Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-320-RC3, 2020

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "Thermal regime, energy budget and lake evaporation at Paiku Co, a deep alpine lake in the central Himalayas" *by* Yanbin Lei et al.

Anonymous Referee #3

Received and published: 18 August 2020

General comments: This study reports lake evaporation in the Tibetan Plateau and explains its seasonal variation through energy storage change within lake water. Since the Tibetan Plateau has been one of the least studied areas, lake evaporation study in this region is welcomed and worth publication in HESS. However, I find there are substantial problems in this study. That is the accuracy of the evaporation in their study. I pointed out this issue in the review at the time of their previous submission. Unfortunately, they failed to solve this problem and the manuscript was rejected for publication. The authors have added new data at the lake center to compare their measurements on the shoreline. This is good. On the other hand, their treatment of the comparison is not enough to convince readers that their evaporation estimates are

Printer-friendly version

Discussion paper

accurate and reliable. Details are listed below.

Major comments. The authors gave error estimates of evaporation in Section 4.2. They selected (1) net radiation and (2) water temperature differences between the northern and southern basins as the error sources. I think they should add other relevant error sources. They include, first, the use of air temperature and humidity on the shoreline instead of those above lake water. They compare the measurements on the shore and on the lake in Fig. 11 and conclude they are "very similar. . .. data from the shoreline. . . can be used to represent the general condition of the whole lake. . .". This is new information and should be used to evaluate error from using onshore measurements. What they should do is to determine the RMS error of temperature and humidity measurements on the shore and used them to estimate errors of Bowen ration and sensible and latent heat fluxes. This can be added to the final error evaluation. The second error source they should consider is the use of water temperature instead of surface temperature. They claim that "the daily average between them is very similar. . .". But no supporting evidence is shown. In fact, previous studies do indicate a difference between the two even for mean values for day or longer. The authors should accept this and add this as one of the error sources for the evaporation estimates. The third error source is the error in the energy storage estimation. To estimate energy storage, spatial mean water temperature profile, spatial mean water level, water level-volume relation, water level-surface area relation are needed. Since they are all based on some kind of measurement, there are always errors (measurement errors as well as sampling errors). They should be considered.

[Minor comments] Introduction. - The originality of the study: It is not clear what the original contribution of this study is. Authors claim that previous studies do not provide evaporation throughout a year. But in their study also, evaporation was not determined during the winter period. So it is not quite new. Please make it clear what is missing in previous studies and why their studies are needed based on a comprehensive review of previous studies. Also, these points should be reflected in discussion and conclu-

sions. - Importance of TP lakes. The authors explain the abundance of lakes in TP. Then authors should add relevance of these lakes for TP (or even for larger areas). - Eddy correlation method. The authors mention that it is not suitable for long-term measurements. I believe this statement was correct perhaps 20 years ago. But it is easy to see the results of long-term measurements based on the eddy correlation method in the literature as well as datasets on the flux net sites. - Direct measurements of lake evaporation: I do not think the Bowen ratio method is in the category of direct measurements. It measures energy balance and evaporation is obtained only indirectly as one of the residuals of the energy balance equation. It relies on the similarity between temperature and humidity profiles.

- L96-97. "...therefore the meteorological condition over the lake surface can be recorded". This cannot be true without evidence. I made the same argument in previous reviews so please read it again. What I would suggest is to acknowledge that it is not the location where measurements should be made, but the measurements were used as a proxy of the above-lake measurements, and validity of this proxy will be discussed in section...(see major comment)

- L106 "weekly averaged radiation..."; What are the possible errors to apply the Bowen ratio method with weekly averaged data? The Bowen ratio equation (4) was derived from two profile equations of H and LE. Profiles equations derived by applying the similarity theory are valid for the steady-state condition under certain stability. So we generally apply them for 30-min to hourly mean values. For practical purposes, we apply them for daily data assuming neutral stability but strictly speaking, this is not valid since profile equations are not linear and therefore simple time averaging does not yield valid equations for the given averaging period.

- L114-115. "the influence of river discharge...can be neglected" You cannot say this without supporting evidence. The authors should give and compare lake storage and river discharge.

- L114-115. “. . .therefore. . .we do not consider . . .G..” There is no mention of the reason why G can be neglected.

- L153-155, “. . .reduction in wind speed (data not shown)”; As I recall from the reply of authors to the reviewers’ comments in the previous version, authors do not have wind speed data. Then the statement of “data not shown” is misleading. When we see this statement, we tend to believe that there were data and authors checked them to validate what is written in the manuscript even though they are not shown in the manuscript with a figure or a table. If you do not have data, then you should not mention wind influence as if it was based on data. There are similar statements on wind speed here and there in the manuscript. Authors need to remove them or change expressions. Alternatively, authors could rely on wind speed from reanalysis data. However, the reliability of any reanalysis data set should be established first (perhaps by referring to previous studies) for the study area before they can use the reanalysis data.

- L179 “..but also the bottom water”; I do not understand what authors want to claim.

- L200-201; "....Lower temperature gradient caused stronger water convection....."; I do not understand the logic in this part. I assume water convection is stronger when the vertical gradient is larger.

- L204-205 "(Fig.1)"; Fig. 1 shows the locations of water level loggers but authors are talking about water temperature. In Fig.1, there are also the locations of water temperature measurements. This is confusing.

- L208-209 "....large errors can result if only water temperature data collected at the shoreline are used to calculate lake heat storage and energy budget."; Similarly, errors can result if only water temperature data collected at the center of the lake are used. The authors should acknowledge this possibility to make analysis accordingly (see major comment).

- L223 " Indian summer monsoon precipitation"; there is no mention of heat advection due to precipitation in the application of the Bowen ratio method. All energy sources that are used for the turbulent neat fluxes should be considered and mentioned.

- L257-259 " Negative value...., indicating the lake water absorbed energy from the overlying atmosphere. Positive value...., indicating the lake water released energy to the overlying atmosphere." These statements are not correct. The sign of the Bowen ratio simply indicates whether the fluxes are in the same direction (positive), or different direction (negative).

- L274-276 " Lake evaporation between middle January and April is not determined... "; Why not? The authors do give latent heat flux for this period. If it is not certain whether the lake surface is covered with solid water or liquid water, then authors could give two values of evaporation. One in the case of the ice surface, and another one in the case of liquid water surface. The true evaporation is somewhere in between. This can be used together with the evaporation estimate obtained by assuming it is the same as water level change in L290-291.

- L288 ".....lake ice can effectively prohibit evaporation."; Is this true? How about sublimation? Is the latent heat flux on ice-covered lake zero? The authors could add references to support their statement.

- L290-291 "Assuming lake evaporation between January and April is equal to lake level decrease ..."; the Authors should provide an error estimate of evaporation based on this assumption. Errors due to lake level measurements, mean lake water level estimation, water level-volume relation, water level-lake surface area relation, etc.

- L293 "20.4%"; the Authors should explain how this ratio was derived.

- L299 "We set up a platform in the southern centre of Paiku Co"; The location should also be shown in Fig.1

- L302 "....fluctuated very similarly between..."; This is a subjective statement. In fact, I

do not quite agree that they are VERY similar. A better presentation would be the determination of the difference between the two measurements and its error propagation into Bowen ratio and flux estimates (see major comment).

- L303 "...can be used to represent the general condition of the whole lake..."; This statement is based on a superficial analysis. It should be based on the error propagation analysis mentioned above (see also major comment).

- L318-319 "Although there is some spatial difference, the similar seasonal patterns of energy budget and lake evaporation at different sites indicate that our results are reliable."; Just like L208-209, authors should acknowledge the difference and estimate the magnitude of the error due to spatial difference, rather than ignoring the difference by simply saying "reliable". In fact, authors do give error estimates in L329-344. Thus the statement of "reliable" is not quite consistent with the error estimates.

- L320, Section 4.2. Here authors should give all possible error sources and their likely magnitude, and use them to give a total error in their evaporation estimates. They should include, among others, the error due to the use of temperature and humidity measurements at the shoreline. Also, when they talk about annual evaporation, there are possible errors due to the assumption in L290-291 (see major comment).

- L326 "very similar seasonal fluctuations (R2=0.55....with standard deviation of 23.9 WÂům-2.)"; With Rˆ2=0.55, I do not think it is VERY similar. Why the standard deviation? ãĂĂRMS error is a more appropriate indicator of the similarity.

- L326-327 " Assuming approximately 70% of the net radiation was consumed by lake evaporation (Lazhu et al., 2016)..... ∼74.5 mm per year "; This percentage is from a different lake. Why not use estimates for Paiku Co. given in Table 2? The authors should explain how 74.5 mm/year was derived.

- L345- " Uncertainty of lake evaporation in this study was also validated by comparing lake level changes"; Just like the case in L290-291, authors should provide error

estimates for the evaporation estimates based on lake level measurements. Possible advection due to precipitation should be addressed as commented above for L223.

- L352 "As shown in Table 3, runoff at the three large rivers can contribute to lake level increase by 0.7∼1.6"; Runoff values in Table 3 are for a short period. Can you use them to estimate monthly runoff?

- L355 "To further explore the impact of lake heat storage on the seasonal pattern of lake evaporation...."; Authors should summarize at the end of section 4.3, what kind of new findings were obtained on the impact of heat storage to lake evaporation from their measurements/analyses and comparison with previous studies. The phase shift of lake evaporation due to lake heat storage is in a way common knowledge. We would like to hear something new here. How about differences among the TP lakes? For example, in the introduction, the authors mention the difference of lake size change between the interior TP and southern TP. Any new findings on this point? Also try to make clear the relation between the statements made in the introduction (e.g., L58-67) and those in this section. You do not have to say similar things in different parts of the manuscript.

- Section 4.3; In addition to comparison with other lakes in TP, authors may want to address the difference of the TP lakes in comparison with other lakes in the world. What are special about the TP lakes? Are there similar lakes in other parts of the world? What are the controlling factors to make them similar/different?

- L359 "2011-2012", L362 "2013-2015"; Evaporation estimates for these periods are continuous even during the winter season? Clarify this in the manuscript since there is a statement in the introduction saying "lake evaporation throughout the year is not typically investigated".

- Fig.1; Add a scale, latitude/longitude to the lower right figure of the panel a.

- Fig.5 caption; "depth of 0 m"; should it be 0.4 m?

- Fig. 9; Change the solid line into a dotted line when there are missing values for an extended period.

- Fig. 10; what are the spikes in the evaporation values? They are weekly values so that they look strange. The authors should explain this in the main text (perhaps in connection with error estimates).

- Fig. 11; explain in the main text what the averaging period of the plotted data is.

- Table 1; add information on GMX600.

Below are my comments made to the authors' reply of the previous version. Since many of the points were not reflected in the current version, I cite them again here. _____ A. 2. L76-84. The use of temperature and humidity measured at this location and by this instrument for the purpose 175 of calculating Bowen ratio (Bo) is questionable. ..... Reply: Fig.2 is replaced by the Figure below, which shows more detailed information about the installation of the instrument. We also address the location of the instrument in more detailed in the revision (line 94-96). We agree that instrument should be installed in a right place. Paiku Co is a deep lake and has steep shoreline. It is 190 difficult to install the instrument in the lake center. The logger was installed in an outcrop ∼2 m above the lake surface at the north part of the lake. The instrument is under a rock where there is a hole facing the lake. This site is very close to the lake and we believe that it is an ideal place to install the instrument. The meteorological condition over the lake surface can be well recorded

I am not convinced at all without supporting evidence that this is an "ideal place to install the instrument" and that the "meteorological condition over the lake surface can be well recorded". Since measurement location does not satisfy what the theory requires, it seems to me that the only way to convince readers is to show that their measurements are not very different from those on lake surfaces, and the minor difference does not propagate into the Bowen ratio estimation too much.

B - The sensor specification states the accuracy of _0.35_C for temperature and _2.5% for RH (from10% to 90%). 205 They are not particularly high. The accuracy of the water temperature sensor is _0.2_C. What would be the resulting accuracy of Bo and fluxes? The final possible error of the estimated fluxes would be due to (1) plus (2). Reply: It is true that the instrument we used in this study is designed for indoor use. We selected this instrument for measuring air temperature and humidity because it is cheap and easy to install. In fact, the instrument is installed just under a big stole where there is good ventilation, so the meteorological condition over the lake 210 surface can be well recorded. The accuracy of air temperature and humidity is also addressed in the revision (line 92-93). The accuracy of Bowen ratio is estimated in the revision (line 313-318). 'The accuracy of Bowen ratio depends on the accuracy of temperature and water vapor differences between lake surface and the overlying atmosphere. The HOBO instrument has an accuracy of 0.35 oC for air temperature and 2.5% of relative humidity. The HOBO water temperature sensor has an accuracy of 0.2 oC. Therefore, the accuracy of temperature difference between lake surface and the overlying atmosphere is estimated to be 0.4 oC (=$\sqrt{}$0.352+0.22). The water vapor difference between lake surface and the overlying atmosphere is averaged to be 0.57 kPa between June 2015 and May 2016. Therefore, the error of Bowen ratio is estimated to be 0.03, according to equation (4) in the main text.'

They are an estimation based on sensor specifications. What happened to the radiation effect on the measurements?

C 4. L103-106. Authors assumed Ts=Tw "because surface water can be mixed quickly by wind in the afternoon" and used Tw for their flux estimation. Please show the data to validate this statement. If no data are available, authors may want to add an argument that a small difference between Ts and Tw does not produce large 260 estimation errors of Bo and fluxes. In general, Ts is not equal to Tw even under windy conditions (see., e.g., Prats et al., Earth Syst. Sci Data, 10, 727-743, 2018). Reply: It is true that Ts is not equal to Tw. In this study, we do not measure the surface water temperature

and lake water temperature at 0.4∼0.8 m is used to represent the surface water temperature. However, there is small difference between them and this difference does not produce large estimation error of Bo and heat fluxes.

How do you know that "this difference does not produce large estimation error of Bo and heat fluxes"? I would like to see the evidence.

D Reply: Thanks for pointing out this. The equation should be 'S=ΣðİŚŘðİŚď×ðİIJŇðİŚď×∆ðİŚĽðİŚŰ×∆ðİŚĞðİŚŰ72.8ðİŚŰ=0ðİŘťðİŚŹ'. Here ðİŚŘðİŚď is the specific heat of water (J kg-1 K-1), ðİIJŇðİŚď is water density (kg m-3), ∆ðİŚĽðİŚŰ is the lake volume at certain depth, and ∆ðİŚĞðİŚŰ is water temperature change at the same depth, ðİŘťðİŚŹ is lake area (m2). Changes in lake heat storage are calculated at an interval of 5 m and therefore there are 13 layers in vertical direction. Lake volume is acquired according to the 5 m isobaths. Lake water temperature at each layer is taken as the average value between the top and bottom lay. We do not estimate the accuracy of lake heat storage in this study because both the lake bathymetry and lake water temperature are all in-situ measurement and the error can be neglected (line 140-147).

I do not think "the error can be neglected" "because the lake bathymetry and lake water temperature are all in-situ measurements." If they are based on measurements, there are always errors in the measurements. Sensor errors, sampling errors, etc, etc...

E 7. L170-174. "water circulation"; this is an interesting point. Are there any supporting data for the presence of such circulation? Reply: We discuss this in more detailed in the revision (line 208-214). 'This contrasting pattern of water temperature at the bottom layer occurred during the late summer or early autumn when the vertical temperature gradient started to decrease. As shown in Fig.3, both the start and end of lake stratification were about half a month earlier in the southern basin relative to the northern basin. However, deeper water convection occurred earlier in the northern basin relative to the southern basin during this period (Fig. 3) due to relatively lower vertical temper-

ature gradient in the northern basin. Lower temperature gradient caused deeper water convection in the northern basin compared with the southern basin during the late summer and early autumn

The authors do not directly reply to my question. Perhaps authors do reply to it indirectly, but and I do not understand the logic of the authors' reply.

F 13. L333-335. "In-situ observations of runoff at the three main rivers indicate that the surface runoff had weak impact on lake level changes.....(Table 3)"; Discharge values in Table 3 are only for short durations. Are those periods during baseflow? What would happen in case of rainfall-runoff events, or snow melting discharge? Reply: Runoff measurement was mainly conducted in late May or early October when the water level is already low. Besides runoff measurement in the three rivers, water level is also records by using HOBO water level loggers. We found that this discharge can approximately represent the average state in spring and autumn. Fieldwork in early April 2018 shows that there was almost no surface runoff between January and March.

The authors do not directly reply to my question. Perhaps the authors' reply is an indirect manner, but and I do not understand the logic of the authors' reply.

Specific comments: 1. Bowen ratio As I mentioned above, the authors' reply has not convinced me that their measurements can be used to estimate the Bowen ratio above the lake. Since it appears that they do not have evidence that their temperature and humidity measurements are equivalent to those over the lake, what I could suggest is to stop using the name of the "Bowen ratio method". Instead, they could introduce a new method (or a new name), a kind of empirical Bowen ratio method, or a relaxed version of the Bowen ratio method. In this method, a new variable Bo' is defined similarly to the Bowen ratio Bo but air temperature and humidity over the lake surface are replaced with that those over the land surface. Ideally, the validity of this empirical method should be studied first with an independent method. But alternatively, authors could do it indirectly through the comparison of evaporation with water level decrease as they have done in

their study. The only difference is whether they refer their method as the Bowen ratio method or not; I simply do not think it is appropriate to call it Bowen ratio based on their measurement. By the way, this type of empirical methods have been proposed and applied to lake evaporation estimation. For example, Harbeck (1962)'s empirical mass transfer formula for evaporation is E=N u (es-ea). N is the mass transfer coefficient, u is wind speed, es is at the water surface, but ea is over the land surface.

2. Wind speed According to the authors' reply L226-226, wind speed in the study area is not available. Yet, in the main text, there are repeated mentions of wind regime in the study area (e.g., L155, L158, L167, and L168)

- L117 "groundwater"; there is no discussion on groundwater and yet authors came to the conclusion here. - L269-271, "Negative value Bowen ratio indicates the lake water absorbs energy from the overlying atmosphere, and positive value indicates the lake water releases energy to the overlying atmosphere."; this is not quite true. Negative value only means the sin of H and LE are different. Depending on the relative absolute magnitude of H and LE, energy can either be absorbed or released by the lake. |For example, if H|>|LE| and H>0>LE, then H+LE>0 and the lake water does not absorb energy from the overlying atmosphere. - L283-284; "Sensible heat flux was negative between April and July with an average value of -5.6 WÂům-2 (Fig. 9b), which was mainly due to the negative temperature difference between surface lake water and the overlying atmosphere"; it is not "mainly". The negative temperature difference is the ONLY reason for negative H. - Fig.1 (C); what the difference between the squares and circles?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-320, 2020.