Referee 1

I thank the authors for taking well of my suggestions, although I am still not fully convinced with the definition of surface and sub-surface runoff in this paper. The definition to separate surface and subsurface is important to draw one of the conclusions that "ground warming drives a strong increase in subsurface runoff", which is important for Tibetan Plateau hydrology study and decision making.

We are grateful for the reviewer's comments and we understand his will to make precise and clear statements regarding surface and subsurface runoff, as well as connections with ground warming. It is also something we are aiming for with this study. With our model, we consider that surface runoff occurs when precipitation falls over a saturated ground. In comparison, subsurface runoff occurs for a soil water content lower than saturation, when the soil water content is above field capacity, because the slope drains it towards the lake. These approaches can be found in various publications (Samuel et al., 2008; Vörösmarty et al., 1989; Shaman et al., 2002; Kelleners et al., 2010; Kampf, 2011) and are consistent with field observations (Lai et al., 2018). We made clarifications in the text as detailed at the end of this answer.

If more water goes to subsurface, with the same amount of total runoff, surface runoff will decrease. Surface runoff is largely related to sediment transport, thus decreased sediment yield can be expected in the case. However, dramatic increase of sediment yield was observed on the TP (https://www.science.org/doi/10.1126/science.abi9649). These two conclusions (more subsurface runoff and increase of sediment) seem like a paradox.

We thank the reviewer for this interesting perspective on the distribution between surface and subsurface runoff and the connection with sediments. As pointed out by the reviewer, the paradox he mentions might only occur for a constant level of total runoff over time. Yet we believe that such an hypothesis on the total runoff might not be the most relevant given the climate changes observed and forecasted for High Asia. Indeed, the study from Li et al. (2021) mentioned by the reviewer suggests that climate change in High Asia implies a precipitation increase over time, which contributes to an overall increase in runoff. At the scale of the Paiku catchment, we report the same trend : an increase in precipitation that is strong enough to drive a concurrent increase of surface and subsurface runoff. To acknowledge connections between sediment transport and runoff in the study we added the following line to Sect. 5.2.2. (Evaporation and runoff changes):

"These increases in runoff (especially surface runoff) are likely to have an influence on sediment transport. For instance, Li et al. (2021) showed that current precipitation augmentation over High Mountain Asia is driving a runoff increase, which contributes to a significant rise in fluvial sediment fluxes."

I agree that the surface and subsurface runoff are well connected, and in many cases are hard to be isolated. The key question might be "why do we need to split it apart?", since it is an integrated system in nature. The authors can find more detailed discussion about this issue and relevant topics in our HESSD Opinion paper (https://egusphere.copernicus.org/preprints/2023/egusphere-2023-125/). I don't want to use this reason to stop the publication of this paper. For me, the authors did lots of modeling work, and this is an excellent case study on frozen soil hydrology modeling in a small catchment on the TP. I'd like to suggest further improving the description and discussion on this critical issue before acceptance.

We were not aware of this discussion and we thank the reviewer for pointing it out. We will keep in mind the possibility of an environment-driven approach rather than a soil-driven approach in the future. We believe that this is an open scientific debate and we are happy to bring here some arguments supporting our approach. We see several reasons to separate surface and subsurface runoff that we develop in the new paragraph we added to the discussion to account for the reviewer's comment. To summarize it, we believe that surface and subsurface flow have contrasted behaviors that reflect the different physical mechanisms that drive them, both hydrologically and thermally. And it seems to us that this is confirmed by field observations on Tibetan hillslopes (Hu et al., 2020). In turn, these contrasted behaviors have consequences on the rest of the hydrosystem, on the landscape and on the environments and for this reason we believe it is a relevant distinction for our work.

To acknowledge the suggestion of the reviewer to further improve the description and the discussion , the main text has been modified as follows.

In *Model Setup and Validation* (Sect 3.2.4):

"For this study, we rely on a simple approach that is based on thresholds regarding the soil water content (porosity and field capacity). This kind of approaches <u>are thus based on soil properties and have been often</u>

used in hydrological modeling studies (Vörösmarty et al., 1989; Shaman et al., 2002; Kelleners et al., 2010; Kampf, 2011; Samuel et al., 2008). In detail, we compute surface and subsurface flow as follows."

In *Modeling Strategy* (Sect 5.1.2), entirely new paragraph:

"Additionally, our approach regarding the modeling of runoff is relatively simple, i.e. partition between subsurface and surface runoff based on comparison between the soil water content and field capacity and porosity, respectively. More complex approaches split runoff into more sophisticated categories such as Horton overland flow, Dunne overland flow, subsurface stormflow… (e.g. Savenije, 2010; Gao et al., 2014; Mirus and Loague, 2013). However, over the last decade, the relevance of this type of partitioning between different types of runoff has been questioned (McDonnell, 2013; Gao et al., 2023) . In the frame of our study, we find it important to distinguish between surface and subsurface runoff because they generate flows with very contrasted speed. In a general perspective, this significant difference in flow velocities impacts the hydrological system as a whole (e.g. river discharge, evaporation…) and has various consequences throughout the catchment, such as the water availability for vegetation, erosion and sediment transport.

In the particular case of a cryo-hydrological study, separating surface from subsurface runoff is particularly relevant because both flows do not react in the same way to ground temperature changes. As such, we see our approach as a middle way that allows us to make this distinction based on simple hydrological considerations. Yet, we acknowledge that the classification and quantification of the different types of runoff represent a valuable direction for future investigation on catchment-scale cryo-hydrology in Tibet."

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Referee 2

Major comments:

1. The title and abstract of this manuscript are likely to focus on whole Tibetan endorheic catchments. However, the major contents of the article focused on a typical lake with seasonal frozen ground, which is different from the most lakes on the north of the QTP with permafrost. Thus, the title and abstract should be revised to match the content of the article.

Our study focuses on the Paiku catchment and lake. The title mentions "*a Tibetan endorheic catchment*", which indicates that it is a catchment scale study. Similarly, the abstract says "*This study focuses on the cryo-hydrology of the catchment of Lake Paiku*". Broader topics are tackled in the introduction and the discussion in order to connect our work with the rest of the literature and broad scientific questions, which we believe to be a common practice in scientific articles.

To make things even clearer we added the following elements to the title and abstract :

Title

"Recent ground thermo-hydrological changes in a Southern Tibetan endorheic catchment and implications for lake level changes"

Abstract

"Although the present study was performed at catchment scale, we suggest that this ambivalent influence of permafrost may help to understand the contrasting lake level variations observed between the South and North of the QTP, opening new perspectives for future investigations."

2. There are two critical assumptions that may cause larger errors and imbalance of water for the lake: (1) The water flows between the lake and potential aquifers surrounding are negligible due to it is difficult to quantify these flows. Because there is undoubtedly more potential water flows in nature, it is necessary to deeply discuss the errors or uncertainty caused by this assumption.

We acknowledge the point of the reviewer on lake aquifer-interactions. For this study we developed a rather complex hydrological framework and this process is indeed absent from it. Additionally, we have no way to quantify the magnitude of these interactions and we think investigations in this direction would go beyond the scope of our study. Regarding the discussion, we have now extended the paragraph we had on this topic in sect. 5.1.3 (Reconstruction of the Lake hydrological budget and level variations). It now reads:

"A possible reason for this mismatch is that the lake is connected to a larger aquifer that surrounds it. In the context of a decreasing lake level, an aquifer surrounding the lake can create an additional water inflow when the lake level passes below the piezometric level of the aquifer (Yechieli et al., 1995). Such an inflow could mitigate the lake level decrease and thus explain the missing water in our reconstruction (Fig. 6B). It could also explain the gradual stabilization of the lake level that our model does not reproduce. This flow is not part of our conceptual hydrological framework even though it likely exists in reality, especially since there is no permafrost near the lake (as we simulate it here), allowing for the existence of such an aquifer (Walvoord and Kurylyk, 2016). Groundwater has been identified as a potential contributor to lake level rise in other regions of the QTP (Lei et al., 2022). In the long run, lake-aquifer systems commonly follow oscillations of the net atmospheric flux of water (Precipitation – Evaporation) and of the runoff that forces its mass balance (Watras et al., 2014). During these oscillations, the lake can "pump" water from the aquifer or feed it depending on the relative difference of piezometric level between them (Almendinger, 1990; Liefert et al., 2018). Yet, this potential effect is difficult to account for and its magnitude remains unclear. Therefore, the reasons for the mismatch between observed and simulated lake levels could also be connected to other aspects of our methodology such as bias in the climatic forcing data and other shortcomings arising from the lack of field data, or hydrological processes, as developed in Sect. 5.1.1 and 5.1.2."

(2) Because vegetation is very scarce in the catchment, the vegetation transpiration is ignored in evapotranspiration. I think this assumption may cause more errors and uncertainty. There were some researches reported that alpine meadow or swamp meadow with high evapotranspiration are distributed in a certain range around a lake and along a river, and vegetation transpiration and interception of alpine meadow and swamp meadow could account for 30-40% of the total evapotranspiration of the grassland area on the plateau.

We understand the reviewers' concern on transpiration, yet we want to insist on the fact that vegetation is extremely scarce in the catchment. During our field trip, we noted that most of the catchment corresponds to barren lands (Fig. R2.1) and that vegetation is limited to very sporadic herbaceous cover. This assessment is confirmed when looking at satellite images of the catchment, which do not show any noticeable vegetated

area, as well as NDVI values that correspond to barren land in the region (Liu et al., 2022; Mao et al., 2022). Therefore, we believe we have a meaningful argument to support the idea that transpiration is very unlikely to have a strong imprint on evapotranspiration in the catchment. We present below a photo that shows the type of land cover we found on the field that supports this assumption.

Figure R2.1. Ground photo in the Paiku catchment in November 2019 (Credit: Fanny Brun).

Additionally, the evaporation rates we report are extremely high (nearly 90% of the precipitation reaching the ground is evaporated in our historical scenario). Thus we think it is quite unlikely that our simulations underestimate evaporation. Nevertheless, to acknowledge the reviewer comments we added the following to the discussion in the *Modeling strategy* part (Sect. 5.1.2):

"Another potential improvement in our modeling approach could be to unravel evaporation from transpiration. However, since vegetation is extremely scarce in the Paiku catchment, which is largely dominated by barren lands, we suggest that this would not significantly affect our results. However, this limitation should be explored in future field and modeling studies."

3. The data used in model verification is too less to meet the requirements of model parameter calibration and simulation result verification. In a catchment area of 2 400 km2 with permafrost and glaciers distributed in high altitude areas, there is only one observation point at seasonal frozen ground to support the data of temperature, precipitation and ground temperature required by the CryoGrid community model simulation. Also, there are no observations in the lake area to use for lake water balance analysis.

We understand the reviewer's comment and we are aware that the amount of observations we have to frame our simulations is an important question for our study. Yet, we need to correct what is said here. On top of the data from our automatic weather station, our ground temperature loggers and geodetic mass balance reconstructions, we have access to very precise lake level observations from Lei et al. (2018, 2021). So in the end, we use all observations available, including lake level, which bring us observations on the water balance. In turn, any study in the Paiku catchment will have the same limitations. To clarify the existence and the use of the lake level data we modified the last paragraph of Section 2 (Study area: the Paiku catchment).

"More recently, the lake level decreased by 3.7 m between 1972 and 2015, losing 4.2% of its surface and 8.5% of its volume. <u>Measurements have been performed since the end of the 1970s and allow to accurately know the evolution of the lake level until today (Lei et al., 2021, 2018), they are used in this study to validate our hydrological results (Sect 3.2.1, Fig. 5D and 6B)."</u>

Additionally, none of our modeling works are driven by these lake observations, we use them to compare our simulated lake balance with the observed one, which provides validation to assess the model performances. In this regard, our calculations produced 95% of the runoff required to reproduce the lake variations, indicating that the magnitude of our reconstruction is correct. From the thermal point of view, on top of reproducing logger temperature values, our simulations find a very good consistency with larger scale studies covering the same area (such as studies tackling permafrost coverage, Sect. 5.2.1). Considering that our approach tackles water and heat flows and in a coupled and interdependent way, we think that providing this two-fold agreement with observations and other works brings confidence in the robustness of the reported results. To clarify this role of the lake observations we modified the last paragraph of Sect. 3.2.1 (Conceptual hydrological model for the catchment):

"Our catchment-scale approach to represent the hydrological balance of the lake is summarized in Fig. 2. <u>Based on this approach, we can evaluate the performance of our framework (Sect. 4.1.2), by comparing the simulated lake balance with the one derived from the detailed observations of lake level variations over the study period (Lei et al., 2018, 2021)."</u>

Additionally, we want to point out that, even though Tibet deserves major attention from hydrologists and cryosphere scientists, it is very challenging to acquire data in an area like the catchment of the Paiku lake.

It is a very remote region, hard to access for logistical reasons, even for Chinese scientists. And in this particular context, the COVID pandemic made it even harder to access the field. This limited our ability to collect more field observations. We are fully aware that our analysis contains large uncertainties and have therefore included a detailed discussion section (Sect. 5.1.1 Data usage within the conceptual framework and data scarcity, which used to be even more extensive before the reviewer 1 of the first review round recommended to shorten it) in which we present different possible interpretations of our modeling result, in the light of the sparse available observations. To clarify this point we amended the last paragraph of this section:

"A main limitation regarding our usage of the data is related to the limited amount of available field observations required to provide robust model parameterizing, climate forcing and in-depth validation of the simulations, both hydrologically and thermally. Regarding climatic forcing data, our AWS measurement offers sound observations to evaluate and adjust the ERA5 data processed with TopoSUB and downscaled with TopoSCALE. Yet, a period of observations longer than 2 years would have enabled more robust corrections and could have allowed us to perform a more advanced statistical downscaling approach, e.g. quantile mapping (Themeßl et al., 2011). As such, the spatiotemporal domain of relevance of these corrections is insufficient to correct data for the whole catchment and the 40 years of simulations. Overall, considering the strong bias we observe in the raw ERA5 data (Figure D0), these corrections do represent an important first-order improvement. Altogether, this scarcity of field observations is likely to bring significant uncertainties to our analysis. Future efforts should focus on acquiring additional data or developing validation methods based on remotely sensed observations."

As indicated in the paper, based on all these considerations, we believe that the following conclusions can be drawn in the light of this uncertainty:

- **Lost of the permafrost extent (20% loss)**
- **Average ground warming around 1.7°C per decade (at 2m deep)**
- **Increased duration of seasonal thaw (mainly due to later end date of the thaw period)**
- **Evaporation acts as an energy sink limiting active layer deepening**
- **Increase in evaporation, surface and subsurface runoff**
- **Increase of the runoff/(runoff+evaporation) ratio**
- **Connections between permafrost disappearance and subsurface runoff increase**
- **Increased availability of liquid water in the ground connected to higher evaporation rates**
- **Precipitation increase drive a concomitant increase of runoff and evaporation**
- **Potentially ambivalent influence of permafrost on evaporation, that seems to be climate-dependent.**

Altogether, we believe that our results shed light on important cryo-hydrological trends that have the potential to foster new research and improve our understanding of the impact of climate changes on High Mountain Asia and particularly on the understanding of the lake variations across the QTP.

Minor comments:

Abstract: Larger part related to the introduction, however, some key results and conclusion are deficiency.

We believe it is natural to connect the detailed work we did in the Paiku catchment with larger scale scientific questions. Therefore the first paragraph of the abstract aims at framing our study and demonstrating its relevance. Based on the reviewer comments, we have shorten this paragraph, which now reads:

"Climate change modifies the water and energy fluxes between the atmosphere and the surface in mountainous regions such as the Qinghai-Tibet Plateau (QTP), which has shown substantial hydrological changes over the last decades, including rapid lake level variations. The ground across the QTP hosts either permafrost or seasonally frozen and, in this environment, the ground thermal regime influences liquid water availability, evaporation and runoff. Therefore, climate-driven modifications of the ground thermal regime may contribute to lake level variations, yet this hypothesis has been relatively overlooked until now."

The final part of the abstract is based on the results and discussion sections. To make this clearer, we modified the following in the abstract:

"Our results show that both seasonal frozen ground and permafrost…"

L25-27: The last sentence is not suitable and lack of sufficient supports.

This last sentence of the abstract summarizes considerations from the discussion where we try to be prospective and aim at connecting our study with broader questions. To make this clear, we now phrase it

with even more caution. The new version is already quoted higher up, in the answer to the major point number 1.

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Referee 3

In the manuscript titled "Recent ground thermo-hydrological changes in a Tibetan endorheic catchment and implications for lake level changes", the authors performed a physical land surface model and quantify thermo-hydrological changes in the Paiku catchment in the Qinghai-Tibet Plateau. This study contains some interesting findings and are valuable for the understanding of climate-driven ground thermal changes on hydrological cycle in alpine basin. However, the structure of the manuscript needs improvement. Therefore, a major revision is needed before this manuscript could be accepted for publication.

We are grateful for the positive perception of our work.

Major comments :

1. The results of section 4.1 of the manuscript show that glacial runoff exceeds land runoff, suggesting that the literature review of study on glaciers together with glacial runoff in the basin should be added to the introduction section. And how can you get the initial glacier ice volume for the simulation?

We modified and extended the state of the art regarding glacier mass loss and glacier runoff over the QTP in the introduction with new references:

"Overall glacier shrinkage has also been observed since the 1960s with a persistent increase in glacier mass loss rates (Bhattacharya et al., 2021; <u>Hugonnet et al., 2021</u>)."

"The majority of these lakes have experienced a pronounced increase in water levels since the 1990s (Lei et al., 2013, 2014), a trend that was suggested to be mainly driven by changes in precipitation and evaporation patterns (Yao et al., 2018) <u>rather than by an increase in glacier mass loss and runoff (Brun et al., 2020; Zhang et al., 2021)</u>."

Additionally, a state of the art of glacier changes in the Paiku catchment is present in Sect. 3.2.6 (Quantification of glacier mass change). Following the reviewer's comment, to include glacial runoff, we added the following <u>new sentences</u> to this part:

"Regarding glacial runoff, it was estimated to 320 ± 4 mm per year for the 2001-2010 period by Biskop et al. (2016) using a temperature-index approach for ice melt. For the 2000-2018 period, Zhang et al. (2020) derived a runoff value of 52 ± 12 mm per year (1.24 ± 0.29 10^8 m^3 per year that we scaled to the basin area). The value we derive of 39 ± 13 mm per year thus finds good consistency with the latter one (Sect. 4.1)."

Regarding the initial glacier volume, we want to point out that our study does not include glacier simulation. Glacier volume change is derived from geodetic data. Glacier runoff is derived from the glacier volume change calculation and the precipitation from the climate forcing data. Concerning the initial volume of glacier ice, we did not need it because the volume change is directly derived from the difference in elevation change obtained from the DEMs and the area of the glacier.

2. "Result" section : It is proposed that section 4.4 be merged into section 4.1.

We followed the recommendation of the reviewer and merged section 4.4 into section 4.1. We do not reproduce the text here because it is an extensive and relatively straightforward change.

3. "Discussion" section : The scenario experiment reveals the main findings and it is recommended to put the scenario experiment in the results section.

We followed this recommendation, the results of this experiment are now presented in the new section 4.4 (Sensitivity test on evaporation and runoff). What we considered was discussion (and thus not relevant within the Results part) was kept within the discussion part (Sect. 5.3.Evaporation vs runoff and sensitivity to climate conditions).

4. Section 5.3 : It is suggested to add a table to give the value of runoff, evaporation, and precipitation in each permafrost region under two scenarios;

We added the suggested table to this section (which is now section 4.4 after the modification from Major Comment 3).

5. please explain Figure 11C specifically.

We included in the new Section 4.4 the following explanations:

"Figure 10C aggregates over the whole catchment this distribution of this precipitation input to the ground between runoff and evaporation for both scenarios. In between them, it also includes the distribution associated with the steady lake level scenario of Fig. 9C, which is based on the hypothesis listed as bullet points in Sect. 4.3 (climate forcing of the historical scenario, same glacier contribution, only land runoff increases)."

Minor comments

1. Line 2-12 : You can summarize these sentences into 2-3 short sentences.

We shortened the first paragraph of the abstract to the following:

"Climate change modifies the water and energy fluxes between the atmosphere and the surface in mountainous regions such as the Qinghai-Tibet Plateau (QTP), which has shown substantial hydrological changes over the last decades, including rapid lake level variations. The ground across the QTP hosts either permafrost or seasonally frozen and, in this environment, the ground thermal regime influences liquid water availability, evaporation and runoff. Therefore, climate-driven modifications of the ground thermal regime may contribute to lake level variations, yet this hypothesis has been relatively overlooked until now."

2. Line 14-15 : "We use TopoSCALE and TopoSUB to downscale ERA5 data and capture the spatial variability of the climate in our forcing data". This sentence is excessive.

We rephrased for:

"We use TopoSCALE and TopoSUB to downscale ERA5 data, in an effort to account for the spatial variability of the climate in our forcing data."

3. The title has the "implications for lake level changes", but why does the abstract not reflect the results of the study on the water level?

We modified the abstract and from now on, references to the lake level variations appear in 3 different places :

"This study focuses on the cryo-hydrology of the catchment of Lake Paiku (Southern Tibet) for the 1980-2019 period. We use TopoSCALE and TopoSUB to downscale ERA5 data, in an effort to account for the spatial variability of the climate in our forcing data. We use a distributed setup of the CryoGrid community model (version 1.0) to quantify thermo-hydrological changes in the ground during this period. Forcing data and simulation outputs are validated with weather station data, surface temperature logger data and observations of lake level variations. Our lake budget reconstruction shows that the main water input to the lake is direct precipitation (310 mm per year), followed by glacier runoff (280 mm per year) and land runoff (180 mm per year). However, altogether these components do not offset evaporation (860 mm per year).

Our results show that both seasonal frozen ground and permafrost have warmed (0.17 °C per decade 2 m deep), increasing the availability of liquid water in the ground and the duration of seasonal thaw. Correlations with annual values suggest that both phenomena promote evaporation and runoff. Yet, ground warming drives a strong increase in subsurface runoff, so that the runoff/(evaporation + runoff) ratio increases over time. This increase likely contributed to stabilizing the lake level decrease after 2010.

Summer evaporation is an important energy sink and we find active layer deepening only where evaporation is limited. The presence of permafrost is found to promote evaporation at the expense of runoff, consistent with recent studies suggesting that a shallow active layer maintains higher water contents close to the surface. However, this relationship seems to be climate-dependent and we show that a colder and wetter climate produces the opposite effect. Although the present study was performed at catchment scale, we suggest that this ambivalent influence of permafrost may help to understand the contrasting lake level variations observed between the South and North of the QTP, opening new perspectives for future investigations."

4. Line 23-24 : "consistent with recent studies" is proposed to be excessive.

We added some precision to clarify which studies we were mentioning and thus avoid ambiguity:

"The presence of permafrost is found to promote evaporation at the expense of runoff, consistent with recent studies suggesting that a shallow active layer maintains higher water contents close to the surface."

5. Line 148-150 : "It reached 4665 masl (85 m higher than the present level) prior to 25 ka BP and at the onset of the Holocene (11.9-9.5ka BP)", references to the study should be marked.

The long term evolution of the lake level was spread over this sentence and the next one, which carries the bibliographic reference. But based on the suggestion from the reviewer, and to make things clearer, we merged the 2 sentences. The description of the long term evolution now reads:

"Previous studies reported lake level fluctuations over different time scales. It reached 4665 masl (85 m higher than the present level) prior to 25 ka BP and at the onset of the Holocene (11.9-9.5 ka BP), afterwards, the lake shrank gradually (Wünnemann et al., 2015)."

6. Line 236: Please check the number "510-3", perhaps it is "0.005".

We replaced 5 10^{-3} by 0.005.

7. Figure 7(A): Please mark the start date.

We thank the reviewer for noticing this typo. We corrected it and the gray bottom curve is now labeled "start date".

8. Line 447: It is recommended that "AL thickness" be modified to "ALT".

We did this modification. To be consistent, we also applied it to previous and following occurrences of the "active layer thickness".

9. Line 644: It is suggested to change "AL" there to "Active Layer (AL)", and the abbreviation will be logical afterwards.

We implemented this change.

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