Dear Editor,

We thank you for the useful comments and suggestions, and we were following them during the revising of the manuscript. In the revised version, the following principal corrections were implemented: the description of the methods applied to estimate the evaporation over a lake was extended in the introduction; the manuscript has got a new structure, and the quality of the tables and figures were improved. The text of the revised manuscript has got a professional proofreading before the submission. We also revised the list of the references: now it includes 104 scientific manuscripts, and 91 of them are published in English, 11 manuscripts are in Russian, 1 paper is in Portuguese and 1 book is in Finnish.

Added to the references:

Braun, M., H. Saurer, S. Vogt, et al., : The influence of large-scale atmospheric circulation on the surface energy balance of the King George Island ice cap. Int. J. Climatol., 21, 21–36, doi: 10.1002/joc.563, 2001.
Brunke, M. A., Fairall, C. W., Zeng, X., Eymard, L., & Curry, J. A. (2003). Which bulk aerodynamic algorithms are least problematic in computing ocean surface turbulent fluxes? Journal of Climate, 16(4), 619– 635. https://doi.org/10.1175/1520-0442.
Chebotarev A.I. Hydrology. Leningrad: Hydrometizdat; 1975. 544 p. (in Russian)
Edinger, J. E., Duttweiler, D. W., Geyer, J. C.: The response of water temperature to meteorological conditions. Water Resources Research, 4, 1137-1143, 1968.
Ellehoj, M.D., Steen-Larsen, H.C., Johnsen, S.J., Madsen, M.B.: Ice-vapor equilibrium fractionation factor of hydrogen and oxygen isotopes: Experimental investigations and implications for stable water isotope studies. Rapid Commun. Mass Spectrom., 27(19): 2149—2158, 2013.
Favier, V., C. Agosta, C. Genthon, L. Arnaud, A. Trouvillez, and H. Gallée: Modeling the mass and surface heat budgets in a coastal blue ice area of Adelie Land, Antarctica ,J. Geophys. Res., 116, F03017, doi:10.1029/2010JF001939, 2011.
Finch, J.W., Calver, A.: Methods for the quantification of evaporation from lakes. Prepared for the World Meteorological Organization's Commission for Hydrology, Oxfordshire, UK, 41 p., 2008.
Foken, T.: 50 Years of the Monin–Obukhov Similarity Theory. Boundary-Layer Meteorol 119, 431–447, https://doi.org/10.1007/s10546-006-9048-6, 2006.
Hojjati,E., Mahtabi, G., Taran, F., Kisi, O.: Estimating evaporation from reservoirs using energy budget and empirical methods: Alavian dam reservoir, NW Iran. Italian Journal of Agrometeorology (2): 19-34. doi: 10.13128/ijam-1033, 2020.
Holtslag, A. A. M., De Bruin, H. A. R.: Applied Modeling of the Nighttime Surface Energy Balance over Land, *Journal of Applied Meteorology and Climatology*, *27*(6), 689-704, 1988.
How, P., Messerli, A., Mätzler, E., Santoro, M., Wiesmann, A., Caduff, R., Langley, K., Bojesen, M., Paul F., Kääb, A., Carrivick, J.: Greenland-wide inventory of ice marginal lakes using a multi-method approach. *Sci Rep* 11, 4481 https://doi.org/10.1038/s41598-021-83509-1, 2021.
Monin, A.S. and Obukhov, A.M.: Basic laws of turbulent mixing in the surface layer of the atmosphere. *Contrib. Geophys. Inst. Acad. Sci. USSR*, *151*(163), p.e187, 1954.
Monteith, J.: Evaporation and the environment. In: 19th Symposium of the Society of Experimental Biology. Cambridge, UK Cambridge University Press, 205-234, 1965.
Mustonen S. (Ed.), Sovellettu hydrologia. Vesiyhdistys, Helsinki, pp. 291-323, 1986. (in Finnish).
Lu, P., Leppäranta, M., Cheng, B., Li, Z., Istomina, L., and Heygster, G.: The color of melt ponds on

Arctic sea ice, The Cryosphere, 12, 1331–1345, https://doi.org/10.5194/tc-12-1331-2018, 2018.
Konstantinov, A. R.: Evaporation Under Natural Conditions, Israel Program for Scientific Translation, Jerusalem, 523 p., 1968.
Obukhov, A.M.: Turbulence in an Atmosphere with a Non-Uniform Temperature. Boundary-Layer Meteorology, 2, 7-29, https://doi.org/10.1007/BF00718085, 1946.
Salgado, R., Le Moigne, P.: Coupling of the FLake model to the Surfex externalized surface model. Boreal Environment Research, 15:2, 231-244, 2010.
Ramesh, K.J., Soni, V.K., 2018. Perspectives of Antarctic weather monitoring and research efforts. Pol. Sci. 18, 183–188. https://doi.org/10.1016/j. Polar.2018.04.005.
Vignon, É., Roussel, M.-L., Gorodetskaya, I. V., Genthon, C., Berne, A.: Present and future of rainfall in Antarctica. Geophysical Research Letters, 48, e2020GL092281, https://doi.org/10.1029/2020GL092281, 2021.
Vihma, T., J. Uotila, B. Cheng, and J. Launiainen: Surface heat budget over the Weddell Sea: buoy results and comparisons with large-scale models, *J. Geophys. Res.*, 107 (C2), 3013, doi: 10.1029/2000JC00037, 2002.
Webb, E. K.: On Estimating Evaporation with Fluctuating Bowen Ratio, *J. Geophys. Res.* 65, 3415–3417, 1960.
Zhao, L., Xia, J., Xu, Cy. *et al.*: Evapotranspiration estimation methods in hydrological models. *J. Geogr. Sci.* 23, 359–369, https://doi.org/10.1007/s11442-013-1015-9, 2013.

Removed from the references:

Braslavskiy, A. Calculation on evaporation rate from freshwater reservoirs located on flat areas. In book: Materials for a committee on study of methods to calculate an evaporation from water/land surfaces. GGI, Valday, 1966. (in Russian)

Guide to meteorological instruments and methods of observations, WMO No 8: Geneva, Switzerland, ISBN 978-92-63-10008-5, 681 p., 2008.

Kingslake, J., Ely, J., Das, I., Bell R.E.: Widespread movement of meltwater onto and across Antarctic ice shelves. Nature 544, 349–352, https://doi.org/10.1038/nature22049, 2017.

Keijman, J. Q.: The estimation of the energy balance of a lake from simple weather data. Boundary-Layer Meteorol, 7, 399 – 407, doi: 10.1007/BF00240841, 1974.

Kourzeneva, E., Asensio, H., Martin, E., Faroux, S.: Global gridded dataset of lake coverage and lake depth for use in numerical weather prediction and climate modelling, Tellus A., 64, 15640, https://doi.org/10.3402/tellusa.v64i0.15640, 2012.

Majidi, M.; Alizadeh, A.; Farid, A. and Vazifedoust, M.: Estimating evaporation from lakes and reservoirs under limited data conditions in a semi-arid region, Water Resour. Manage., 29, 3711-3733, doi: 10.1007/s11269-015-1025-8, 2015.

Map of the Schirmacher oasis: scale 1:25000. Ministry of Merchant Fleet of the U.S.S.R. (Russia), 3 sheets, 1972.

Nash, J.E., Sutcliffe, J.V.: River flow forecasting through conceptual models: Part I – A discussion of principles. J. Hydrol. 10, 282–290. doi: 10.1016/0022-1694(70)90255-6, 1970.

Sene, K. J., Gash, J. H., McNeil, D. D.: Evaporation from a tropical lake: comparison of theory with direct measurements. Journal of Hydrology, 127, 193–217, 1991.

We further follow the comments given by the Editor and two referees and answer for them point-by-point.

Comment from Editor

ED: “we received the comments from the two Referees that revised your manuscript during the first revision round. Both Referees agree that the manuscript has been improved compared to the original version. Referee #2 has some minor suggestions but Referee #1 still has some major comments on the analysis, which I support. In addition, Referee #1 suggests that the manuscript still needs improvement in terms of English writing. Also in this case I agree with the Referee.

Based on the Referees' comments and on my own reading, I believe that the manuscript is not ready for publication yet. Below I list some specific comments and suggestions to improve the analysis, which add on the comments from Referee #1. I invite the authors to carefully consider these comments, the comments from the two Referees, and the pdf annotations from Referee #1 and resubmit a revised version of the manuscript. I recommend professional English editing before submission”

ED: “Main comment:
The results of the bulk-aerodynamic model are strongly dependent on the choice of the turbulent transfer coefficient. Similarly, those of the Dalton-type equations strongly depend on the values of the empirical coefficients. In the first case, the authors used a value of 1.07E-3 which is rather low compared to other estimates in the literature:

1) Brutsaert, W., 1982. Evaporation into the atmosphere. Theory, History, and Applications. Reidel Publication, 299 pp.;
2) Hicks, B. B. (1972), Some evaluations of drag and bulk transfer coefficients over water bodies of different sizes, Boundary Layer Meteorol., 3,
201–213, doi:10.1007/BF02033919.
3) Ala-Könni, J., Kohonen, K.-M., Leppäranta, M., and Mammarella, I.: Validation of turbulent heat transfer models against eddy covariance flux measurements over a seasonally ice covered lake, Geosci. Model Dev. Discuss. [preprint], https://doi.org/10.5194/gmd-2021-272, in review, 2021
… “

Answer: Thank you for the comment. We do not think that the value of 1.07E-3 is particularly low for a 10-m neutral transfer coefficient for heat and moisture over an open lake. Ala-Könni et al. (in review) write as follows: “Values between CE = CH ≈ 1.0 − 1.5 · 10^{-3} have been reported over the years for neutral conditions at 10 m height, for example in Kagan (1995). In our study a mean of the values reported in Kagan (1995) was taken and the value was scaled to a height of 1.7 m, resulting in a value CE = CH = 1.8 · 10−3.” Our value fits within the range reported above. As the constant-flux layer is assumed, the values are naturally larger when scaled from 10 m to lower heights (1.7 m in the case of Ala-Könni et al., and 2 m in our case). We further note that the lake studied by Ala-Könni et al., as many other boreal lakes, is surrounded by boreal forest. The forest edge typically enhances turbulence while reducing the mean wind, which is reflected as a larger turbulent transfer coefficient. The Antarctic lake we studied was surrounded by open ground and the ice sheet. We do not have access to the very old paper by Hicks.

Further, see below our response to the comment on the utilization of the EC measurements to propose values for turbulent transfer coefficients to be applied in the bulk-aerodynamic method.

ED: "In the second case, in the literature I have not found exactly the same values of the coefficients used by the Authors in eqs 4-6, even looking in the original papers. Are those values from the original papers? Have these values been used in other limnological applications? "

Answer. Indeed, the values for the empirical coefficients in eq 4 are different from those given in the original paper by Penman (1948), where the wind speed is given in miles per day. In this study, the values for eqs 4 and 5 were taken from Tanny et al. (2007), which is one of the examples of applying these types of the equations in limnological studies. The values for the empirical coefficients in eq. 6 are given correctly, and they can be checked in the paper available in English (Shnitnikov, 1974: https://doi.org/10.1080/02626667409493873), which we did not include to the list of references.

The text has changed correspondingly: "In this study, we applied three combination equations to calculate daily evaporation: $E = 0.26\ (1 + 0.54\ w_2)(e_s - e_2)$ and $E = 0.26\ (1 + 0.86\ w_2)(e_s - e_2)$ adopted from Tanny et al. (2008), where these formulas are referred to Penman (1948) and Doorenbos and Pruitt (1975) respectively. These equations are among those which are most often used in hydrological practice (Finch and Calver, 2008), and therefore we have chosen them in this study. We also used the formula $E = 0.14\ (1 + 0.72w_2)(e_s - e_2)$, which has been applied to evaluate evaporation from lakes located in northern Russia (Odrova 1979)."

ED: "However, I fully agree with Referee #1 that the Authors should use a Dalton-type formula of the form E = a(1+bw2)(es−e2), not the equation that they used. The results should be commented in light of the values of these coefficients typically used in limnological literature."

Answer. We removed further all parts related to the empirical relationship. We answered this comment by the referee 1 as follows: "Indeed, the linear relationship suggested to calculate the daily evaporation from the wind speed, humidity and temperature measurements differs from the form of the Dalton-type equations given in eqs. 4-6. However, this particular form is only one among formulas that may include a lake surface area or daily minimum and maximum air temperatures (Hojjati et al., 2020; Zhao et al., 2013)."

We further added the following explanation to the text of the introduction: "We further added the following explanation to the Introduction: " The combination equation approach includes the elements of both energy balance and mass-transfer approaches in the estimation of evaporation. The Penman equation (Penman, 1948) is among the most famous presenting this approach, where evaporation is calculated from the simultaneous solution of diffusion equations for heat and water vapor, and the energy balance equation (Finch and Calver, 2008). A more general form of the combination equation is given by the Penman-Monteith equation (Monteith, 1965), which was developed to describe evaporation from plants (evapotranspiration). There are also a number of empirical formulas that need additional information on lake surface area, radiation, daily minimum and maximum air temperatures, etc. (Hojjati et al., 2020; Zhao et al., 2013) or require only the air temperature and relative humidity to be known (Konstantinov, 1968)."

ED: “Overall, I think that the manuscript would acquire much more relevance and interest if the authors would use their EC measurements to propose values of turbulent transfer (for bulk-aerodynamic model) and empirical (for Dalton-type equations) coefficients, which would be valid for this lake as an example of an Antarctic lake. The latter point has been done, after the suggestion of Referee #1.”

Answer: We followed the suggestion, and utilized our data to derive the turbulent transfer coefficients for momentum and moisture to be applied in the bulk method for estimating and parameterizing fluxes over Antarctic lakes. However, the results did not appear realistic, as the transfer coefficient for moisture was equally large as the drag coefficient. We think that this may result from a negative bias in the lake surface temperature. In such conditions, when regressing the observed evaporation against a negatively biased difference between the surface saturation specific humidity and air specific humidity, the transfer coefficient inevitably becomes too large (see Equation (1) in the manuscript). Hence, we do not consider the results accurate enough to be recommended for a wider use in estimating evaporation over Antarctic lakes. Note that this does not necessarily imply any bias in the Eddy-covariance observations. A bias in the surface temperature (controlling the surface saturation specific humidity) is enough to yield unreliable transfer coefficients for the bulk method.

Other comments by Editor:

ED: “ Line 61: (Guidelines, 2008) check the reference.”

Answer: corrected.

ED: “EC is used at line 62 but the acronym is introduced at line 65.”

Answer: corrected.

ED: “lines 66-67: should "low" be replaced with "lower"? What do the Authors mean here?”

Answer: the sentence was removed. We decided to keep only the next one.

ED: “lines 68-69: this sentence is a repetition of the previous.”

Answer: Indeed. We decided to remove the previous one.

ED:” lines 73-74: it is quite clear that the variation range refers to the coefficients, but the sentence should be reformulated. Also the following sentence (lines 74-75) should be revised: do you mean "are typically/historically/normally evaluated"?

Answer: we removed the sentence about the variation of the evaporation, and formulated the sentences as following: “The disadvantage of the empirical and combination equation approaches is that their application is limited by the features of the location where the empirical coefficients were estimated, and there are no regional values suggested for the Antarctic continent (Finch and Hall, 2001). The combination equations are also named as the Dalton-type equations in Odrova (1979). In this study, we estimated the uncertainties inherent in four equations while estimating the summertime evaporation over lakes located in Antarctica. The equations that were used were by Borghini et al. (2013) and Shevnina and Kourzeneva (2017), however, the uncertainties inherent in the estimations are not yet known."

ED: " In the introduction the Authors dedicate two paragraphs to the EC and Dalton type equations, but not to the bulk-aerodynamic method. Please, write a paragraph on this method as well, citing relevant literature and applications in Antarctica/limnology. "

Answer: The key equations of the bulk-aerodynamic method were presented already in the submitted manuscript (Equations (1) and (2) there). Now we added a paragraph on the method also in the Introduction.

The following text is added: " In the bulk aerodynamic approach, the evaporation is calculated on the basis of data from the Earth surface properties (surface temperature and specific humidity as well as roughness lengths for momentum and moisture/heat) and atmospheric variables (wind speed, specific humidity and air temperature) in the lowermost part of the atmospheric boundary layer. In addition to observational studies on evaporation and associated latent heat flux, the bulk method is the cornerstone for parameterization of the turbulent fluxes of momentum and sensible heat in numerical weather prediction and climate models (Brunke et al., 2003). For Antarctic applications of the bulk method for evaporation and latent heat flux on the basis of in-situ and remote-sensing observations, see Broun et al. (2001), Vihma et al. (2002), Favier et al. (2011), and Boisvert et al. (2020)."

ED: "Also, please refer to the use of ERA5 in the last paragraph of the Introduction."

Answer: using the ERA5 is now mentioned in the introduction. We extended the text as follows: "... The estimates of the evaporation are also available from atmospheric reanalyses which share results of simulations carried out applying numerical weather prediction models. Also in the most recent global atmospheric reanalysis, the ERA5 of the European Centre for Medium-Range Weather Forecasts (Hersbach et al., 2020), the evaporation is estimated based on short-term weather forecasts applying the bulk-aerodynamic method. "

ED: " Figure 1: what do the authors mean by "after map of SA, 1972"?
Is the lake map drawn in 1972 still actual?"

Answer: We excluded the mention of Map of SA (1972), and further we used the images available in Google Earth to show the location of the observational sites. We further implemented the modifications to Figure 1 c.

ED: "Suggestion: "the temporal observation network" -->" the observational network". The colors of the "level and temperature gauge" and "temperature gauge" are the same (please, use colorblind friendly colors)"

Answer: indeed, the colors were almost the same. A new color palette is implemented. Modifications implemented in new Figure 1: new subfigure (b) now shows the shift in the location and lake's boundaries in the ADD dataset; the new color palette is applied to distinguish between the gauges.

ED: "are the bathymetry from ('60 and '80) still representative (see lines 130)?. "

Answer: Indeed, the bathymetry of the biggest lakes is also available from more recent studies. We added more references to the revised text: " … .. lakes (Simonov and Fedotov, 1964; Loopman et al., 1988; Khare et al., 2008; Dhote et al., 2021)." It is difficult to say how representative the bathymetry of the lakes gathered in the 1960s and 1980s may be for the recent state of the lakes in the Schirmacher oasis. The water level (and volume) of Lake Zub/Priyadarshini is reducing continuously: in 2018 the lake water level lowered by approximately 0.4 m (Dhote et al., 2021). Our next study will address the question how the bathymetry of the lakes in the Schirmacher oasis changed from the late 1980s to the late 2010s.

ED: "Table 1: I suggest to indicate that the data refer to min/mean/max not in the footnote, but in the caption or column headers. Having two rows for 2017 and 2018 makes the table longer than needed and may introduce confusion. It is sufficient to refer to the year directly in the column headers. "

Answer: We agree that it is better to move the explanation marked by a sterix from the footnotes. We added the information that the min/mean/max values are estimated from the monthly series of the meteorological variables. We shortened the table as it was suggested.

ED: "The values in the table are not consistent with those provided at lines 110-112. What is the temporal resolution of these values?" and also " Lines 109-110: the sentences should be revised, e.g., "compared to the long term mean values, temperature and wind were …." Line 111: "wind ranged speed" to be revised.

Answers: On lines 109-112, we discussed the weather conditions during the season 2017-2018 based on the daily observations estimated from the data gathered at Novo site. Therefore, the ranges given for the air temperature and wind speed complemented the information given in Table 1, where only the average values are shown for these meteorological parameters. We revised the sentence given on line 110 as follows: "Generally, the weather during the experiment was colder and less windy, while the relative humidity and amount of the precipitation were close to the monthly means estimated for the period 1961 – 2010 (Table 1)."

ED: "Line 117-124: this part should be rewritten and better explained. The Authors should start writing what one should check to effectively plan an EC field campaign and why. Fig 2 misses all units of measure and from the caption and legend one does not understand what the colors refer to. Also the text should be improved, better introducing how/why you calculated wind anomalies and practically how you used this information to plan the fieldwork (e.g., referring to the concept of footprint provided later). "

Answers: This paragraph was moved to the end of section 2 and was improved regarding the plan of the EC measurements campaign.
The paragraph now reads as follows: "Lake Zub/Priyadarshini presents ideal conditions to study evaporation over a glacial lake, and to plan the field experiment, we accounted for the location to set up the EC measuring systems. Selection of the exact site for EC measurements requires, among others, data on the prevailing winds and their fetch over the lake, and naturally also the accessibility for regular maintenance. To evaluate the prevailing wind direction, we used 6-hourly synoptic observations at the Novo site available from the British Antarctic Survey Dataset (https://www.bas.ac.uk, last accessed 14.12.2018) covering the period 1998–2016. We calculated the number of cases when wind was blowing from 36 sectors each 10 degrees wide, and then defined the prevailing wind directions (marked with the black arrows in Fig. 2). The prevailing wind directions range from 110 to 140°.
We also calculated the wind speed anomalies for each 10-degree sector, given in color codes in Fig. 2. Positive wind speed anomalies were often observed within the range of the prevailing wind directions (marked with orange, yellow, red, brown and black in the legend of Fig. 2). Therefore one can expect the majority of strong winds from these directions." We improved Fig. 2 by adding the units to the x-axis, by implementing uniform labeling to the y-axis, and by showing the prevailing wind direction ranges. We also reduced the number of the sub-figures in figure 2.

ED: "Line 118: if the meteorological data are available from https://www.bas.ac.uk, this should be said before, when introducing the monitoring network."

Answer: we agree, and accordingly added the following sentence early in the text: "… Both meteorological sites are included in a long-term monitoring network, and their measurements are done according to the WMO's standards (Turner and Pendlebury 2004)."

ED: " Line 152: properly write CO2 and H2O (use of subscripts)"

Answer: corrected.

ED: " Line 155: please rephrase "Irgason was deployed…". What is the boom? Is the detail about the metal guidelines relevant?

Answers: The boom is the tip - removed. As well as the details about metal guidelines.

ED: "In general, please do not refer to the EC station as to the "Irgason". "

Answer: we agreed that generally the EC station better not refer to the Irgason since it is the only one of the instruments performing the EC measurements. However, we named the components of the observation network as it is suggested by the manufacturers (Hobo, Solinsts, IButton, Irgason), and we refrained to change the name only for one component (Igrason). We added the explanations in the text: "… concentrations, and it is among other EC stations performing this kind of measurements (in this study, we named our EC station as Irgason)…"

ED: "Table 2: the period of the fieldwork should be reported explicitly also in the text, as long as the time resolution of the different measurements. Please report the key technical information of all the sensors (accuracy, resolution, time interval etc) when introducing Table 2. In particular, you used different water temperature sensors: are they comparable in terms of accuracy and resolution?

Answer. We added the reference to the period of the experiment in the second sentence of section 3.1. We also reported in the text the period with the measurements of the water temperature sensors in Table 2.

Table 2: Does the asterisk refer to all elevations? If not how the other elevations were measured?

Answer. We removed the asterix, and given the further comments in the text: "The elevation of the lake water level was measured by the geodetic instrument Leica CS10 during the installation of the Solinst water level logger on 30 December 2017 (Fig. 1 c). The same instrument was used to measure the elevation of the Maitri site in January 2018 (Dhote et al., 2021). The elevation of the lake water level was 122.3 meter (WGS84 ellipsoid vertical datum), we further used this elevation while calculating the elevation for the Hobo, iButton and Irgason temperature sensors."

ED: " Line 162-175: this whole part needs improvement. The concept of footprint and footprint length should be better explained. The quality of Fig3b and 3c should be improved. X90 should be explained."

Answer. Corrected. This paragraph now reads as follows: "The footprint is an important concept for evaluating fluxes correctly with the EC method. The footprint is defined as the area upwind of the EC station where the fluxes observed at the station originate from Hence, the footprint area depends on the location of the EC station, the height of its sensors, the roughness of the upwind surface, and the stratification of the upwind atmospheric surface layer (Kljun et al., 2004; Burba, 2013). The Irgason was settled at the height of 2 metres above the ground, which yields footprint lengths of less than 200 metres, which in this study was defined as X90 and represented 90 % of the cumulative contribution to the fluxes (Fig. 3 c). This distance is less than twice of that between the Irgason and the shore of Lake Zub/Priyadarshini in an east-southeast direction (Fig. 1 c), and it ensures that the measured data is representative only for the lake and free of contamination from the upwind shore. The tower height of 2 m generates a blind zone near the tower, so that the stones on the downwind shore do not affect the fluxes. The location of the EC tower accounted for the prevailing wind directions (Fig. 2) meaning that the footprint area is mainly represented by the lake surface. We filtered out data outside the footprint (Fig. 3 b). Gaps in the wind direction were replaced with the average values of the neighboring 30-minute blocks. The Irgason's raw data consisted of values measured at a frequency of 10 Hz. We used these raw data to calculate a 30-minute time series of evaporation, turbulent fluxes of momentum, sensible heat and latent heat, as well as air temperature, wind speed, and wind direction. The daily evaporations were calculated as a sum of the 30-minute time series. The low observation height of 2 m guarantees that the vertical divergence of the water vapour flux is negligible, and therefore the water vapour flux observed at the height of 2 m represents the surface evaporation. "

ED: "Lines 182-185: this detail should be removed."

Answer. Removed.

ED: “ Lines 193-199 and Fig 4: is there any reference/data to support the last sentence? What is the effect of having the Solinst at 3.9 m instead than at the surface? I do not think that at this depth the temperature is representative of surface conditions. Why are daily min/max provided only for the iButton?

Answer. We removed the sentence from the text. We further did not use the water temperature measurements done by the Solinst. The Solinst is measuring the temperature and the barometric pressure, and its installation is primarily aimed at evaluation of water level variations for the water balance estimations (Dhote et al., 2021). However, we did not exclude the mention of the Solinst sensor from the text, since it is required to explain how the elevations of the network components were calculated (Fig. 1c, Table 2). We also removed the min/max values for the series measured by iButton from Fig. 4.

ED: “Lines 205-206: "after applying" please reformulate.”

Answer. corrected.

ED: “Lines 223-226: some details on the despiking is needed. E.g., how spikes are defined (did you use any threshold)? "the 30-minute values were obtained of the atmospheric fluxes", please reformulate.”

Answer. Corrected. The paragraph now reads: “In the third step, the spiks were removedafter applying the method by Vickers and Mahrt (1997), fixing the threshold window of ±3.5 standard deviation for horizontal wind speed, CO_2 and H_2O and ±5.0 for vertical wind speed. This procedure was repeated up to 20 times or until no more spikes were found. Finally, we obtained, among others, the 30-minute fluxes of momentum, sensible heat and latent heat (evaporation), as well as the water vapor and carbon dioxide concentrations (see the Supplement).“

ED: Lines 228-229: the purpose and definition of the filtering steps are unclear. " with the median and median values" this is also unclear.

Answer. Corrected. The following text is added to the revised version: “We excluded 18 % of the total data from further consideration after the three step filtering. To fill these gaps we replaced the excluded values by the mean value, which was estimated from the time series of 30-minute values. “

ED: “Lines 238-240: here the Authors repeat the same information provided at lines 228-229. I would keep this information either before or here. In any case, the Authors should clarify the procedure, as it is unclear in both cases.”

Answer. Corrected.

ED: “Lines 243, 249, 266, and 289: evaporation is defined in kg/m2s, then in kg/s, and finally in mm/day. Please be consistent, I suggest providing it always in the same unit of measure (e.g., mm/day). The units of measure of the terms in eqs (1)-(6) should be provided in the text.”

Answer. We clarified that the equation of the bulk-aerodynamic method yields evaporation in kg/m2s, which was then converted to mm/day.

ED: “Line 259: for the sake of completeness, the definition of Monin-Obukhov length should be provided. It is important here to specify that stratification refers to the atmosphere.”

Answer. We added the definition (equation) for the Obukhov length, and the following text: “The Obukhov length (Obukhov, 1946) is the key element of the Monin-Obukhov similarity theory (Monin and Obukhov, 1954; Foken, 2006), and needed to adjust the bulk transfer coefficients to the actual stratification in the atmospheric surface layer.”

ED: “Line 269: compute-->computes”

Answer. Corrected.

ED: “Lines 312-315: this paragraph is disconnected from the rest. I suggest moving it before, when introducing the winds. In any case, it should be expanded.”

Answer. We moved the paragraph to the end of section 2.

ED: “Table 5: The acronym SSC should be introduced in the methods. In the methods you indicated the Nash-Sutcliffe Index with NSS but in Table 5 with NSI. Values of NSS<0 indicate that the model is worse than taking the mean, thus any model with NSS<0 should be avoided. So, saying that the bulk-aerodynamic model is better than the others because NSS is larger (but anyway<0) sounds a bit strange to me. Do you need to show NSS and SSC? Perhaps, introducing other metrics as bias, max error etc is more useful here (?)”

Answer. The acronym SSC is now introduced in the subsection of methods. We excluded the Nash-Sutcliffe Index in the revised version of the manuscript, and instead, we introduced the root mean standard error as the scope.

ED: “Figure 5: what does R2 refer to? It should be explained in the caption.”

Answer: R^2 refers to the determination coefficient, and we added the explanation to the text after fig. 5. We also corrected the acronyms for the Pearson correlation coefficient used in Table 5.

ED: “Lines 348-349: if I am not wrong, the values of the correlation coefficient 0.12-0.34 are not consistent with those in Table 5.”

Answer: Corrected.

We have implemented as many suggestions as possible in this study, however there is still work to do, and we will do it in our next study.

with the best regards,
Elena Shevnina,
On behalf of authors

Dear Referee,

We thank you for the useful comments and suggestions, and we were following them during the revising of the manuscript. In the revised version, the following principal corrections were implemented: the description of the methods applied to estimate the evaporation over a lake was extended in the introduction; the manuscript has got a new structure, and the quality of the tables and figures were improved. The text of the revised manuscript has got a professional proofreading before the submission. We also revised the list of the references.

We further follow the comments given by the Editor and two referees and answer for them point-by-point.

Comments from Referee 1 (R1)

R1: "The revised version of the paper by Shevnina et al. shows a high degree of improvement. The authors responded to most of my comments and I see they seriously worked on the manuscript focus and structure. I believe that the paper can be now considered for publication on TC after some minor revisions. These are mostly related to the following three points:

1) Despite the overall consistency of the revised version, there are still some speculative sentences and unclear passages which should be fixed.

2) The authors note that the paper was revised by a professional English corrector, and I confirm the general improvement from this side as well. However, I must say there are still some grammar/language revisions to be done, which I annotated in the manuscript pdf.

3) I'm happy to see the authors welcomed my suggestion on deriving their own empirical formula, and I appreciate how they structured the paper accordingly and commented the results. However, I must say I disagree on the way they did that, as their parameters cannot be compared to any existing formulation. My suggestion was to tune their own parameters for a Dalton-type formula, so that there can be a consistent comparison of their regional parameters against the ones they already tested from the literature. I don't know if the authors already tried this way and. If they haven't done it yet, I strongly recommend to –at least- test a Dalton-shaped formula. To be clear, not (as they did):

$E = a + b1w2 + b2 (es-e2)$

but:

$E = a(1+bw2)(es-e2)$

If they have tried this way already, I ask to clarify why they chose to change the shape of the formula.”

Answer. We removed all parts related to the empirical relationship E = a + b1w2 + b2 (es−e2) from the revised text. Indeed, the linear relationship suggested to calculate the daily evaporation from the wind speed, humidity and temperature measurements differs from the form of the Dalton-type equations given in eqs. 4-6. However, this particular form is only one among formulas that may include a lake surface area or daily minimum and maximum air temperatures (Hojjati et al., 2020; Zhao et al., 2013).
We further added the following explanation to the Introduction: “The combination equation approach includes the elements of both energy balance and mass-transfer approaches in the estimation of evaporation. The Penman equation (Penman, 1948) is among the most famous presenting this approach, where evaporation is calculated from the simultaneous solution of diffusion equations for heat and water vapor, and the energy balance equation (Finch and Calver, 2008). A more general form of the combination equation is given by the Penman-Monteith equation (Monteith, 1965), which was developed to describe evaporation from plants (evapotranspiration). There are also a number of empirical formulas that need additional information on lake surface area, radiation, daily minimal and maximal air temperatures, etc. (Hojjati et al., 2020; Zhao et al., 2013) or require only the air temperature and relative humidity to be known (Konstantinov, 1968).”

We also discussed the fitting the empirical coefficients in the formula E = a(1+bw_2)(es−e2) in the text: “… Therefore, the relationship between evaporation and 2-meter wind speed and saturation deficit was approximated by the formula reading as $E = a + bw_2 (e_s - e_2)$, and it’s similar to the combination equations (given in form $E= a (1 + a b w_2)(e_s - e_2)$ in Table 3), where the saturation deficit $(e_s - e_2)$ is expressed in (kPa), and two empirical coefficients (*a* and *b*) were evaluated from the series of the evaporation (after the EC method) and the wind speed and air temperature observations done at Maitri site, which is nearest to Lake Zub/Priyadarshini. The daily series for the period lasting from 01.01.2018 to 07.02.2018 was used in the fitting procedure. Figure 6 shows the daily evaporation estimated by the EC method, by the bulk aerodynamic method and new combination equation with two empirical coefficients fitted from the observations.
…
The daily evaporation was estimated to be 3.3 +/-1.6 mm day^{-1} (where the numbers represent the mean and standard deviation, respectively) by the equation $E = -0.33 + 0.60w_2 (e_s - e_2)$; and sum of the evaporation for the period 38 days by this method differs for less than 10 % from those estimated by the EC method. It is the lowest difference for the indirect methods considered; the Peason correlation coefficient and the mean root square standard error are estimated to be 0.59 and 1.0, respectively. These scopes allow us to consider the equation $E = -0.33 + 0.60w_2 (e_s - e_2)$ the second best among the indirect methods (Table 3), the only bulk aerodynamic method showing the better scope. However, these estimates are done on the similar data as the empirical coefficients were fit, and the independent data are needed.
The efficiency of the empirical formula $E = -0.33 + 0.60w_2 (e_s - e_2)$ with the independent data was estimated from the wind speed and air temperature measured at Irgason site (Fig. 1 c). We also used the lake water surface temperature measured at iBuntton site for the period of 27.01.2018 – 07.02.2018 (or 12 days); the daily series of the evaporation were calculated with this formula and then they were compared with those estimated after the EC method. The Peason correlation coefficient and the mean root square standard error are estimated to be 0.68 and 1.3, respectively. The sum of the evaporation for the period 12 days by this method is over 30 % higher

than those estimated by the EC method. ”

R1: “Below my detailed comments:

Abstract: the abstract is now more focused on the main findings of the manuscript.
L23-25: In the authors’ response they replied to my comment on ERA5 estimates in this way: "We are not analyzing the EC (or other estimates of evaporation) against the evaporation evaluated from the ERA5 data since they represent different spatial scales and thus we believe that comparing doesn't make sense." I agree and I appreciated how the authors integrated the ERA5 paragraph in the discussion. However, this makes the entire discussion on ERA5 data not a result anymore, hence it shouldn't be reported in the abstract. The authors should remove these lines and add a generic conclusive statement on what is novel from this paper and how it relates to the existing estimates (just one short sentence synthetizing your discussion section).”

Answer. We agree to remove the mention of ERA5 from the abstract. Now, the text reads 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, 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 (EC) method, and estimated on the basis of the bulk-aerodynamic method and four combination equations. We used meteorological and hydrological measurements collected during a field experiment carried out in 2018. The EC 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 estimated to be 3.0 mm day^{-1} in January 2018. 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}, which is over 32 % less than the results based on the EC method. This method is much better in producing the day-to-day variations in evaporation compared to the combination equations, which underestimated the evaporation over the lake open water table by over 40–72 %. We also suggested a new combination equation to evaluate the summertime evaporation of Lake Zub/Priyadarshini 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 EC method. ”

R1: “L56-57: “This study suggested… Antarctica” : this sentence is not very clear (English should be revised), but however it sounds as a conclusive statement which is not appropriate in the introduction. I suggest rephrasing or removing it.”

Answer. Removed.

R1: “L60-61: “The methods… Guidelines 2008)” What do the authors mean with methods that are narrow or pointed measurements? Please revise the English of the entire sentence.”

Answer. We changed the reference to more appropriate (Finch and Calver, 2008) and corrected the text.

R1: “L63-65: "profile measurements" and "measurements at various heights" sound like synonyms. What do the authors mean by differentiating the two terms? Please clarify.”

Answer. We removed this part of the text.

L66-69: Please rephrase this paragraph. I think I got what the authors meant but it's really badly written. EC is the key part of this manuscript and it is not well described. I don’t mean the authors should report all formulas (they already refer to Potes et al 2017 and that’s enough), but they definitely should describe in a clean way what EC measurements are (high frequency atmospheric variables and water vapor density) and what they are used for (computing heat fluxes at the air-water interface) based on what assumptions (. I could not find a clear sentence like that in the entire manuscript.
“low”: what do the authors mean with "low"? "lower" maybe? I don't see the point anyway, the height of the EC station is not something you "assume"...

Answer. We removed this part of the text.

L70: “The Dalton type semi-empirical equations allowed us to calculate the evaporation”: please make this sentence more general, we are still in the introduction! e.g. The Dalton type equations allow calculating evaporation from … at monitoring sites….

Answer. This part of the text was removed.

L76: “evaporation over the lakes”: I still don’t get why the authors refer to “lakes” when they evaluated evaporation from one lake only, Lake Zub/Priyadarshini.

Answer. Corrected.

R1: “L96 (as well as…) what do the authors mean with “temporal” observation network? Maybe they mean “temporary”?”

Answer. Removed.

R1: “Table1: please make the table more consistent and report all measurements as min/mean/max .”

Answer. Implemented.

R1:”L129 and comment to figure 2: I see the authors improved the comment to the figure but still they forgot to clarify what are the colors in their figure. Please state this clearly e.g. in L129 eight ranges of WHAT? Wind speed anomalies if I understood correctly) and add “wind speed anomaly” in the legend as well as in the figure caption.”

Answer. Yes, the same comment is given by the Editor. We improved the text as follows: “ We calculated the number of cases when wind was blowing from 36 sectors each 10 degrees wide, and then defined the prevailing wind directions (marked with the black arrows in Fig. 2). The prevailing wind directions range from 110 to 140°. We also calculated the wind speed anomalies for each 10- degree sector, given in color codes in Fig. 2. Positive wind speed anomalies were often observed within the range of the prevailing wind directions (marked with orange, yellow, red, brown and black in the legend of Fig. 2). Therefore one can expect the majority of strong winds from these directions." We improved Fig. 2 by adding the units to the x-axis, by implementing uniform labeling to the y-axis, by showing the prevailing wind direction ranges, and by reducing the number of subfigures.

R1: "L123: "to aim the water vapor sensor": what do the authors mean? Please revise the English here."

Answer. The sensors of the EC station should be aimed/directed to the specific wind direction, which is the optimal in order to collect much data from the area of interest (the lake surface). We used the verb "to aim" which is replaced by the verb "to direct". This paragraph was reformulated in the revised version of the text.

R1: "L135: Do the authors have any supporting data to contradict the previous study by Phartiyal et al 2011? Saying that "melting is a major inflow on the lake's water budget" without data or a reference is just speculation."

Answer. Indeed, the contradiction required the reference to Gopinath et al., 2020. The authors collected the water samples from 12 lakes (including Lake Zub/Priyadarshini) in Schirmacher Oasis during austral summer (17 February – 24 February 2010). The samples were analyzed with the method of isotopes in order to recognize the major sources of water in the lakes. Isotopic concentration shows that water in Lake Zub/Priyadarshini mostly comes from the adjacent glaciers. For the landlocked lakes, the major source of water is melting the seasonal snow cover (Holgson, 2012). Dhote et al. (2021) show that the inflow/outflow streams transit a substantial amount of water during the summertime, and we doubt that such amount of water can be formed only by melting of snow cover over the lake catchment.

We further add the following explanation to the revised text: "Gopinath et al. (2020) used water samples collected from 12 lakes (including Lake Zub/Priyadarshini) located in the Schirmacher Oasis to recognize major sources of water in the lakes. The samples were analysed with the isotope method (Ellehoj et al., 2013), and the isotopic concentrations show that Lake Zub/Priyadarshini is mostly sourced by the melting of the adjacent glaciers. For landlocked lakes, the major source of water is melting seasonal snow cover (Shevnina and Kourzeneva, 2017; Hodgson, 2012). It allows us to suppose that Lake Zub/Priyadarshini is the glacial type, as it is not the landlocked type as given in Phartiyal et al. (2011). Lake Zub/Priyadarshini is the lowest in the chain of the glacial lakes sourced by the ice/snow melting in the lowermost zone of the glaciers, and we estimated that more than 60 % of its catchment area is covered by rocks. This allows for the specific thermal regime and water balance of this glacial lake, which is among the warmest in the oasis: its water temperature rises up to 8 – 10 °C in January (Ingole and Parulekar, 1990). Such water temperatures are typical for the landlocked lakes (Simonov, 1971)."

R1: "L183-185: I see the authors replied to my question and I'm sorry for such a misadventure, but

I think this is not relevant in a scientific publication. I suggest the authors to simply remove these Solinst data if they are not what they wanted? I don't see the point of keeping them in the paper if they are considered inadequate for their study and in the end they don't even use them."

Answer. We corrected the text. The Solinst is measuring the temperature and the barometric pressure, and it allows evaluation of the water level at the lake. The installation of the Solinst is primarily aimed at evaluation of water level variations for the water balance estimations (Dhote et al., 2021). In fact, it would be better not to mention the water temperature measurements gathered by Solinst in this study connected to the evaporation. We do not exclude the mention of the Solinst sensor from the text (and Fig. 1 c) since its elevation is required to explain how the elevation of the network components were calculated.

R1: "L187-189: Please include the comment on the role of strong katabatic winds allowing mechanical mixing of water, as commented in the response to the review, IF supported by Shevnina and Kourzeneva 2017."

Answer. We removed the speculation on the role of the katabatic winds. We modified the text as follows: "To allow the estimation of evaporation by the combination equations, measurements of the water temperature are needed. We measured the water temperature of the lake's surface with two sensors: 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 resolution (Table 2). The lake surface temperature was measured every 10 minutes, and we calculated the daily average series of the water temperature in the lake. The Hobo consists of two sensors measuring temperature and barometric pressure allowing us to evaluate the water level/stage, and we used only the temperature measurements in this study. Sinha and Chatterjee (2000) reported that Lake Zub/Priyadarshini was thermally homogeneous down to the bottom almost from mid-January 1996 to mid-February 1997, and we assumed that the lake had no thermal stratification during the whole field experiment in 2018. "

Figure4 caption: Solinst sensor is at 3.9m depth. The lake is 6 m deep. These are not surface water temperatures at all! I strongly recommend the authors to remove these data from the manuscript. "The red dotted…" please anticipate this before (b).

Answer. We agree, these data were removed from Fig. 4. We further corrected the text and table correspondly. The mention of the Solinst sensor was only in the explanation of how the elevations of the sensors in the network were estimated.
The text was revised as follows: "Figure 4 shows the daily time series of the lake water temperature, air temperature and wind speed calculated from the measurements done by the sensors during the period of the experiment. The correlation coefficient equals to 0.89 for the series of the water temperature measured by two temperature sensors (Hobo and iButton). We

further used the measurements collected by the Hobo temperature sensor to estimate the evaporation over Lake Zub/Priyadarshini in January 2018."

R1: "L207: "2 meters over ground (the lake water table). Please see my comment above on the use of "lake water table":. However, are the authors saying that 2m over the ground coincide with the water table? Are the authors referring to sensors in the air (as I suppose) or in the ground? Please rephrase this sentence and clarify the experimental setup."

Answer. We revised 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, and the meteorological sensors are deployed at the different elevations (Table 2). The elevation 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). We did not use any height correction for the data on the relative humidity and air temperature since their changes with elevation are negligible in our case (Tomasi et al., 2004)."

We also add the following details to clarify the setup: "The elevation of the lake water level was measured by the geodetic instrument Leica CS10 during the installation of the Solinst water level logger on 30 December 2017 (Fig. 1 c). The same instrument was used to measure the elevation of the Maitri site in January 2018 (Dhote et al., 2021). The elevation of the lake water level was 122.3 meter (WGS84 ellipsoid vertical datum), we further used this elevation while calculating the elevation for the Hobo, iButton and Irgason temperature sensors."

R1: "L208-209: "It requires…equations." revise English."

Answer. Corrected.

R1: "L211: Why do the authors think that the "The Swiss Wind Power Data Website" is a correct reference for the roughness length classes? I mean, I see this website provides reasonable numbers, but can't the authors find a scientific reference? This is a scientific publication, not a student's thesis…."

Answer. Replaced by a reference to the textbook by Stull (2017).

R1: "L218-219: Please clarify that you followed Potes et al.2017 not only to fix and filter the data but most importantly to obtain turbulent atmospheric fluxes from the covariances between fluctuations of wind temperature and H2O measured by your station."

Answer. We removed this sentence. The text is now included the description of the steps, which were applied to process the data: "The Irgason raw data were measured with a frequency of 10 Hz, which were further analysed in the following steps: in the first step, bad data with less than 50 % of total 10 Hz measurements were excluded. In the second step, we excluded all data automatically flagged for low quality, and the data with a gas signal strength less than 0.7 (or 70 %

of the strength of a perfect signal). The gas signal strength is usually lower than 0.7 during rainfalls, which were not observed in January, 2018 in the Schirmacher oasis. Generally, rainfalls are rare along the East Antarctic coast where rainfall occurs 22 days per year at most (Vignon et al., 2021). In the third step, the spiks were removedafter applying the method by Vickers and Mahrt (1997), fixing the threshold window of 3.5 standard deviation for horizontal wind speed, CO_2 and H_2O and 5.0 for vertical wind speed. This procedure was repeated up to 20 times or until no more spikes were found. Finally, we obtained, among others, the 30-minute fluxes of momentum, sensible heat and latent heat (evaporation), as well as the water vapor and carbon dioxide concentrations (see the Supplement).”

R1: L222-223: “rain is not observed in Antarctica”: table 1 does not say the same.

Answer. Table 1 gives the data on the precipitation which generally occurs in the form of snow (solid phase) and rain (liquid phase). We further modified the text as follows: “The gas signal strength is usually lower than 0.7 during rainfalls, which were not observed in January, 2018 in the Schirmacher oasis. Generally, rainfalls are rare along the East Antarctic coast where rainfall occurs 22 days per year at most (Vignon et al., 2021). ”

R1: L301: NSS or NSI? Please chose one (best NSI or NSC -coefficient) and keep it.

Answer. We unified the acronyms in the text.

R1: L305: and negative NSI means…?

Answer. We replaced the Nash-Sutcliffe efficiency index with the estimations of the biase, and made the changes on the text accordingly.

R1: L312-314: please move this paragraph in a more appropriate section (e.g. 3.1 where Figure 3 is presented).

Answer. Corrected.

R1: L348: the authors should comment why NSI is negative. The fact that the bulk aerodynamic method shows such a good R and a negative NSI means that the model produces an evaporation that is well correlated with the measured one (good R), but it fails to reproduce the mean (negative NSI). This is also clear from your figure 6 where a bias is displayed between the two methods. Please comment your results, numbers and tables are not enough.

Answer. This criterion is now excluded in the revised version of the manuscript. We further used the root square standard error instead of NSI. We also separate the description of the measures of uncertainties in the separate subsection in the revised manuscript.

R1: “L358: Section name: all models tested so far were empirical, please rename the section as "Regional empirical relationship"

Answer. We removed this section.

R1: “L361-362: please revise. My suggestion: "based on the daily evaporation estimate from the direct EC method".”

Answer. Corrected.

R1: “L363: see my point 3 in the comments to the authors. Why didn't the authors choose a formulation similar to the Bulk aerodynamic or Dalton laws? In this way it would have been easier to compare their own regional formula to those already applied. I'm not fully convinced by this empirical formulation as it assumes that evaporation linearly depends on wind, which is not supported by the consolidated literature. I think the authors would get way better fitting if they try tuning e.g. their own Dalton parameters and this would greatly improve the impact of this paper.

Answer. We now tested the empirical equation reading $E = a + bw_2 (e_s - e_2) = a (1 + a b w_2)(e_s - e_2)$, and its similar form as for the combination equations in Table 3. The empirical coefficients were evaluated from the series of the evaporation (after the EC method) and the wind speed and air temperature observations done at Maitri site, which is nearest to Lake Zub/Priyadarshini. The manuscript was modified as given previously (Main comments of the Referee 1: point 3).

We also add the following text to the section of discussions: … “The empirical coefficients for the combination equation were fitted from the series of the evaporation (by EC method) and the meteorological observations at the station nearest to the lake site. This combination equation can be potentially used in estimations of the evaporation over the ice-free glacial lakes located in Schirmacher oasis. However, in this study the estimations of the daily evaporation and efficiency indexes were performed on the same data for the experiment (lasting 38 days). Also, we estimated the efficiency using the independent data on the air temperature, wind speed and lake surface temperature. The estimations of efficiency indexes were also done with the full independent data including the evaporation estimated by the EC method, therefore we would not suggest applying these coefficients as the regional references without further analysis.”

We have implemented as many suggestions as possible in this study, however there is still work to do, and we will do it in our next study.

with the best regards,
Elena Shevnina,
On behalf of authors

Dear Referee,

We thank you for the useful comments and suggestions, and we were following them during the revising of the manuscript. In the revised version, the following principal corrections were implemented: the description of the methods applied to estimate the evaporation over a lake

was extended in the introduction; the manuscript has got a new structure, and the quality of the tables and figures were improved. The text of the revised manuscript has got a professional proofreading before the submission. We also revised the list of the references.

We further follow the comments given by the Editor and two referees and answer for them point-by-point.

Comments by Referee 2(R2)

R2: “ Table 1: ?/?/?”
Answer. Table 1 was changed.

R2: “L. 266 of the Tracked changes copy: source area. The footprint…”
Answer. Corrected.

R2: “L. 611 <using? applying?>”
Answer. Corrected.

We have implemented as many suggestions as possible in this study, however there is still work to do, and we will do it in our next study.

with the best regards,
Elena Shevnina,
On behalf of authors