We are very grateful to Reviewer 1 for the in-depth reading and for the thorough review we received. We present below our detailed answer to the discussed points. The reviewers' comments appear in black and our responses appear in brown. Quotes from the manuscript are in *brown italic*, modified parts from the revised version are ***in bold font***.

Review 1

The manuscript by Martin et al. describes the application of a coupled hydro-thermal modeling approach to a high-altitude catchment. The authors relate long-term lake level variations in an endorheic basin of Lake Paiku, Southern Tibet, to changes in both water balance and permafrost distribution across the catchment (finally, and this is my major concern, the manuscript lacks such relation). This modeling effort is based on ERA5 reanalysis data as driving climate forcing, downscaled and distributed across the homogenous response units with TopoSCALE/TopoSUB, the CryoGrid3-Flake as lake module and a distributed CryoGrid CM model as basic hydro-thermal model for both permafrost and hydrology in the basin. The results presented in the manuscript is scientifically sound and the obtained results enhance our understanding of permafrost hydrology and change in a high-altitude catchment with limited direct anthropogenic pressure. However, the interpretation of the results, or what exactly our understanding gains, is not of immediate evidence to me because of the issues raised below, and I suggest this manuscript is subject to major revision.

The evidences of both cryologic and hydrologic change are presented, but overall reasoning behind the conclusions is unconvincing from the hydrological perspective. First, the manuscript, since its title, aims at relating cryohydrologic change to the Paiku Lake level

We understand the reviewers' concern. As detailed below in our answers, we have further processed our simulation results to be able to present the lake hydrological budget and discuss it. Therefore, we hope that the new results, figures and discussion developed along this line make the title more consistent with the manuscript content.

– nonetheless, the simulated lake level data are not presented in the manuscript. Figure 5D showcases the runoff needed to close the lake water balance based on observed data, but it might be useful to covert runoff directly to lake level fluctuations, given that stage-volume relation is known to the authors.

Regarding Fig. 5, our intention is to provide a validation figure. Connecting the simulated runoff to observations is important for our study considering our focus on land cryo-hydrology. Yet, we understand the reviewer's comment regarding the need for more results to present and discuss the lake hydrological budget. Modifications in these directions are presented in the new manuscript and in the response to the next comments.

The manuscript beyond Section 4 discusses secondary effects without relating them to the modeled lake level change; this is the reason why finally Section 5.4 'Implications for lake level change' is so faceless and merely doubles the Section 6 'Conclusions'.

In the revised version of the manuscript, we now present our results on the lake budget in a new section (Sect. 4.4, 4.4. Hydrological budget of Lake Paiku) with a new figure (Fig. 9). The new text and figure show the reconstructed lake level and the different terms of the lake budget in m of lake level change over the simulation period:

"4.4. Hydrological budget of Lake Paiku

Our observations, climate data, simulations, geodetic data and the lake level data from Lei et al. (2018, 2021) enables us to quantify the different terms of the lake hydrological budget. We present these results in m of lake level change based on the average slope of the Volume = f(level) relationship (Fig. 9). As the unique output term, evaporation dominates the lake budget with an average annual value of 0.86 m (34.6 m / 40 years, Fig. 9A). Direct precipitation in the lake is the dominant input with an average annual value of 0.31 m (12.3 m / 40 years), followed by glacier runoff (0.28 m/yr, 11.3 m / 40 years) and land runoff (0.18 m/yr, 7.0 m /40 years). When compared with lake volume observations over the 40 years of the simulation period, the simulated lake budget is 1.04 m too negative.

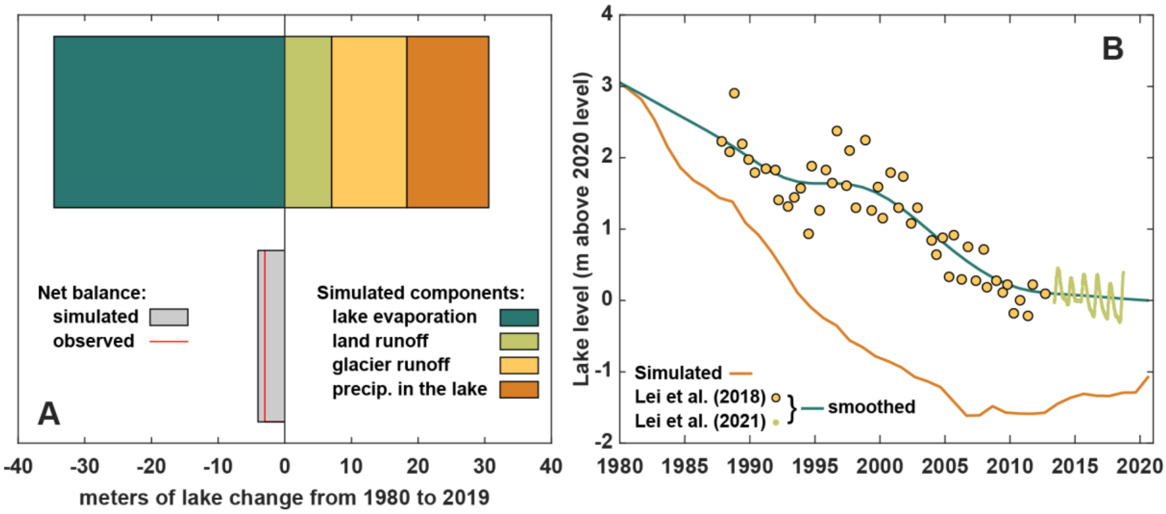

Figure R1.1 (Fig. 9 of the revised manuscript) Budget and level of lake Paiku for the simulation period (1980-2019). A. The different components of the hydrological budget of the lake according to our framework. Results are given in m of lake change based on the average slope of the Volume = f(level) relationship. B. Lake level data. Points correspond to observations from Lei et al. (2018, 2021) that we smoothed (green curve, based also on observation points older than 1980). The simulated lake level appears in orange.

Based on our results, we also reconstructed lake level variations that we compare with the observed variations (Fig. 9B). Following our framework, our values are presented at an annual timestep. They qualitatively reproduce the overall lake level decrease but tend to overestimate this decrease and show an increasing mismatch with the observations from 0 in 1980 to 2 meters in 2005. This mismatch is later compensated by an increasing lake level trend in our simulation from 2005 to 2019. At the end of the simulation period, the mismatch is 1.04 m, consistent with the budget values (Fig. 9A) and the fact that our approach provides 95% of the required runoff to close the lake budget (Sect. 4.1.). This pattern of a too strong decrease followed by an increase is consistent with the comparison between simulated and required runoff presented on Fig. 5D."

The revised manuscript now also includes a discussion section about these new results (Section 5.1.3. Reconstruction of the Lake hydrological budget and level variations).

"5.1.3. Reconstruction of the Lake hydrological budget and level variations

The total lake level change we simulate is a decrease of 4.11 m. This is qualitatively consistent with the overall observed trend. The mismatch with the observations is limited to a 1.06 m excess in the simulated level drop (Fig. 9A). Our reconstruction shows a decrease of 4.66 m from 1980 to 2007, which is an overestimation of the initial drop. Afterwards, while observations indicate a gradual slowdown of the lake level decrease, we simulate a stabilization followed by a slight increase (0.55 up between 2013 and 2019).

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. Such an inflow could mitigate the lake level decrease and thus explain the missing water in our reconstruction. 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). Ground water has been identified as a potential contributor to lake level rise in other regions of the QTP (Lei et al., 2022). 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.

Our reconstruction of the lake budget is informative regarding the respective contribution of the different inputs and outputs. Regarding lake evaporation, our mean value of 870 ± 23 mm is close to the one modelled by Yang et al. (2016) with the Flake model for lake Nam (832 ± 69mm) for the period 1980-2014 but we do not report a significant increasing trend in our results. Yet for the same lake (Nam Co) and a similar period (1980-2016) Zhong et al. (2020) reported an average value of 1149 ± 71 mm (along with an increasing temporal

trend) using the Penman formula (Penman, 1948), thus highlighting the potential dependence of the results to the methodology. In our results, direct precipitation to the lake represents 40% of the inputs, followed by glacial runoff (35%) and land runoff (25%). Glaciers are therefore a particularly important contributor to the runoff towards the lake (60% of the total runoff, vs. 40% for land runoff), what contrasts with the results from Biskop et al. (2016) who calculated that the runoff input to the lake Paiku was dominated by land runoff (70% and 30% for the glacier contribution). Here again, these difference likely arises from important differences in input data and methodologies to quantify the different hydrological processes (evaporation, runoff, snow and glacier melt). Yao et al. (2018) reported that, at the QTP scale, the balance between precipitation and evaporation (over land and lake) was dominant over glacier melt to understand both lake storage increases and decreases. Our reconstruction does not give us access to significant temporal variation of the glacier contribution but the above-mentioned proportions in the contributions to the lake (40%, 35% and 25%) show that the glacier contribution does not dominate the input terms. At the catchment scale, these proportions can vary significantly depending on the glacier coverage. For Lake Selin, Zhou et al. (2015) reported that runoff towards the lake, evaporation from the lake and on-lake precipitation altogether explained 90% of the lake storage variations for the 2003-2012 period. The catchment of lake Selin exhibits a very limited glacier coverage (0,63% of its area, Lei et al., 2013) compared to the Paiku (5%).”

Moreover, following the comment on the difference between correlation and causation, we moved the part on connections between the ground thermal regime and the hydrology. It was initially presented in the results, it is now presented in the discussion (section 5.1.4. of the new manuscript) and fully reworked (detailed answer on this point below).

Second, behind all modeling exercises, lake level variations in an endorheic basin ΔH are described by a three-member water balance equation, where ΔH is on the left, and on the right, the three members are: (1) lake surface balance, described as a (P-E) term, (2) catchment runoff, split into river runoff and side inflow. In this equation, when all the members are conditionally known, the unmeasured components, e.g., loss to the deep subsurface through infiltration, can be deduced. This basic hydrological approach has only limited use in the manuscript, i.e., in the sections where water balance components are presented and discussed, there is always a component that is missing, so that overall catchment water balance cannot be closed through mental calculation. See, e.g., Section 3.2.1, Section 4.1.

The new section 4.4. and Fig. 9 now detail the different components of the hydrological budget of the lake. Regarding deep subsurface infiltration, we believe that we cannot use the method provided by the reviewer because the data we have limits us in the way we can validate our simulations. Our understanding of the reviewer’s suggestion is to say:

Δlake_level = lake_precipitation – lake_evaporation + land_runoff + glacier_runoff – deep_infiltration

We have real world controls on: the lake level (observations), the precipitation (AWS data), the evaporation (Lei et al., 2021), the glacier runoff (geodetic observations) but neither on the land_runoff nor on the deep_infiltration. From there, having to quantify two variables is problematic. As discussed earlier, we decided to compute an “observation-derived” land_runoff to control our simulated runoff because it appeared important for the study. To do so, we had to assume losses to the deep subsurface to be null (as well as any type of lake-aquifer interactions). It is one of our working hypotheses to be able to implement our approach. We see that this hypothesis was not detailed enough in the initial manuscript so we modified it to clarify this point in section 3.2.1 (Conceptual hydrological model for the catchment):

“Because the quantification of water flows between the lake and potential aquifers surrounding it is difficult (Rosenberry et al., 2015), our approach assumes that these flows are negligible.”

We also added a mention to this hypothesis in the discussion (pasted in the answer to the previous part).

It is sufficient to give long-term values for 1980-2020, and show how the balance is not closing and why; then how permafrost thaw promoted subsurface runoff (or ground ice thaw?) to finally stabilize the lake water level around its present reference level. Or the like. I don’t know.

We are not sure we fully understand what the reviewer means. The calculation we did for Fig. 5D shows indeed that a term of land runoff is needed to close the lake budget (i.e. lake_precipitation – lake_evaporation + glacier_runoff is not enough) but we believe that this can be true regardless of the state of the permafrost in the catchment. Given the data we have access to, we think that deriving the land runoff from the lake level variations (and other variables) is the best we can do and that this value alone does not give access to information on the role of permafrost on the land hydrology.

Third, the manuscript draws into vague conclusions ignoring the ‘correlation is not causation’ axiom. In this respect, Figure 7C

Figure 7C presents the evolution of the active layer over time (similarly to other graphs presenting temporal evolution of different simulated variables). It does not aim at correlating 2 modeled variables like those of Fig. 9 (of the former manuscript) further mentioned by the reviewer.

and Figure 9A-C are illustrative. A (sometimes not so) strong correlation between E and physiographical features may well reflect a spurious correlation, i.e., when there is a third common factor that correlates to both variables (Pearson, 1897). E.g., in Figure 9B which is incorrect in itself – there is no seasonal thaw in ‘no permafrost’ points

Seasonal freezing and seasonal thaw are the two sides of the same coin. As much as seasonal freezing, seasonal thaw is a straightforward and valid physical value that we derive from our ground temperature results.

– both variables might well be related to an increase in mean air/ground surface temperature, and juxtaposed control in precipitation, so that ground remains frozen longer at 0.7m when air temperature is low and precip is high causing most sensible heat available to be spent on evaporation and ground cooling. A common variable – or a multivariable set – vaguely explains this relation. Or not – but you have the data at hand to disprove my reasoning. Since this physics drives the CryoGrid model, as well as many other models, I think I am not too wide in my perception.

We understand the concern of the reviewer on these questions of correlation and causation and we have significantly modified the manuscript accordingly. We detail here our perception on this question. These correlations compare the variability of key variables for the study and test relatively straightforward and simple hypotheses regarding physics, therefore we think they are worth being part of the manuscript. Since this material (initially presented in section *4.4. Thermo-hydrological couplings)* allows us to discuss connections between key variables (ground temperature, active layer depth, evaporation, runoff), we think it represents relevant discussion content. We therefore moved this section to the Discussion, part 5.1.4. (The interdependence of thermal and hydrological variables). We also largely re-worked it and rephrased the presentation of the graphs with more cautious wording in order to avoid the confusion between correlation and causation:

“5.1.4. The interdependence of thermal and hydrological variables

Our simulation results enable us to explore the interplay between the fluxes of energy and water at the surface and subsurface. In this regard, we tested the correlation of evaporation with the proportion of liquid/total water in the ground for cold and warm permafrost, as well as the correlation between evaporation and the duration of seasonal thaw at a 70 cm depth (Fig. 10A and B). For permafrost areas (cold permafrost and warm permafrost), evaporation shows a strong correlation with the seasonal distribution between liquid and frozen water, similar to previous modeling works for the region (Cuo et al., 2015). As such, this correlation suggests that the intensity of seasonal ground thaw plays a role in enabling higher or lower evaporative fluxes. This is likely due to cold surface temperatures strongly reducing water loss from the surface and because moisture delivery to the surface is inhibited when the ground is frozen. We suggest that this dependence is particularly important in the Paiku Catchment because evaporation is strong (88% of the precipitation input to the surface evaporates on average) and because frozen water is the dominant form of water in the ground in permafrost areas (Fig. 10A, the calculation includes the first 2 meters below the surface).

Similarly, evaporation in no permafrost areas shows a significant correlation with the duration of the seasonal thaw (Fig. 10B). We suggest that this result arises from the fact that frozen ground limits the evaporative fluxes and thus years during which the subsurface seasonal thaw is shorter are associated with reduced evaporative fluxes. We also tested the relationship between the linear trend of active layer deepening and the mean evaporation (over the 40 years of simulation) for warm permafrost areas (Fig. 10C). Thus, this graph does not present annual values and one point corresponds to one of the 92 TopoSUB points classified as warm permafrost (values based on the 40 years). The graph highlights that TopoSUB points showing an AL deepening trend are associated with low evaporation and precipitation. From there, TopoSUB points with stronger evaporation show no deepening trend or even a shrinkage of the AL. This relationship is contradicted by the highest level of evaporation observed for warm permafrost, for which AL deepening is observed again (dark blue points of the graph). These TopoSUB points with the highest levels of evaporation also correspond

to those receiving the largest amount of precipitation. Further discussion on active layer trends is provided in the next section.

Runoff also shows a strong connection with the ground thermal regime (Fig. 10D). At the beginning of the simulation, years with an average 2 m-deep temperature below 0 °C are associated with limited subsurface runoff (< 5 mm per year). Over the years, as the ground warms up and permafrost disappears, subsurface runoff increases and can reach 20 to 45 mm per year. This result is consistent with increased subsurface connectivity expected when permafrost thaws (Gao et al., 2021; Kurylyk et al., 2014) that has been both observed (Chiasson-Poirier et al., 2020; Niu et al., 2016) and modeled (Gao et al., 2018; Huang et al., 2020; Lamontagne-Hallé et al., 2018). We suggest that these substantial changes in subsurface runoff, associated with changes in the ground temperature in Fig. 10D support the hypothesis of a modification in the hydrological pathways as permafrost thaws.

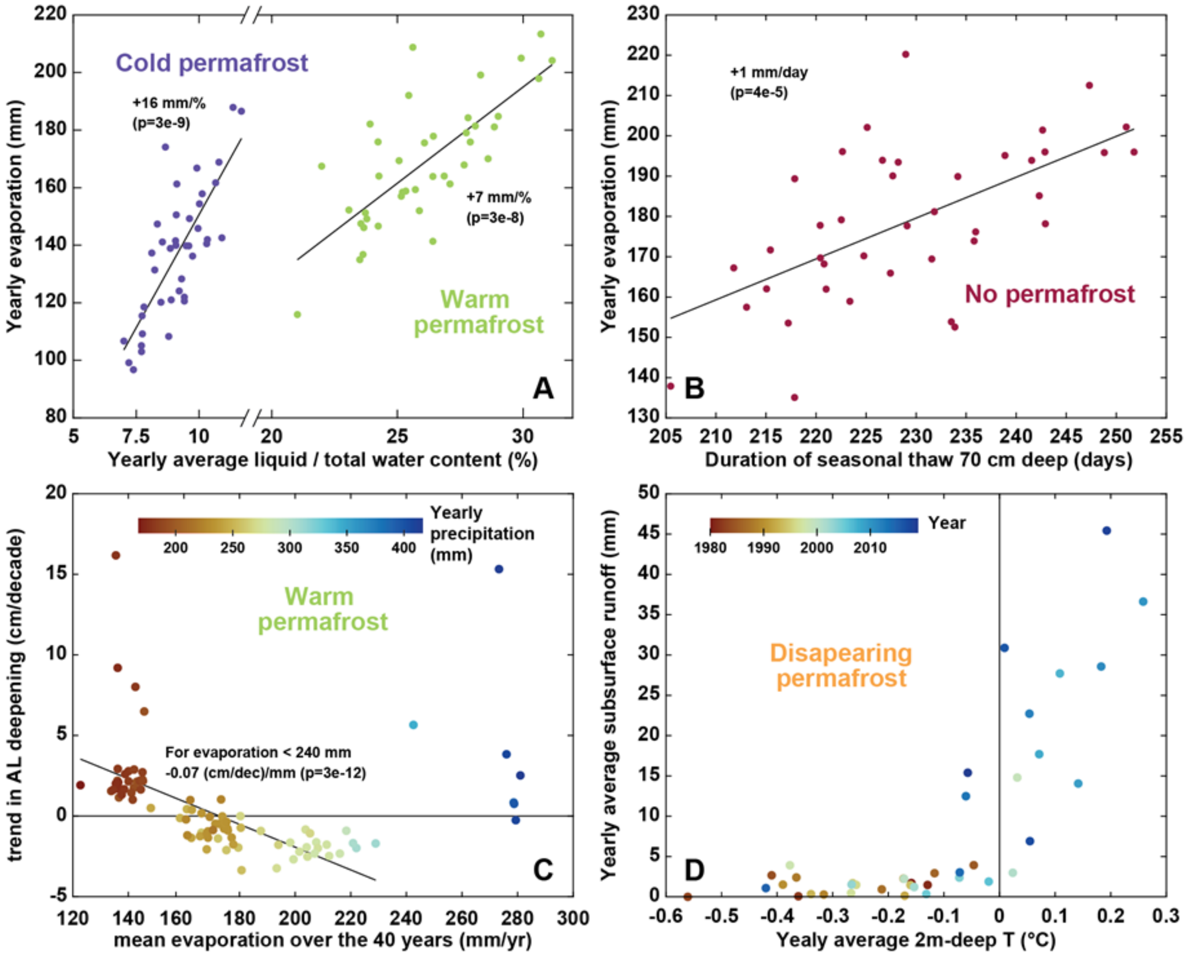

Figure R1.2 (Fig. 10 of the revised manuscript). Thermo-hydrological couplings. A: Annual evaporation vs. annual mean of the liquid / total water ratio over the first 2 meters of ground, averaged for simulations corresponding to cold permafrost and warm permafrost (one dot per year for each permafrost category). B: Annual evaporation vs. duration of seasonal thaw at a 70 cm depth averaged for simulations corresponding to locations without permafrost (one dot per year). C: Active layer deepening trend vs. mean evaporation over the 40-year for each simulation corresponding to warm permafrost (here one dot corresponds to one TopoSUB point). The color of the dots shows the precipitations averaged over the 40 years for each simulation. The linear regression excludes simulations exhibiting annual evaporation higher than 240 mm. D: Annual subsurface runoff vs Annual 2 m-deep temperature averaged for simulations corresponding to locations with disappearing permafrost (one dot per year). The color of the dot indicates the year of the simulation.

Altogether, these results suggest a dependence of key variables quantifying the catchment hydrological balance (evaporation, runoff) to the seasonal characteristics and interannual trends of the ground thermal regime (temperature, liquid vs frozen water content). Similar to previous studies (Ding et al., 2020; Wang and

Gao, 2022), we think these results advocate for the necessity to couple thermal and hydrological modeling to improve our ability to understand and quantify changes in the hydrological balance of high mountain catchments. To our best knowledge, along with Gao et al. (2022), our study represents to date the most complete effort to include the variety of coupled climatological, surface and subsurface processes characterizing the climate, hydrology and ground thermal regime of high-mountain catchments in Tibet at a small scale with a high spatial resolution."

In the same fashion, on Figure 9C, less evaporation means faster active layer deepening exclusively in dry simulations with P < 200 mm. The AL reduction is driven by an increase in BOTH evaporation and precipitation, which presumably means that across simulations (TopoSUB points?) the evaporation is in fact moisture-limited not energy-limited (Haghighi et al. 2018, https://doi.org/10.1002/2017WR021729). This is however a speculative conclusion as I do not have all the data in hand and not intended indeed to fully reproduce this research from eleven contributing authors.

We are not sure to understand what puzzles the reviewer here. But we fully agree to the other look she/he gave at this result in its specific point to L496, where she/he expresses the message we want to convey and concludes that it seems plausible to her/him.

Finally, on several occasions, the authors were particularly imprecise in interpreting the references. See below, comments to L71-72 and L671-672. I was not up to verifying the correctness of all references, but hope that the authors will do so during the revision.

We are sorry for these imprecisions. We are grateful to the reviewer for such an in-depth verification. We went through all the references of the draft to ensure they were used correctly.

Multiple line-by-line comments are also provided:

L39: Bibi et al. 2018 does not refer to Bowen ratio or latent heat fluxes; also, should be (Yang et al. 2014a)

We split the references to avoid misunderstanding (Yang et al., 2014a speaks of Bowen ratios).

L71-72: this is an incorrect citation; Qin et al. 2017 found that evaporation is increasing along with an increase in both precipitation and air temperature (Qin et al. 2017, Figure 5, p. 837). Then, "The annual precipitation first decreased <…> from 1981 to 2002 and then increased <…> from 2002 to 2015. The annual runoff exhibited a trend similar to that of precipitation, but the runoff coefficient displayed a decreasing trend" (Qin et al. 2017, p. 839). In (Wang et al., 2020b), their Figure 5c, d (p. 8 of 13) does not show a runoff decrease; Figure 5c shows an upward trend since the mid-1990s, and Figure 5d shows variations similar to those shown by (Qin et al. 2017). So the claim that runoff is found to decrease is straightforwardly incorrect, and not supported by the references.

We modified the text for:

"***Similar ground warming trends were reported in the regional modeling study from Qin et al. (2017) along with an increasing trend in evaporation and a decrease of the runoff coefficient over time. Plateau-scale surface energy balance modeling from (Wang et al., 2020b) reported that increasing trends in evapotranspiration could be mainly explained by variations in air temperature and net radiation at the surface.***"

Additionally, I find slightly controversial the two claims presented in both the manuscript and the cited literature, that (1) the change in Bowen ratio decreases as latent heat fluxes limit sensible ground warming, in other words, increased evaporation limits ground warming, and (2) ground warming promotes evaporation. This is the reasoning of a kind, "more cheese (warming) = more holes (evaporation), more holes = less cheese, more cheese = less cheese", and I struggle to find a correct line of thinking to get the logic right.

As with the cheese, we believe that both propositions 1 and 2 are correct and that the sophism arises when trying to assemble them under different working hypotheses, like different changes of parameters.

The effect "increased evaporation limits ground warming" supposes that under a similar climate signal, grounds that are not or less limited by water availability (compared to those who are) will enable a greater evaporative flux during summer, an energy loss that will counterbalance the seasonal energy intake.

The effect "ground warming promotes evaporation" supposes that, for a given level of total water availability, a ground exposed to a warmer climate (when compared to a colder one) will enable more evaporative fluxes than the one exposed to the colder climate because more liquid water is available.

From there we believe that both effects are relevant to interpret our results, provided that they are used within the correct frame. In the case of the Paiku catchment, these effects only marginally affect the main trends of ground warming and evaporation increase imposed by the surface energy balance in response to the climate. Indeed, the energy input from the climate signal was strong enough to warm the ground, despite an increasing evaporation trend. Effect 1 only affects the magnitude of this warming between wetter and dryer locations experiencing similar climatic conditions, as shown on figures 7C and 9C. Similarly, at a given location, we think that our results support the idea that the increasing proportion of liquid water (compared to ice) over time contributes to promote evaporation, along with other changes of the surface energy budget.

L82-83: for consistency and clarity, please express all trend rates across the manuscript in units per decade, not per century.

We applied this modification throughout the text and figures

L91-92: see above; Qin et al. 2017 reason on decrease in runoff coefficient, not runoff itself.

We replaced “*runoff*” by “*runoff coefficient*”.

L93: here, and elsewhere in the manuscript, replace ‘yearly’ with ‘annual’; the former is most used as an adverb, while the latter, as an adjective.

Modification applied in the text and figures

L122; here, and throughout the manuscript, better use (Appendix B, Figure B1) to refer to Appendix data, otherwise your current reference style causes confusion, i.e., later in the manuscript, L215, and particularly L363.

Done

L128-129: better provide the range than a single value, also 200 mm is significantly lower than your Figure 1C, Figure 3C, and multiple figures throughout the manuscript, i.e., Figure 9C.

We replaced “*(~ 200 mm year*$^{-1}$*)*” by “*(200-300 mm year*$^{-1}$*)*”.

L178-179: this is a proper line to place the water balance equation and present its terms – this will structure the presentation in the following sub-section.

This part now reads:

“As such, the present study requires quantification of all these terms of the hydrological balance. ***The hydrological balance of the lake is given by the following equation:***

$$\Delta z_{Lake} = Precipitation_{Lake} + Runoff_{Land} + Runoff_{Glacier} - Evaporation_{Lake}“$$

L185: ‘in Section 3.2.5’

Done

L203: SRTM30 is known to be highly imprecise in mountainous regions to the degree it is red-flagged to be used ‘as is’ e.g., in the Himalayas (Mukul et al. 2017, https://www.nature.com/articles/srep41672). Please comment on the potential uncertainties of your approach, or, otherwise, how was SRTM30 data treated to limit such uncertainty.

The topographic parameters (elevation, slope, aspect, skyview factor) that we derive from the SRTM DEMs are clustered by the TopoSUB algorithm. As a consequence, they are smoothened and averaged by a convergence on a cluster mean value, limiting the impact of specific errors at specific DEM pixels . Additionally, the catchment presents a very strong spatial variability regarding all of these parameters so that we do not need a higher level of precision to capture these gradients. In this regard, the method manages to reconstruct the catchment topography in a reliable way (Fig. C1). Finally, this approach has, for now, no equivalent regarding its ability to capture the complexity of mountain terrains with limited computational costs for applications requiring surface energy balance calculation. Thus, even with the 10m additional error above that published for SRTM DEMs in undulating terrains (reported by Mukul et al., 2017), we believe that our methodology is still a substantial improvement compared to others that would be based on the delimitation of sub regions of the catchment or any type of response units that would be much bigger than a 100 x 100 m pixel and thus would tackle the topography in a simplified and less accurate way.

L220-224: am I correct to understand that: the observed data from one-year long record, October 2019 to September 2020, was monthly-averaged compared to a 40-year monthly-averaged ERA5 data (for a pixel/TopoSUB point where the AWS is located?), then correction factors were obtained bringing ERA5 monthly data to the AWS data, and they were applied then to other TopoSUB points? So to say, longer records were corrected by a shorter record, and regional data were corrected by punctual correction factors? If so, a largely uninspiring Section 5.1, notably sub-sections 5.1.1 and 5.1.2, can be animated with discussions on the applicability of this approach and potential uncertainties implied.

The reviewer understood correctly. There is no other meteorological measurements available to perform the necessary corrections. We extended the initial paragraph on this topic in section 5.1.:

"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 D1), these corrections do represent an important first-order improvement****."*

L226, Figure 3: if providing a p-value for a trend, explain how it was obtained, in the separate Statistics paragraph in the Methods section. This applies to this figure and to multiple occasions across the manuscript. Were the trend tests performed, and if yes, which exactly. Mann-Kendall test would roughly give p-value of 5e-4, though consistent Sen's slope.

We added the following information to section 3.2.2 (right before the first appearance of a p-value Fig.3 presenting the forcing data):

"In this figure and across the rest of the study, we use p-values to evaluate the significance of linear trends in the temporal evolution of certain variables (temperature, precipitation, evaporation…). This p-value tests the null hypothesis which supposes that the value of the slope is equal to zero. The hypothesis is tested using the Student's t-test, by comparing the distance between the estimated slope and 0, relative to the standard error of the slope. We did not report trends when this p-value (probability of a null slope) was higher than 5 10^{-3}."

L231, Section 3.2.3: evapo(transpi)ration from the land surface is not presented in this section. However, this variable plays an important part in your reasoning throughout the manuscript! Was it E or ET, is your basin al bare soil, or vegetation is present?

This comment is similar to the comment of reviewer 2. In response to both we have modified the end of section 3.2.3 (The CryoGrid community model) which now reads as:

"***At the surface, the model uses a surface energy balance module to calculate the ground surface temperature and water content. The turbulent fluxes of sensible and latent heat are calculated using a Monin–Obukhov approach (Monin and Obukhov, 1954). Evaporation is derived from the latent heat fluxes using the latent heat of evaporation and is adjusted to the available water in the soil. It occurs in the first grid cell only, but water can be drawn upwards due to matric potential differences. Because vegetation is very scarce in the catchment, we do not expect transpiration to have a strong imprint on evapotranspiration and our calculations do not unravel evaporation from transpiration.***"

L291-292: for the TopoSUB, the lake surface is a homogenous surface hence represented by four TopoSUB points, one for each ERA5 pixel? Explanations are needed, otherwise unclear how lake climate forcing was assembled.

To be precise, we got 7 TopoSUB points for the lake, 1 corresponding to one ERA5 pixel and 3 pairs for the 3 other ERA5 pixels. For each pair of TopoSUB points, the two datasets were almost identical and produced highly similar simulation results. We then averaged the obtained results based on their spatial footprint. We believe that, methodology wise, what is important for the reader is the averaging of results associated to 4 different ERA5 pixels and not the details on the pairs of nearly identical TopoSUB points. So we added the following explanation to the manuscript at the end of section 3.2.5 (Lake modeling):

"*Similar to the land simulations, the lake simulations were forced by the downscaled ERA5 data* ***(with the TopoSUB and TopoSCALE methodology)****, with the corrections derived from the AWS data (Sect. 3.2.2). The simulations were initiated with a 20-year spin-up of the 1980-1984 climate.* ***The simulation results corresponding to the four ERA5 tiles covering the lake were then averaged using the respective spatial footprint of each tile on the lake.***"

L294, Section 3.2.6: data from L342 belongs here.

These data are presented in section 3.2.6. To make clear that their mention line 342 is a reminder, we modified the following sentence:

*"**As presented in section 3.2.6,** glacier mass balance values are considered constant for the 1980-2000 period and the 2000-2019 period and are respectively equal to -4.6 ± 2.5 10^7 and -6.4 ± 2.8 10^7 m^3 per year."*

L309, Section 4.1: see general comments. In this section, besides model validation, the summary of the hydrological results is partially given, but incompletely. The water balance equation approach would help structuring the narration, and interpreting the results. In general, all members of the lake water balance equation are written first in absolute values, i.e., volumetric units, km3, then converted to layer units, mm, scaled either to lake surface or, less often, to catchment area. See, e.g., (Szesztay, 1974; https://doi.org/10.1080/02626667409493872) for reference water balance equation for an endorheic basin. From this approach, deep groundwater component can be roughly estimated as well. Besides, this approach allows the derivation of lake level time series which can be directly comparable to the observed data. Isn't this, according to the title, an important aspect of your study?

Following our response to the main comment and the comment on line 178-179, the equation for the lake budget is now given in section 3.2.1. Additionally, each component of the lake budget and the reproduced lake variations are presented in the new result section 4.4. Following our response to the previous comment on the topic, our approach does not give us access to a quantification of the deep groundwater component.

L316, Figure 5: on Figure 5C, does the scale refer to lake level, or lake level change? If this is change, is it change to previous year? If it is lake level, explain the reference level – which level is taken as zero. Also, order of figures is different from other figures, Figure C is top right, while on other figures, it is in the bottom left. This is acceptable, but potentially confusing.

We modified the Y axis that now states lake level relative to August 2019, we also modified the order of the letter.

L336-337: This is unclear, rephrase and explain. Otherwise, it is evident that in lake water balance, the catchment input is important.

We removed the sentence.

L341-342: see above.

We modified the text according to the previous comment of the reviewer (comment on line 294).

L342-343: is it correct that only the annual precipitation over the glacier area was considered? Am I right to understand that all precipitation over glacier area was flushed toward the lake at all altitudes, so to say there was no glacier feeding during this time above the ELA?

To be precise, when the total precipitation equals the glacier yield, the glacier loses exactly what it gained and the mass balance over the year is 0. In this case there is no feeding only if there is also no ablation which is unlikely. When there is a negative mass balance, the glacier yield can be counted as the precipitation plus the glacier mass loss, as we presented it in the methods.

L348-349: simulated lake level curve would be more informative on this matter.

We now present the simulated lake level in the new result section 4.4.

L363: why 8m? I am curious since the model had a spin-up period of 60 years to reach the steady-state conditions at the first 2m only (L268-269). Does this mean that below 2m the model was not in the steady state after the spin-up period and hence at least some change at 8m can be attributed to non-steady-state evolution?

The motivation was to show results of temperature changes where the year to year variability is limited and where most of the signal comes from long term trends. Yet following the next remark from the reviewer we no longer display these maps.

Relatively to the steady state at depth, we realize now that our wording was misleading. The effectiveness of thermal initialization was checked for all the runs and ranged from 9 to more than 80 meters of depth, depending on the magnitude of phase changes during the initialization and the difference between the values at t_0 and the steady state temperature values. We rephrased those lines which now read:

*"To do so, we start our simulations with a 60-year spin-up of these first 5 years (12 repetitions), which is sufficient to establish a stable temperature profile **over the first 9 to 80 meters depending on the simulations, extending beyond** the hydrologically active part of the ground (the first 2 meters)."*

L372, Figure 6C, D: as change is not immediately deducible from this pair of images, would not it be more informative to provide one figure with change in DJF temperature between the two time periods?

We agree with the reviewer and think that this map was not the most relevant way to present our results. We have now reworked the figures presenting the thermal results. We removed these temperature maps and present instead the maps of the ground thermal regimes along with the temperature trends for each thermal type of ground (initially presented in the next figure of temperature related plots). Since this freed a space in the other temperature-related plot figure, we included the depth and duration of seasonal freezing (these 2 operations allowed to remove to appendices):

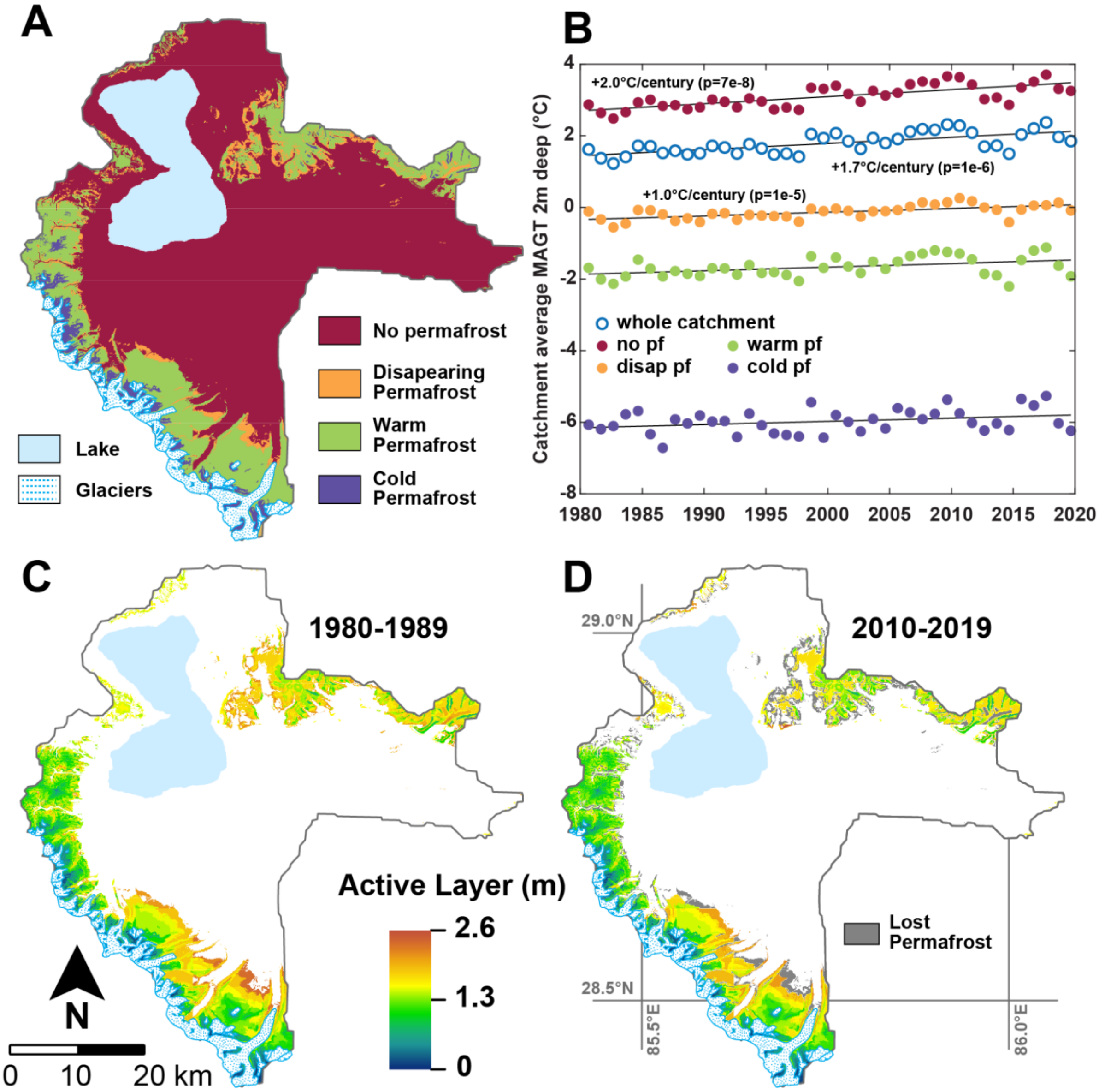

Figure R1.3 (Fig. 6 of the revised manuscript). A: Different cryological states of the ground throughout the catchment for the 1980-2019 period (see Tab. 1). B: Annual 2 m deep ground temperature averaged for the whole catchment and for the different cryological states of the ground. C: Average active layer depth over the 1980-1989 period. D: Average active layer depth over the 2010-2019 period. Only locations presenting permafrost at the end of the simulation are assigned a color on the map on C and D. Locations where permafrost has disappeared are shown in gray on D.

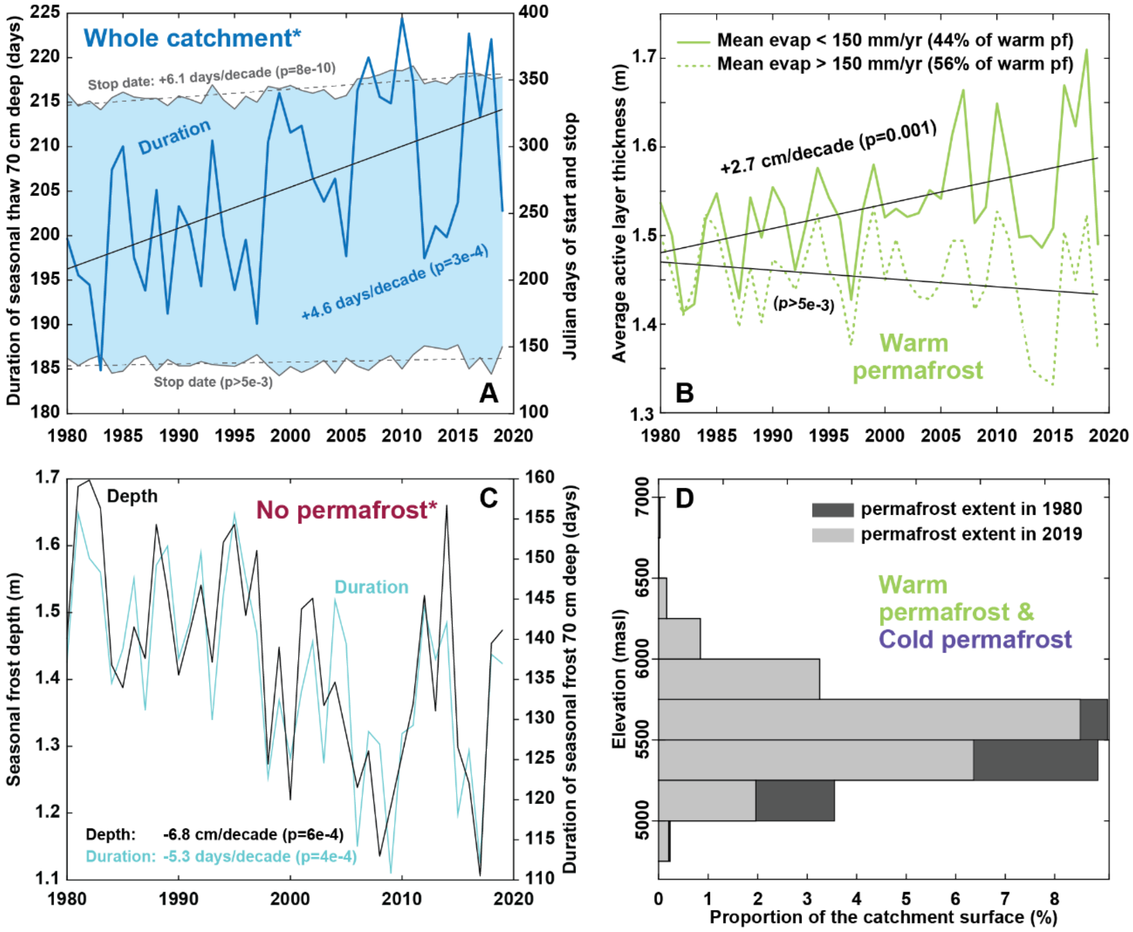

Figure R1.4 (Fig. 7 of the revised manuscript). A: Duration of seasonal thaw 70 cm deep averaged over the catchment. The asterisk indicates that the presented curves average 89% of the surface of the catchment (Sect. 4.2). The gray curves and the light blue area are associated with the right axis and indicate the average start and stop day of the seasonal thaw in the Julian calendar. Values higher than 365 indicate that freezing conditions came back after the 31st of December. B: Active Layer Thickness (ALT) evolution for warm permafrost. The solid line shows the ALT for simulations experiencing an annual evaporation lower than 150 mm when averaged over the 40 years. The dashed line shows the ALT for simulations with annual evaporation higher than 150 mm. C: Temporal trends for seasonally frozen ground where there is no permafrost. The asterisk indicates that simulations were excluded if one of the simulated years did not present freezing conditions 70 cm deep (persistence of thawed conditions from one year to another). The presented curves thus average 88% of the total permafrost-free areas of the catchment. D: Altitudinal distribution of permafrost in 1980 and 2019. This distribution includes both cold and warm permafrost.

L382-383: what is a ‘distinct active layer season’? Same in L385. The active layer is relatively thin in cold permafrost, but the winter-summer temporal pattern holds for cold permafrost as well.

Locations corresponding to cold permafrost are located very high up and show important amplitude of the daily temperature cycles. As a consequence, the ground can freeze from the surface to the top of the permafrost (which is shallow, smaller than a meter) overnight during the warm season. This does not happen for the warm permafrost. We now phrase this better:

“For cold permafrost, frozen conditions dominate the first meters of the ground most of the year and surficial thawing during summer ***can be interrupted by ground freezing from the surface to the top of the permafrost at night****.”*

L406: Here, and throughout the manuscript, if the trend is not significant, avoid presenting trend rates as they do not convey reliable information and can be misleading. See, e.g., Figure 7C.

We removed all y=ax+b and p-values from trends for which the p value was superior to $5x10^{-3}$. We also now mention this threshold in the methods.

L420: if possible, avoid starting your paragraphs with presenting figures. Figures accompany the manuscript text and serve as references confirming your textual statements. When the figure is presented 'as is', decoupled from the main text flow, it loses its reference value. But is a scientific paper, there is no value for a figure other than a reference. Try to better integrate your figures in the text flow. Also, for Figure 7C, add precipitation time series for both high and low evaporation regions (TopoSUB points?).

We complied with the style preferences of the reviewer and reworked the text accordingly. We prefer not to apply the suggestion of the reviewer for figure 7C because we believe that the role of precipitation on evaporation and active layer deepening is covered in the more detailed figure 10C (formerly figure 9C) where the precipitation is represented with the color scale. This allows to not over complicate figure 7C.

L432-433: in other words, locations with average seasonal freezing depth was less than 0.7m, were excluded from calculations? Is it correct?

Rather than the average value, it is if the minimum value of seasonal freezing depth did not reach 0.7m that we excluded the points, we rephrased to:

"These values average 88% of the no permafrost areas since locations showing persistent thawed conditions ***at this depth*** *from one year to another were excluded* ***(i.e. minimal seasonal freezing depth over the 40 years lower than 70 cm).****"*

L436-437: in evaporation calculations (as well as other hydrological variables though), how were the layer units (mm) obtained? Are they direct model output for a TopoSUB point? Were they averaged over the TopoSUB point representative area?

Yes, for a given TopoSUB point, the model produces hydrological values in m^3 using the area of a TopoSUB pixel on the catchment map. We added a paragraph explaining this at the end of section 3.2.4. (model setup and validation):

"***The following method is used to produce area-averaged evaporation and runoff (in mm water equivalent) in a zone of interest. For a given TopoSUB point in this zone, the model produces hydrological values in m3 using the area of a TopoSUB pixel on the catchment map. Then these values are multiplied by the number of pixels in the zone corresponding to this TopoSUB point in particular, and this for all the relevant TopoSUB points covering the zone (e.g. evaporation in warm permafrost). Then the area of interest is calculated by counting the number of pixels in the zone of interest and multiplying this number by the area of a pixel. Then the total volume is divided by the total surface for the zone of interest to obtain the final value in mm***"

L443: Why not runoff coefficient?

We found different definitions of the runoff coefficient online but the most common definition seems to be that the runoff coefficient is calculated as **runoff / (rain + snow)**. For our study we calculate **runoff / (rain + snow – snow sublimation)** which is slightly different. We think it is a more relevant value because it is equivalent to **runoff / (runoff + evaporation)** so it focuses on the precipitation that makes it to the ground surface and splits it between the proportion that can produce runoff towards the lake and the proportion that goes back to the atmosphere through evaporation.

L460, Figure 8C: besides the steady lake level, it could be instructive to present the ratio values explaining the lake level variations, notably its observed gradual decrease since the 1980s. Also, Figure 8D: with +48mm per century trend, we can assume no runoff around 1950s, even earlier for the subsurface runoff.

We understand the point of the reviewer but given the data we have and our framework we are limited in our ability to produce the expected graph. The goal of our graph is to present hydrological outputs from our simulations with a reference point regarding the lake. We suggested this reference level for a steady state lake because we could design it as a value independent of our glacier runoff estimate. To do so, as explained in section 4.3, we assumed that the runoff and evaporation values are correct and that the actual glacier runoff is the runoff we need to add on top of the land runoff to reach the runoff required to close the lake budget (reproducing the lake level observations, as on validation figure 5D). If understood correctly, to add the graph suggested by the reviewer, we would need to use our glacier runoff estimates which are based on only 2 averaged values of glacier mass balance for the whole period and for which uncertainty bars neighbors the size of the values of land runoff. For this reason, we think this additional data on the graph would not improve the presentation of our hydrological results, which is the initial goal of this figure.

We agree that the trends we simulate take high absolute values and most likely express a non-linearity in the evolution of these variables. We added this idea to the first paragraph of Section 4.3. (Hydrological results):

"***These linear trends we report are high compared to the absolute values of the variables and their extrapolation backward in time would lead to null values in the recent past which is unrealistic. This suggests a non-linear evolution of these variables over the XXth century.***"

L466: isn't 'liquid/total' more correct, as shown on the Figure 8D?

We applied the correction.

L483: does this mean, that out of 368 TopoSUB points, 92 were classified as 'warm permafrost'? In other words, does 'simulations' refer to 'TopoSUB points' here?

Yes, we replaced simulation by TopoSUB point in the text (which now appears in the discussion, part 5.1.4.)

L484-485: also, AL deepening is associated with low precipitation!

We added "and low precipitation" in the text (same new location as previous point).

L474-476: 'correlation is not causation' holds here, and while Figure 9A shows correlation, it does not necessarily reasonable. What if this is a spurious correlation with precipitation as a driving variable? This must be tested otherwise can be highly misleading (see general comments)

As stated earlier, we understand the concern of the reviewer on this point. Therefore, we moved this outcome of our study in the discussion part and we now use careful wording such as "correlations suggest…" see the response to the main point above.

L498, Figure 9B: under 'no permafrost' condition, there is no seasonal thaw, but rather seasonal freezing. The manuscript contains the data required to produce the correct figure (Appendix E, Figure E, right), but whether such figure is useful, I am not convinced.

See our previous comment on the quantification of seasonal thaw based on ground temperatures.

L498, Figure 9C: see comment on L484-485. Dry locations = less evaporative loss (moisture-limited E) = less latent heat fluxes = higher sensible heating = deeper AL. Sounds plausible to me.

This is indeed the message we want to convey.

Also, combining Figures 9A and 9C, is it so that for the points (years) in Figure 9A, there must have existed points with average P over 400mm and E over 280mm, counterbalanced by points with much lower E values, so that annual E would not exceed 220mm? What are these points?

There is an important North-South precipitation gradient in the catchment. The first points the reviewer mentions are in the north where precipitation can reach 400 mm and evaporation can go beyond 250 mm, whereas in the south some points can receive less than 200 mm per year. Since figure 9A averages the point to point variability to calculate the average values for warm permafrost, this variability is toned down. Like this, the maximal annual evaporation over the 40 years (when averaging all the warm permafrost locations) is lower than the maximum evaporation (averaged over the 40 years) for the topoSUB points where a lot of evaporation is happening.

L510: Sections 5.1.1 and 5.1.2 are unimpressive at best. Yes, we know field data are scarce, but would it be catchier to discuss uncertainties arising from data assimilation techniques, not data absence. Some related questions are listed in the comments above.

Section 5.1.1 has been streamlined and largely reworked. It now includes discussion on the relation between the data and the modeling as suggested by the reviewer. Section 5.1.2 has also been shortened following the later comment of the reviewer on line 539. The beginning of Section 5.1.1. now reads as follow:

"***Our approach relies on a variety of data regarding their scientific focus (glaciers, ground, lake, atmosphere), their type (in situ observations, remotely sensed data, reanalysis data), their characteristics (point wise data, distributed data, constant or with various time resolution) and the way they interact with our models (model parameters, forcing data, validation data, result data in case of the glacier runoff). Such a diversity arises from our goal to quantify both the ground thermo-hydrological regime and the different terms of the lake budget. This variety also makes it challenging to consistently merge these data into a unique framework. For example, our quantification of the glacier mass change reconstruction is made of two constant values for the study period (1975-2000 and 2000-2020), which limits the relevance of the comparison between the observed lake level variations and the simulated ones.***

Yet, the lake level variations… [initial text]"

L529: Finally, there is no lake level variation curve generated as an outcome from this study, so no, the robustness was not evaluated against this directly observed variable.

The lake level variations are now presented in the result part of the revised manuscript (Sec. 4.4).

L532: in fact, not; red curve is not lake level fluctuations, but runoff required to close the observed annual water balance.

The initial sentence in the submitted manuscript is:

"We combine lake level observations with our precipitation forcing data and lake evaporation quantifications in a simple mass conservation calculation to derive the land runoff to the lake required to reproduce the level variations (red curve on Fig. 5)"

Hence the "red curve" does not refer to "the level variations" but rather to "the land runoff to the lake required to reproduce the level variations", which is correct.

L539: Water routing has minor importance on annual timescale (you admit it in L546-548). This paragraph can be omitted from the manuscript. In L544-545, the 95% argument is reiterated though it was just evoked in L532-533 to support the correctness of the magnitude.

We deleted the paragraph.

L578-579: Figure 9B is unrelated to frozen water content, maybe Figure 9A? Figure 8D looks contradictory in this scope; although it refers to the whole catchment dominated by non-permafrost areas.

Indeed, the mention should have gone to Fig 9A, which is now Fig. 10A in the revised manuscript. We corrected this mistake.

L671-672: this effect was not modeled by Wang et al. 2018 but it is represented in several global climate models in this way under RCP4.5 (see, e.g., their Table 3, p. 1159).

From our understanding of this article, we believe this is incorrect. Wang et al. (2018) used outputs from GCM models to force their GBEHM model which calculates both phase changes at the surface and subsurface and runoff (among other physical values, see their section 3.2 that starts on page 1156).

L721: Sections 5.4 and 6 are repetitive, they can be merged into one, otherwise, provide mode discussion concerning lake level changes in the respective section.

Following the suggestion from the reviewer, we merged the two parts into a conclusion.

L724-726: modeled data can not lead to observed lake level change. Also, how modeled data drives modeled lake level change, is not presented in the manuscript (a major flaw).

We rephrased for:

"The sum of the direct precipitation in the lake, the land runoff and the glacier runoff are not enough to compensate for the lake evaporation over the study period, hence driving the observed lake level decrease."

Modeled lake level changes are now presented and discussed in the revised version of the manuscript.

L730: 'affecting' stands for 'increasing' here? Also, L733-734 is not about change in permafrost but the presence of permafrost, which is different.

Yes, we changed "affecting" for "increasing".

We also agree with the second sub-comment from the reviewer but we do not see how it can be seen as problematic, as we are simply listing results.

L727-728 and L733-734 need to be consistent and better supported by results/discussion. E.g., the sequence "catchment loses permafrost (Figure 7D) = less ET (L733) = increase in P (Figure 3) = increase in runoff & runoff ratio (Figure 8B)" might be incorrect straight away because E is also increasing catchment-wise. But where ET is increasing most? There is no answer in Figure 8A nor in the manuscript text. Does the increase in ET coincide with TopoSUB points where permafrost was lost? The answer is relatively easy to answer.

We are not sure to fully understand the reviewers point here. Regarding L727-728. We rephrased the sentence in a more cautious and complete fashion:

"Long-term hydrological trends in the catchment are led by trends in climate; ***and precipitation increase, jointly with glacier melt, provides enough water to drive a concomitant increase of runoff and evaporation.****"*

Regarding the second point, L733-734 is merely a summary of the sensitivity test presented in Section 5.3. (Evaporation vs runoff and sensitivity to climate conditions). It is written with careful wording and neither adds new discussion elements nor new implications on top of the simple results of this test (Fig. 11).

L755: not where it is limited, but just were it is 'relatively' low compared to other TopoSUB points, for whatever reason; Figure 9C suggests that the main reason is low precipitation amount. Both figures are for warm permafrost.

We changed "limited" for "relatively low"

L766-767: see above.

Here again, we do not see why describing a result related to the presence or absence of permafrost poses a problem.

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