Dear Editor,

Thank you for the comments and recommendations, we were paying attention to each of them; however, the second comment was considered as the most important for the revision of the manuscript. We discussed this comment in more detail in the revised version of the manuscript, and then implemented as many suggestions by the Editor as possible. We added two new figures to the revised manuscript: (a) to show the agreement between the measurements by two temperature sensors installed in the lake (new Fig. 4a); and (b) the 30-minute time series of evaporation calculated from the EC measurement to show the amount of the data excluded from other analysis (new Fig. 5). We also gave the explanations why deriving the mass-transfer coefficients was not included in this study, and then improved the description of the dataset with EC measurements done on the shore of the glacial lake in Antarctica (included in the supplement for this manuscript).

We chased the list with the references for this manuscript by adding:

1. Agustsson, H., Olafsson. H.: Mean gust factors in complex terrain. *Meteorol. Z.* **13**: 149–155, 2004.
2. Guest, P. S.: Inside katabatic winds over the Terra Nova Bay polynya: 2. Dynamic and thermodynamic analyses. Journal of Geophysical Research: Atmospheres, 126, e2021JD034904. https://doi.org/10.1029/2021JD034904, 2021.
3. Valkonen, T., Vihma, T., Kirkwood, S., Johansson, M. M.: Fine-scale model simulation of gravity waves generated by Basen nunatak in Antarctica. *Tellus*, 62A, 319–332, 2010.

We have changed the plain text describing the finding of this study: "The evaporation over the ice-free glacial lake was measured in January 2018, and the uncertainties inherent to five indirect methods were quantified. Results show that in summer up to 5 mm of water evaporated daily from the ice-free surface of the glacial lake located in Antarctica. The indirect methods underestimated the evaporation over the lakes' surface up to 72%. The results are important for estimating the evaporation over polar regions where a growing amount of lakes is recently evident."

We further answer the comments step-by-step.

MAIN COMMENTS:

<u>Comment 1:</u> Lines 536-544: Based on the previous review round, I wonder why the authors have not used a formula having the same structure as the formulas that they tested a(1+b w_2)(e_s−e_2), with the coefficients a and b calibrated for the specific case of your lake. This was explicitly requested by Reviewer 1 and by myself, and I believe that this test is required. Then, of course, it is interesting to consider other formulas as such as e.g., (a+b w_2)(e_s−e_2) and a w_2^b(e_s−e_2) as done by the authors. In this paragraph, the authors should specify how they fitted the parameters (Least Squares Fitting?)
Am I wrong or just the first equation (a+b w_2)(e_s−e_2) has been tested by the Authors? This comment only applies if the water temperature data are representative and thus can be used in combination equations (see the following two comments).

Answer: Thank you for this comment. We were following the recommendations by Editor and Referee 1 and, therefore, we derived the empirical coefficients for the relationship during the first revision. The relationship was written in formula which is not the same as for those (so-called

Dalton-type equations) we applied in this study; and during the second revision we derived the empirical coefficients for the relationship written with the same formula as the empirical equations by Penman (1948), Doorenbos and Pruitt (1975) and Odrova (1979):
$E = (a + b * w_2) * (e_s - e_2) = \{ [1/a] * (1 + [b/a] * w_2) \} * (e_s - e_2)$. The only formula by Shuttleworth (1993) differs from others.

In the revised version of the manuscript, the formula of the empirical equation is the same. We also have tried to estimate the empirical coefficients in the formula w_2^b(e_s–e_2); and then use them to calculate the daily evaporation during the revision. It is done because the points in the relationships between E/(e_s – e_2) and w_2 show the cloud of points (Fig. XX), which subjectively seems to be better approximated with the function like a w_2^b(e_s – e_2), it is confirmed by the calculations. We were considering including the results into the revised manuscript, however finally we refrained from it because of the criticism which was raised by using the non-Dalton-type formula for the empirical relationship in the previous step of the revision.

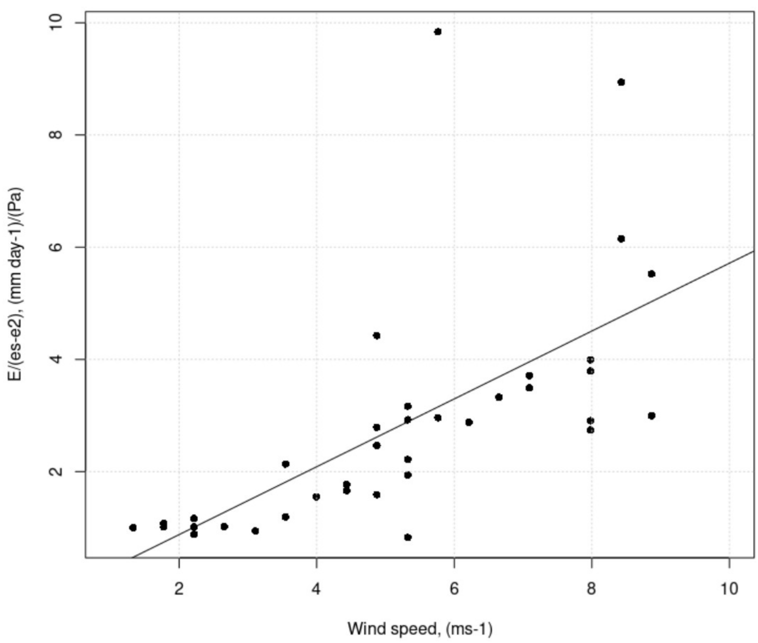

Figure XX: To derive the empirical coefficients for the combination equation: the line indicated the linear approximation for the equation $E = (a + b * w_2) * (e_s - e_2)$.

The sentences about the second empirical relationship aw_2^b(e_s–e_2) were excluded from the revised manuscript. We also added that the least squares method was applied in the fitting of the empirical coefficients in our relationship.

The text was modified as follows: "The empirical coefficients in the combination equations usually limit their applicability to the region where such coefficients are obtained (Finch and Hall, 2005). The empirical coefficients in four selected equations are evaluated from data gathered in regions with different climates, and therefore they probably will not be applicable for lakes located in Antarctica. In this study, we suggested the regional empirical relationships based on the daily series of evaporation estimated by the direct EC method and the meteorological observations at the Maitri site, which is the nearest meteorological site to the lake. The evaporation (E, mm day^{-1}) was evaluated with the linear model $(a + b\, w_2)\, (e_s - e_2)$, where a and b are fitted with empirical coefficients, and $(e_s - e_2)$ is expressed in mbar. The efficiency of fitting the coefficients was performed on the same data for the experiment (lasting 38 days); the least squares method was applied in the fitting of the empirical coefficients in our relationship."

Comment 2: *Lines 730-731: in their response letter, the authors commented that "A bias in the surface temperature (controlling the surface saturation specific humidity) is enough to yield unreliable transfer coefficients for the bulk method." For this reason, when deriving the turbulent transfer coefficients for momentum and moisture from their data they did not "consider the results accurate enough to be recommended for a wider use in estimating evaporation over Antarctic lakes". I wonder that the same considerations hold true also for deriving the empirical coefficients of a combination equation: if the water temperature is not representative of lake surface water temperature (biased), then this will affect the saturated vapour pressure of the air at the water surface temperature (e_s) and consequently the empirical coefficients, thus preventing from the application of available relationships or from the derivation of new relationships.*

Answer: Thank you for the important comment. We admit that we had not presented clear arguments for our decision to use the data set for deriving coefficients for the combination equation but not for the bulk-aerodynamic equation. We carried out further analyses on the uncertainties in the measurements of the lake surface temperature (LST); they were done with two temperature sensors installed in the lake, and it was concluded that they are accurate within 0.5 C (and it is with the measurement's precision of one of the sensors). Utilizing this result, we made sensitivity tests for the dependence of the bulk-transfer coefficients, derived on the basis of our data, on LST. It appeared that the inaccuracy of LST cannot explain the two strange aspects of the bulk-transfer coefficients based on our data: (1) the larger magnitude of the transfer coefficient for moisture than that for momentum, and (2) the strong wind dependency of the moisture transfer coefficient.

We interpret the situation so that these strange aspects, contradicting the literature on bulk transfer coefficients, may arise from three potential factors (a) evaporation from spray droplets, which is sometimes very large, when dry Antarctic air masses are advected over open water (Guest, 2021), but not accounted for by the bulk formulae; (b) non-local factors affecting turbulence in the lake; or (c) some unidentified error source in the data or from particular. By (b) we mean that turbulence over a small lake may be affected not only by the roughness and stratification over the lake surface but also by non-local factors, such as orography of the nunataks and glaciers upwind of the lake. Even if the flux footprint is over the lake, the structure of turbulence may be affected from more remote areas. For example, orography has a strong impact on gustiness of the wind (Agustsson and Olafsson, 2004), which directly affects turbulent mixing, and gravity waves are common downwind of nunataks (Valkonen et al., 2010), their breaking generating turbulence.
Hence, it is not guaranteed that the bulk transfer coefficients based on our data will be useful for estimating evaporation from other Antarctic lakes. Each lake has specific topography/orography around it, and the optimal transfer coefficients may therefore vary a lot between lakes. We therefore do not consider it useful to publish the detailed equations for the bulk transfer coefficients based on our data. Publishing them could be misinterpreted so that we try to show that the transfer coefficient for moisture increases with wind speed, although the apparent increase may be due to spray evaporation, which is not accounted for by the bulk formulae.

We have added text on these issues in the Discussion section: "We also applied the EC measurements to derive new mass-transfer coefficients for the bulk method; however, the results shown two strange aspects: (1) the larger magnitude of the transfer coefficient for moisture than that for momentum, and (2) the strong wind dependency of the moisture transfer coefficient. We interpreted the situation so that these strange aspects, contradicting the literature on bulk-transfer coefficients, may arise from three potential factors (a) evaporation from spray droplets, which is sometimes very large, when dry Antarctic air masses are advected over open water (Guest, 2021), but not accounted for by the bulk formulae; (b) non-local factors affecting turbulence in the lake; or (c) some unidentified error source in the data or from particular. By (b) we mean that turbulence over a small lake may be affected not only by the roughness and stratification over the lake surface but also by non-local factors, such as orography of the nunataks and glaciers upwind of the lake. Even if the flux footprint is over the lake, the structure of turbulence may be affected from more remote areas. For example, orography has a strong impact on gustiness of the wind (Agustsson and Olafsson, 2004), which directly affects turbulent mixing, and gravity waves are common downwind of nunataks (Valkonen et al., 2010), their breaking generates turbulence. Hence, it is not guaranteed that the bulk transfer coefficients based on our data will be useful for estimating evaporation from other Antarctic lakes. Each lake has specific topography/orography around it, and the optimal transfer coefficients may therefore vary a lot between lakes. We therefore do not consider it useful to publish the detailed equations for the bulk transfer coefficients based on our data. Publishing them could be misinterpreted so that we try to show that the transfer coefficient for moisture increases with wind speed, although the apparent increase may be due to spray evaporation, which is not accounted for by the bulk formulae." In the section of the conclusions, we further stress that the empirical coefficients derived for the combination equation are specific for Lake Zub/Priyadarshini, and not necessarily valid for other Antarctic lakes. We also added three new references.

We now included the analysis of the inaccuracy inherent in the measurement of the LST which were measured during the period of 12 days with two sensors (Hobo and iButton); they were installed at the depth of 0.2 m in two different places; and both sensors were new, factory calibrated. The sensors measure the temperature with different precision (0.1 and 0.5 C) every 10 minute. We used the 10-minute values measured by two temperature sensors to estimate the difference between the measured LST: and the mean was -0.05 C; and this value is comparable to the precision of measurements by one of the sensors. The standard deviation in the differences between the LST's measurements was 0.62; and the correlation between the LST measured by two sensors equals 0.94. The estimates of the evaporation with the indirect method were done based on the LST measurements of the sensor of better precision (0.1 C); however we used the measurements by the sensor with the precision of 0.5 C while estimating the evaporation with the new empirical coefficients during the period of 12 days (discussed in the answer to the comment to the lines 732-735).

The text was modified as following: "	We measured the water temperature of the lake's surface with two sensors during the period of 14 days: the iButton temperature sensor was installed in Lake Zub/Priyadarshini in the depth of 0.2 metres and was placed ahead of the EC station (Irgason) toward the prevailing wind directions. The Hobo temperature sensor was deployed in the depth of 0.2 metres in the end of the stream inletting the neighbouring lake (Fig. 1 c). This stream is an outlet of Lake Zub/Priyadarshini, and we assumed that the observations collected by the Hobo were representative for the stream more than for the neighbouring lake itself. The accuracy of both temperature sensors is similar, and the resolution of the Hobo temperature sensor is better than the iButton's precision. The lake surface temperature was measured every 10 minutes, and we further calculated the daily average time series of the water temperature in the lake. The mean difference between the measured lake surface temperature is –0.05 °C; and it is comparable to the precision of the iButton temperature sensor (Table 2). The correlation coefficient between the 10-minute series of the water temperature measured by two temperature sensors Hobo and iButton equals 0.94 (Figure 4 a). We further used the measurements collected by the temperature sensor with better precision (Hobo) to estimate the evaporation over Lake Zub/Priyadarshini in January 2018. Figure 4 b shows the daily time series of the lake water temperature and air temperature during the period of the experiment on the shore of Lake Zub/Priyadarshini."

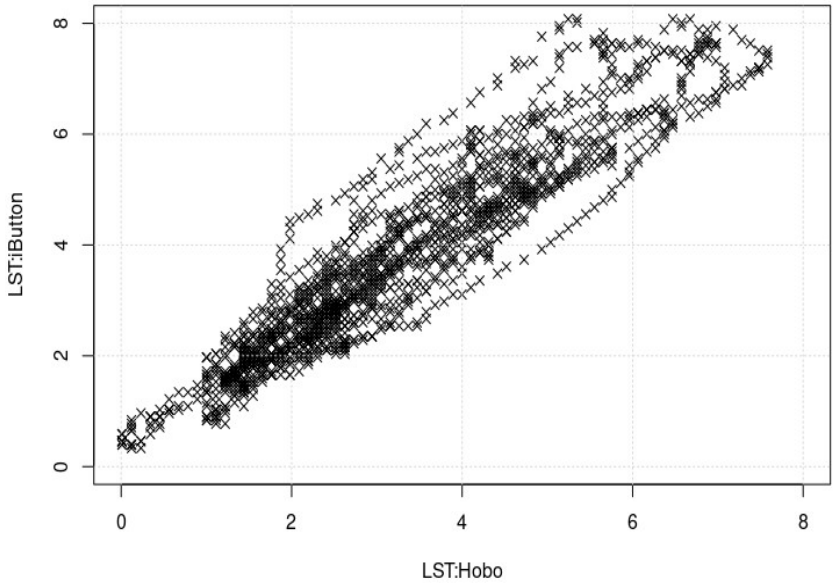

Figure 4 a: 10-minute lake's surface temperature (LST) measured by Hobo temperature sensor (x-axis) and iButton sensor (Y-axis).

<u>Comment 3:</u> I invite the Authors to carefully consider the above comments. If the derivation of empirical coefficients to be used in a combination equation representative for Antarctic lakes is not possible due to the limitations of the water temperature data, I think that the Authors should put emphasis on the interesting EC evaporation dataset that they have acquired: i) adding some statistics and summary plots, ii) improving the analysis of the processes, and iii) allowing for a smooth interpretation and possible re-use of the data.

Answer. See our response above. We consider it relevant to present the empirical coefficients for the combination equation, but stresses that are not necessarily valid for other Antarctic lakes.

… As for this last point, do the data available at https://zenodo.org/record/3469570#.YkrwCedBxPY include also the evaporation estimates? Please, add a read me file to support the understanding and re-use of the data in the suppl. material (e.g., meaning of the columns in 20180101_20180207_EC_FLUX.txt)

Answer: Thank you for the comment. We used the unprocessed data of the EC measuring system (Irgason), published at https://zenodo.org/record/3469570#.YkrwCedBxPY ; the dataset consists of 10 Hz measurements which we used in our calculation of the evaporation and this data is available for re-using. This manuscript will share the post-processed data with the calculations of 30 minute evaporation and fluxes (2*0180101_20180207_EC_FLUX.txt*) included in the supplement. Once this manuscript will be published, this data will be available for re-using with the reference to this study. The explanation of the names and units for all variables given in the file is already included in the header of the file, therefore we did not include a separate read_me file.

My point is that if you know that the water temperature data acquired with the Hobo sensor "were representative for the stream more than for the neighboring lake itself", then it is more or less expected that when using these data into empirical equations to estimate evaporation, the results will be biased compared to the EC data. If this is the case, I would reduce the emphasis on the use of existing parameterizations and derivation of new ones and, as suggested above, focus more on presenting the EC dataset.

Answer: We appreciated this recommendation. We assumed that the measurement of the lake's surface temperature is acquired enough to estimate the daily evaporation by the combination equations.

ALL COMMENTS:
Abstract: There are some sentences that need clarification. The concept is clear, but I think that adding some additional words can avoid possible misinterpretations of what is written.
Line 9: "The lake is among the warmest glacial lakes" in the oasis?

Answer: We agreed, the sentence has changed.

Line 17: "This method" which one, EC of bulk-aerodynamic?

Answer: We agreed, the sentence has changed.

Line 18: underestimation compared to EC?

Answer: Answer: We agreed, the sentence has changed.

We further modified the text of the abstract as follows: "The study provides estimates of summertime evaporation over a glacial lake located in the Schirmacher oasis, Dronning Maud Land, East Antarctica. Lake Zub/Priyadarshini is the second largest lake in the oasis, and its maximum depth is 6 m. The lake is among the warmest glacial lakes in the oasis, and it is free of ice during almost two summer months. The summertime evaporation over the ice-free lake was measured using the eddy covariance method, and estimated on the basis of the five indirect methods (bulk-aerodynamic method and four combination equations). We used meteorological and hydrological measurements collected during a field experiment carried out in 2018. The eddy covariance method was considered the most accurate, and the evaporation was estimated to be 114 mm for the period from 1 January to 7 February 2018 (38 days) on the basis of this method. The average daily evaporation was 3.0 mm day^{-1} in January 2018. During the period of the experiment, the largest changes in daily evaporation were driven by synoptic-scale atmospheric processes rather than local katabatic winds. The bulk-aerodynamic method suggests the average daily evaporation to be 2.0 mm day^{-1}, and it is 32 % less than the results based on the eddy covariance method. The bulk-aerodynamic method is much better in producing the day-to-day variations in evaporation compared to the combination equations. All selected combination equations underestimated underestimated the evaporation over the lake by 40–72 %. The scope of the uncertainties inherent in the indirect method does not allow to apply them while estimating the daily evaporation over Lake Zub/Priyadarshini. We suggested a new combination equation to evaluate the summertime evaporation over the lake's surface from meteorological observations from the nearest site. The performance of the new equation is better than the performance of the indirect methods considered. After this equation, the evaporation over the period of the experiment was 124 mm, which is only 9 % larger than the result according to the eddy covariance method.”

Line 119: "For Antarctic applications of the bulk method for evaporation and latent heat flux" --> "For applications of the bulk method for evaporation and latent heat flux in Antarctica"

Answer: We agreed, the sentence has changed.

Lines 187-188: "In this dataset the location of the lakes in the Schirmacher oasis were systematically shifted to the LIMA composite" What is the reason? Different projection or errors/deformation? In the first case, please re-project one of the two maps. In the second case, please add a comment.

Answer: Thank you for this comment. We think that the shifts in the location of the lakes is sourced by the errors/deformation in the SCAR dataset. These deformations appeared locally in the vicinity of the Schirmacher oasis; ie. in the Larsemann Hills oasis, no deformations were evident in the SCAR dataset. In our opinion, the correction of the local deformation in the location of the lakes is outside of this study where we wanted to mention that such local deformation exists for the SCAR dataset. In the revised version of the manuscript we excluded the information about the shifts because it is not relevant for this study. We also corrected Figure 1 by excluding the location of the lakes in the Schirmacher oasis.

Line 226: "(also known as Lake Priyadarshini), and hereafter we will use both names of the lake" --> "(also known as Lake Priyadarshini; hereafter we will use both names of the lake)"

Answer: We agreed, the sentence has changed.

Figure 2: please, specify the source of the data and period of analysis also in the caption. Units of measure are still missing in the legend. Please, specify what you mean by wind anomalies: I guess it is the anomaly relative to the long-term mean value, but this must be explicitly specified.

Answer: Thank you for the comment, we not correct the caption for the Figure 6 as follows: “Figure 2: Wind direction and wind speed anomalies for two austral summer months (December and January): the data extracted from the British Antarctic survey dataset (available at https://www.bas.ac.uk) for the period 1998–2016.“ The text was also corrected: “We also evaluated the wind speed anomalies of each 10-degree sector given in colour codes in Fig. 2: the anomalies are calculated as the difference between the observed value and the long-term mean value estimated for the period of 1998–2016 in our study.” The unit of the anomalies of the wind speed is added to the figure 2.

Table 2: in the caption you mention the Solinst logger, which is also reported in Fig 1c. However, it is not cited in the text. Please, explain that this instrument was used to monitor the water level. Be consistent with the number of decimals in the table (elevation with 1 decimal, e.g., 124.0 and

122.0 m a.s.l.). If the pressure measured by the Hobo has not been used, I would skip it from table and text.

Answer: We removed the Solinst logger from the figure and the text. We corrected the number of decimals given for the elevation in Table 2. We skip the description of the pressure measurements by Hobo from Table 2 and also from the text.
We corrected the revised text as follows: “30 December 2017, the elevation of the lake water level was measured by the geodetic instrument Leica CS10; the level was 122.3 m, WGS84 ellipsoid vertical datum. We used this elevation to calculate the elevation of the Hobo, iButton and Irgason temperature sensors. The Leica CS10 instrument was used to measure the elevation of the Maitri site in January 2018 (Dhote et al., 2021).”

In the caption: "The elevation of the lake water level was 122.3 m (WGS84 ellipsoid vertical datum), we further used this elevation while calculating the elevation for the Hobo, iButton and Irgason temperature sensors." --> "The elevation of the lake water level was 122.3 m (WGS84 ellipsoid vertical datum). We used this elevation and the information provided by the Solinist to calculate the elevation of the Hobo, iButton and Irgason temperature sensors." Is this correct? This part can be moved to the main text.

Answer: the text is corrected as suggested; however we did not move the text. What do you mean by “the main text”?

Line 284: should be Fig 1c instead of 2c

Answer: We corrected the text.

Line 322: a full stop is missing

Answer: We corrected the text.

Figure 3c: I think that you should explain how the footprint has been determined. Which footprint parameterization or footprint model has been used? Did you use the parameterization proposed by Kljun et al. (2004) cited before? Please specify.

Answer: Yes, we used the parameterization proposed by Kljun et al. (2004). We revised the following sentence in the text: “The footprint is defined by a sector of wind direction covering the source area and its length depends on the sensors’ height (Kljun et al., 2004; Burba et al., 2016). The footprint was estimated according to the parameterization proposed by Kljun et al. (2004) and the 90% contribution (X90, m) is shown in Fig. 3c.”

Line 390: "and wind speed calculated" Wind speed is not shown

Answer: thank you for the comment. We excluded the mentioning of the wind speed from the revised text.

Line 395: English to be revised. E.g. replace "and the meteorological sensors" with "that"

Answer: We corrected the text.

Lines 395-398: I guess that here you mean the distance from the ground (m above ground), not the elevation (m a.s.l.).

Answer. Thank you for the comment. Yes, the sensors were installed on the different heights over the ground; and the ground elevation at the sites are also differ: Maitri site is located on the hill while the Irgason was installed on the lake’s shore where the elevation is lower. We further modified the text as follows: “In our calculations based on the combination equations we applied the data collected by the meteorological sensors installed both at Maitri and Irgason sites, that are deployed at a different height over the ground. The height over the ground of the temperature sensor and gas analyser of the Irgason is lower than the sensors at Maitri site, and therefore we used the logarithmic approximation of the wind profile to correct the wind speed data measured at the Maitri site, for which we estimated a constant aerodynamic roughness length of 0.002 m (Stull, 2017).”

Section 3.2: I suggest splitting this part into sub sections or paragraphs depending on the method used.

Answer: We split the section into three subsections to separate the description of three methods used in this study.

Line 413: "First, the bad data with less than 50 % of total 10 Hz measurements were excluded." What do you mean here? Over which time window?

Answer: it is in the 30-min time window. We mean that we discard data where more than 50% of the measurements (10 Hz) present malfunctions in the 30-min block. These data are detected in two diagnostic variables, one for the sonic anemometer and another for the gas analyser. We revised the following sentence: “In the first step, we discard data where more than 50% of the measurements (10 Hz) present malfunctions in the 30-min block. These data are detected in two diagnostic variables, one for the sonic anemometer and another for the gas analyser;”

Line 467: check the unit of measure of wind speed: m/s

Answer: We corrected.

Lines 536-544: Based on the previous review round, I wonder why the authors have not used a formula having the same structure as the formulas that they tested $a(1+b w_2)(e_s-e_2)$, with the coefficients a and b calibrated for the specific case of your lake. This was explicitly requested by Reviewer 1 and by myself, and I believe that this test is required. Then, of course, it is interesting to consider other formulas as such as e.g., $(a+b w_2)(e_s-e_2)$ and $a w_2^b(e_s-e_2)$ as done by the authors. In this paragraph, the authors should specify how they fitted the parameters (Least Squares Fitting?)
Am I wrong or just the first equation $(a+b w_2)(e_s-e_2)$ has been tested by the Authors?

Answer: We only included the results for the formula (a+b w_2)(e_s−e_2). The text is corrected as follows: "The evaporation (E, mm day^{-1}) was evaluated with the linear model $(a + b w_2)(e_s - e_2)$, where *a* and *b* are fitted with empirical coefficients, and $(e_s - e_2)$ is expressed in mbar. The efficiency of fitting the coefficients was performed on the same data for the experiment (lasting 38 days); the least squares method was applied in the fitting of the empirical coefficients in our relationship."

We derived the empirical coefficients for the formula with the same structure as the empirical equations by Penman (1948), Doorenbos and Pruitt (1975) and Odrova (1979):
$E = (a + b \cdot w_2) \cdot (e_s - e_2) = \{ [1/a] \cdot (1 + [b/a] \cdot w_2) \} \cdot (e_s - e_2)$. The only formula by Shuttleworth (1993) differs from others.

Table 3: " r is ratio the sum EEC divided by the sum Em" --> " r is the ratio between the sum EEC divided by the sum Em". Please, use either Em or Emod (see also lines 559 and 561)

Answer: We corrected the text as suggested. We also uniformed the notation E_{mod} for the evaporation estimated after the indirect methods.

Line 605: Table 4 not Table 5

Answer: We corrected.

Table 4: The authors excluded the Nash Sutcliffe Efficiency index after my comment in the previous round. However values of SSC > 1 means values of Nash Sutcliffe <0, which indicate that the model is worse than taking the mean. Any model with Nash Sutcliffe < 0 should be avoided. So, saying that the bulk-aerodynamic model is better than the others sounds a bit strange to me. Here the authors should revise their description of the results pointing out that although correlation are typically good, RMSE and SSC are not meaning systematic under/overestimation (in this case underestimation) while the time pattern is satisfactorily reproduced.

Answer: Thank you for this important comment. Indeed, any model should be avoided from the hydrological practice if SSC > 1, and it is mentioned in Popov (1979). Unfortunately, none of the combination equations considered cannot be suggested to apply while estimating the daily evaporation for the water balance studies due to big uncertainties inherent in these methods. It needs to find what method allows better estimation of the daily evaporation over the lakes located in Antarctica.
We corrected the text as follows: "Popov (1979) suggests that any model is applicable for hydrological practice if only SSC < 0.8. Unfortunately, none of the considered combination equations cannot be suggested while estimating the daily evaporation for the water balance studies due to big uncertainties inherent in these methods. It needs to derive the regional coefficients for the combination equation allowing better daily evaporation over Lake Zub/Priyadarshini."

Lines 624-625: Does "mean difference" refer to mean absolute error?

Answer: We corrected the text as follows: "The mean absolute error of the bulk-aerodynamic method ..."

Line 627: according to my comment above: why the authors have not considered the same formula? This is needed for fair comparison. Then, if they want to consider other similar formulas this is certainly interesting and useful.

Answer: Thank you for this comment. Indeed, we were following the recommendations by Editor and Referee 1 and therefore we derived the empirical coefficients for the relationship during the first revision. The relationship was written in formula which is not the same as for the known (so-called Dalton-type equations); and during the second revision we derived the empirical coefficients for the relationship written with the same formula as other empirical equations: $E = (a + b * w_2)*(e_s - e_2) = [1/a] * (1 + [b/a]*w_2) * (e_s - e_2)$. So, in the current version of the manuscript, the formula of the empirical equation is the same. In this context, we did not understand the comment on "why the authors have not considered the same formula?" We have applied the same formula. In our next manuscript, we derive the empirical coefficients for other formulas since the results may be better than for the Dalton-type formulas.

Line 644 and also 651: Peason-->Pearson Check for other occurrences.

Answer: We corrected.

Line 646: "showing the better scope" please, rephrase

Answer: We corrected the text as follows: "... showing the better results in the estimations of the daily evaporation."

Lines 646-647: please clarify this sentence. As the coefficients are obtained from data fitting, they are necessarily dependent on the observed data. This comment also applies to lines 731-732.

Answer: Indeed, it would need to specially notice that the errors were estimated from the same data as the empirical coefficients are derived. We changed this sentence as follows: "The independent data are needed to test the new empirical equation."

Line 650: iBuntton --> iButton.

Answer: We corrected.

Table 5: specific humidity should be indicated either with capital or lowercase q (coherently with eq 1)

Answer: We corrected.

Lines 730-731: in their response letter, the authors commented that "A bias in the surface temperature (controlling the surface saturation specific humidity) is enough to yield unreliable transfer coefficients for the bulk method." For this reason, when deriving the turbulent transfer coefficients for momentum and moisture from their data they did not "consider the results accurate enough to be recommended for a wider use in estimating evaporation over Antarctic lakes". I wonder that the same considerations hold true also for deriving the empirical coefficients of a combination equation: if the water temperature is not representative (biased), then this will affect the saturated vapour pressure of the air at the water surface temperature (e_s) and consequently the empirical coefficients, thus preventing from the application of available relationships or from the derivation of new relationships.

Answer: See our response for the comment 2 above. We consider it relevant to present the empirical coefficients for the combination equation, but stress that they are not necessarily valid for other Antarctic lakes.

Lines 732-735: please clarify/revise these sentences. As for the revision: e.g., "using the independent data on" --> "using independent data of" and explain what you mean (different meteo station and water temperature sensor). As for clarification, please better explain what are the limitations.

Answer: Thank you for this comment. The empirical coefficients in the relationship were estimated from the evaporation estimated from the EC measurements, and also from the measurements of wind speed, air temperature and lake surface temperature. The wind speed and air temperature were measured in two sites (Irgason and Maitri) during 38 days; and also the lake surface temperature was measured at two sites with two temperature sensors (Hobo and iButton) for the period of 38 days and 12 days. Therefore, we used the measurements of wind speed, air temperature at Maitri site, the lake's surface temperature measured by Hobo and daily evaporation by EC method to derive the empirical coefficients in the relationship. Then, we estimated the evaporation using the newly derived relationship for the period of 12 days with the wind speed and air temperature measured at Irgason site, and also the lake’s surface temperature measured by iButton. However, the measured evaporation by EC method during the same period (12 days) was only possible for the comparison of the results, therefore the estimations of the efficiency for the new relationship is not fully independent.

The text was corrected as follows: “We derived the regional empirical coefficients for the combination equation, and it can be potentially used in estimations of the evaporation over the ice-free glacial lakes located in Schirmacher oasis. The empirical coefficients in the relationship were derived from the evaporation estimated from the EC measurements, and from the measurements of wind speed, air temperature and lake surface temperature. The wind speed and air temperature were measured in two sites (Irgason and Maitri) during 38 days. The lake surface temperature was measured at two sites with two temperature sensors (Hobo and iButton) for the period of 38 days and 14 days. We used the measurements of wind speed, air temperature at Maitri site, the lake's surface temperature measured by Hobo and daily evaporation by EC method to derive the empirical coefficients in the relationship. Then, we estimated the evaporation using the newly derived relationship for the period of 12 days with the wind speed and air temperature measured at Irgason site, and also the lake’s surface temperature measured by iButton. However, the measured evaporation by EC method during the same period (12 days) was only possible for the comparison of the results, therefore the estimations of the efficiency for the new relationship is not fully independent. Therefore we would not suggest applying these coefficients as the regional references without further analysis. In this study, we did not estimate the evaporation using the energy balance method, but plan to further evaluate the uncertainties inherent also in this method while estimating the evaporation over the glacial lakes located in Antarctica.”

Line 745: "Thy" --> "They"

Answer: We corrected.

Line 748: "caould" --> "could" This sentence is incomplete.

Answer: We corrected the typos, and the sentence as follows: "… but the daily evaporation rates are of the same order of magnitude, and although one could expect a much larger evaporation over the surface of the landlocked lakes than over the surface of the glacial lakes."

Line 749: "method by" --> "the method by"

Answer: corrected.

Lines: 789-794: This part should be better linked to the results of the present analysis. While the following sentence, perhaps, could be expanded considering that Dhote et al. (2021) analyzed the same lake and period.

Answer: Thank you for the comments. The evaporation over the glacial lakes is rarely estimated for the lakes located in Antarctica; and we included as many estimates of the evaporation (sublimation) by various methods as we have found in the literature. Many of the evaporation estimates are obtained by the empirical equations or energy balance method; the uncertainties inherent in these methods are unknown as well as the uncertainties in the method of sticks applied by Faucher et al. (2019).

We further modified the text as follows: "Faucher et al. (2019) evaluated the evaporation (sublimation) over the surface of the glacial Lake Untersee, Dronning Maud Land, East Antarctica (71° S). Lake Undersee is perennially frozen year-round, this lake is directly attached to the continental ice sheet; not being the landlocked type lake as given by the authors. The evaporation over the lake surface was estimated based on two years of measurements by sticks installed on the lake's surface. The water losses from the ice-covered surface of the lake due to sublimation (evaporation) to be from 400 to 750 mm year-1; and the daily evaporation from the lake surface was approximately 1.1–2.1 mm day-1, however the uncertainties inherent in measurements by sticks are not known and they need to be also quantified in the future study.

We also extended the text to better present the study by Dhote et al.: "Dhote et al. (2021) study the water budget of Lake Zub/Priyadarshini, which was given a water supply of the Maitri scientific base. The discrepancies in the lake's water budget depend on the uncertainties inherent in methods used to estimate the lake's budget components; and the evaporation over the lake's surface is among others. In this study, the evaporation is calculated with the empirical equation using the observations collected at the Maitri site. The sum of the evaporation over the lake surface was estimated to be 167 mm for two summer months in 2018 (January and February); it is about 2.8 mm day^{-1} and this estimate is close to those based on the EC method given in this study."

We also corrected more sentences in order to make the language of the manuscript soft.

with the best regards,
Elena Shevnina
from behalf of the Authors