Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
https://doi.org/10.5194/hess-2022-241-RC1, 2022

Comment on hess-2022-241

Anonymous Referee #1

Referee comment on "Recent ground thermo-hydrological changes in a southern Tibetan endorheic catchment and implications for lake level changes" by Léo C. P. Martin et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-241-RC1, 2022

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 – 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. 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'. 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. 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.

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

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.

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)

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.

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.

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

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

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.

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.

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.

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.

L185: 'in Section 3.2.5'

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.

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.

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.

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?

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.

L294, Section 3.2.6: data from L342 belongs here.

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?

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.

L336-337: This is unclear, rephrase and explain. Otherwise, it is evident that in lake water

balance, the catchment input is important.

L341-342: see above.

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?

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

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?

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?

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.

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.

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?).

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

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?

L443: Why not runoff coefficient?

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.

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

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?

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

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)

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.

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. 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?

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.

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.

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

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.

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.

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).

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.

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).

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

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.

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.

L766-767: see above.