.

Dear Reviewer:

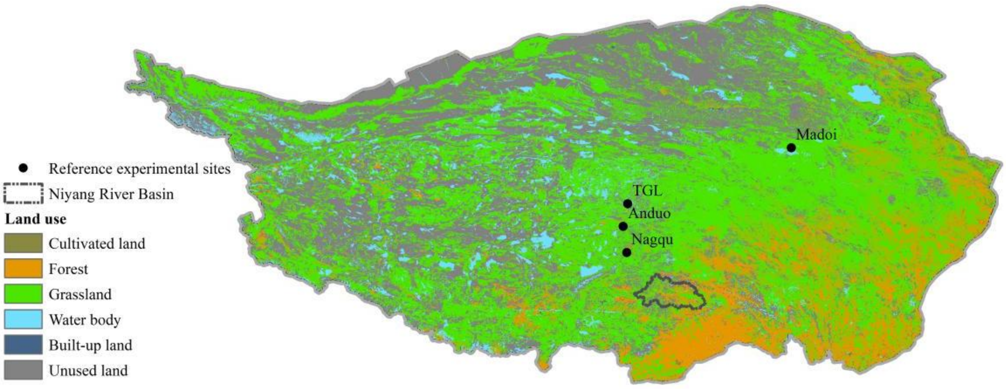
Due to the limitation of the field experiment environment on the QTP, we only used one site for model validation of the water-heat transport in soil. However, the flow process was validated using data from three hydrological stations. To make up for this deficiency, we have added the values of relevant parameters and reference notes in the revised manuscript. We thank you for your suggestion, which we will take into consideration in our subsequent study by selecting more typical experimental sites to optimize our model.

Our detailed responses to your comments are provided below.

1. The primary motivation of developing WEP-QTP is the dualistic SGS in QTP. The authors provided site-specific example of the structure (Fig. 2). But it is unclear about that SGS is extensively distributed in QTP. So the authors should discuss the extensive distribution of SGS.

Reply: The dualistic soil–gravel structure of the QTP was formed under the combined effect of long-term low temperature and plate collision, so this structure is commonly found in cold alpine regions. In these areas, physical weathering is dominant due to long-term low temperature, mineral decomposition is low, and clay content gradually decreases from top to bottom. In the plateau meadow areas of the QTP, due to the slow decomposition of biomass in the soil, the topsoil (typically 0–20 cm) in this region accumulates much denser grassroots and more soil organic matter than does the deep soil (Yang et al., 2009).

Similar soil stratification structures were also observed by Yang et al. (2009) in Anduo, Chen et al. (2015) in TGL, and Pan et al. (2017) in Madoi and Nagqu. We plotted the sampling sites of these studies and our study areas in the figure below so that you can better understand the distribution of the current stratified research on the QTP.



We also revised the introduction regarding the distribution of this soil stratification structure accordingly to make our discussion more rigorous.

The following change was made:

“In addition, under strong freeze-thaw conditions in the cold plateau region, the humus accumulation of herbaceous plants is slow, while the decomposition of minerals is weak, resulting in slow soil development on the surface of Quaternary deposits and a thin soil layer above the SGM (Deng et al., 2019; Yang et al., 2009; Chen et al., 2015; Sun, 1996).”

To:

“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).”

2. If it is true the SGS is quite extensive in QTP, but the authors did not show the thicknesses of the top-layer soil and the underlying gravel. The thickness of the two structure is important to determine the related model parameters. Certainly, model calibration will improve the model performance, but can the calibrated parameters represent the physical structure of the SGS?

Reply: For the thickness of the upper soil layer, we found through field sampling that they gradually decreases from the foot to the peak of the mountain in the study area (Lines 149-151). Higher elevations on the mountainside are generally alpine meadows with a soil layer thickness of about 40 cm. The bare lands further up the mountaintop are difficult to reach, but we speculate that they may be thinner. Therefore, in the model parameter setting, we set the soil thickness to 0.2 m, 0.4 m, and 1.0 m in the upper, middle, and lower contour zones, respectively (Lines 352-353).

For the depth of the g-layer (the lower gravel and soil mixed layer), due to the difficulty of actual measurement, we set it in the model as a multiple of the soil layer thickness, which is determined by the model parameter calibration. Its thickness is the total thickness of the aquifer and vadose zone above the impermeable boundary minus the surface soil thickness.

The calibrated model performs well in both the flow simulation and the water−heat transfer simulation. Especially for the simulation of the vertical variation of the moisture content (Fig. 3), from which we can observe that the parameter generalization of the model structure adequately reflects the actual situation (simulated soil moisture content varies discontinuously between above and below 40 cm).

Accordingly, we provided the following supplementary explanations in Section 2.2 of the manuscript:

“Under fully thawed conditions, the calculation object of water movement was defined as the dualistic soil–gravel structure (Fig. 3a). The upper layers were soil (s-layers), whose thickness was determined by the location of the calculation unit, and which gradually decreased from the foot to the peak of the mountain (Fig. 3b). The lower layers were a mixture of gravel and soil (g-layers), dominated by gravel, the thickness of which was the total thickness of the aquifer and vadose zone above the impermeable boundary minus the surface soil thickness."

3. The so-called improved model also introduced many new parameters as shown in Eqs. (1-3), for example, Am, Bm, and h. How were these parameters estimated in the application? What are the ranges of these parameters? Moreover, what about the sensitivity of the model performance to these parameters? The information regarding parameter sensitivity will be very important if the model is used in other watersheds. It would be better to provide a list of these new introduced parameters and their ranges.

Reply: For Equation 1, A_m, B_m, and λ are the empirical parameters to be estimated, and we determined the values of these parameters based on the recommended values combined with the simulation effects according to Wang et al. (2013) (A_m=1.45, B_m=0.2, and λ=0.18). Wang et al. (2013) obtained a range of values for these equation parameters by fitting water retention curves to soil mixtures with different gravel sizes and gravel contents and provided a complete description of the factors and parameters used in the equation.

We added the following supplementary notes to this equation in the revised manuscript:

"Wang et al. (2013) provided a full description of the factors and parameters used in Equation (1), A_m =1.45, B_m =0.2, λ=0.18 in this study."

For Equation 2, A_{itf} and B_{itf} are not parameters to be estimated, but intermediate parameters calculated from water content, capillary suction pressure, and hydraulic conductivity. Their specific calculation methods are shown in Appendix B by Equations B5 and B6.

For clarity, we added the following supplementary notes to the equation in the revised manuscript:

"...A_{itf} is the total water capacity of the s-layers above the interface (mm); and B_{itf} is the error caused by the different soil moisture content of the s-layers above the interface (mm). A full description of the two parameters A_{itf} and B_{itf} has been provided by Jia and Tamai (1998), and their calculation is shown in Appendix B by Equations B5 and B6."

4. WEP-QTP coupled a water-heat transfer process. Actually, a few other hydrological models (e.g., VIC) have considered this process (including the effect of freeze－thaw soil) so they are applicable to simulate hydrological processes in cold regions. What is the advantage of WEP-QTP?

Reply: These models, like the WEP-COR model, define the simulated object of the water-heat transport process as a homogeneous medium, which is applicable to the simulation of the water-heat transport process in typical cold regions. However, the underlying surface of the QTP is different from that of typical cold regions, and the soil stratification structure affects the water-heat transport process. When these models are directly applied to this region, the soil moisture content of the topsoil and the surface air temperature gradient are significantly underestimated (Yang et al., 2009).

Our study also shows that the soil-gravel layer structure in the QTP affects flow processes. Neglecting the presence of gravels will underestimate the groundwater regulation, which would be detrimental to the accurate estimation of surface and subsurface water resources in this region under future climatic conditions.

The improved WEP-QTP model takes into full account the influence of the soil-gravel layer structure on the water-heat transport process and water cycle process, and its performance is significantly improved compared with the hydrological model (WEP-COR) applicable to typical cold regions.

5. Lines 160-161, why were both LAI and NDVI used to calculate ET? The two are changeable in many cases.

Reply: In this model, LAI was used in the Penman-Monteith formula to calculate transpiration. NDVI was used to calculate the fractional vegetation cover (*Veg*), which was used as a coefficient to calculate transpiration from the dry part of vegetation leaves (Jia et al., 2001):

$$E_{tr} = Veg(1-\delta)E_{PM}$$

where E_{tr} is the transpiration from the dry part of vegetation leaves; δ is the fraction coefficient of the foliage covered by a water film; and E_{PM} is the Penman–Monteith transpiration.

6. Lines 190-193: was the hydrological process in farmland improved in the study? Does this study area contain farmland?

Reply: Yes, farmlands are generally located at the foot of mountains where there is a thicker soil layer, and these areas were also improved as a calculation unit with soil thickness of 100 cm. However, the share of farmland is relatively small, accounting for only 1% of our study area.

7. What is the difference between subsections 3.1 and 3.2? Both of the subsections described the comparison of streamflow.

Reply: I think you may be confused by the difference between sections 3.1 and 3.3 (and not section 3.2, which presents a comparative analysis of soil temperature and moisture). Section 3.1 only introduces the parameters of model calibration and the simulation results. Section 3.3 is designed to explore the effect of the snow-soil-gravel layer continuum on the water cycle processes by comparing the simulated differences between the hydrological cycle flux before and after the model improvement.

To better organize the paper, we have changed the title of Section 3.3 from:

“Simulation and comparison of watershed flow process”

To:

“Analysis of the snow–soil–gravel layer continuum effects on the process of water cycling”.

Also, we have added the following exposition at the beginning of this section:

“To explore the influence of the snow–soil–gravel layer continuum on the process of water cycling and the reasons behind the improvement of the model simulation, 2014 (a year for which all measured data were available) was selected as a typical year to compare and analyze the simulation results before and after model improvement (Fig. 9).”

8. Please provide legends for the lines in Figs. 7, 8.

Reply: Thank you for pointing out these errors. We have replaced these figures with new figures in the revised manuscript.

9. Please give language editing to make the paper concise professional.

Reply: Thank you for your suggestion. The language of the revised manuscript has been improved by a native English speaker/professional science editing service.

References

[1] Chen H, Nan Z, Zhao L, et al. Noah modelling of the permafrost distribution and characteristics in the West Kunlun area, Qinghai - Tibet Plateau, China[J]. Permafrost and Periglacial Processes, 2015, 26(2): 160-174.

[2] Jia Y, Ni G, Kawahara Y, et al. Development of WEP model and its application to an urban watershed[J]. Hydrological Processes, 2001, 15(11): 2175-2194.

[3] Pan Y, Lyu S, Li S, et al. Simulating the role of gravel in freeze - thaw process on the Qinghai - Tibet Plateau[J], Theoretical and applied climatology, 2017, 127, 1011-1022

[4] Yang K, Chen Y Y, Qin J. Some practical notes on the land surface modeling in the Tibetan Plateau[J]. Hydrology and Earth System Sciences, 2009, 13(5): 687-701.