Dear Editor,

We have received two comprehensive reviews where several issues were mentioned. We implemented most of the modifications suggested by the reviewers in the revised version of the manuscript and, in our opinion, the manuscript profited with this revision. We also extended explanations of those issues, which were not concretely implemented in the revised manuscript. We followed the comments of two Referees in answering them point-by-point, and then we showed the new text to be included into the revised manuscript.

During the major revision, the manuscript has got a new abstract and two new subsections. The first new subsection is given the fitting of the empirical relationship to estimate the evaporation over ice free glacial lakes in Antarctica. The second subsection analyzes the impact of katabatic and synoptic forcing on day-by-day changes of evaporation. The revised version of the manuscript has a new structure with respect to representation of the material, and the quality of the figures was improved, and the list of the references was revised. We hope that it helps to stress the aim and objectives of our study. The new manuscript is checked by a professional English corrector, therefore we hope that the new text includes less language related issues.

Elena Shevnina,
On behalf of authors

The changes implemented in the revised version of the manuscript are given further in our answers to the comments by referees.

Answers to the comment on tc-2021-218

given by Anonymous Referee #1

Referee comment on "Evaporation over glacial lakes in Antarctica" by Elena Shevnina et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-218-RC1, 2021

General comment:

Anonymous Referee #1 has written a comprehensive review where many issues are mentioned, therefore we would like to thank the valuable comments that greatly contributed to the improvement of the manuscript. Some issues require serious reconsideration of the manuscript structure, and some of them need only additional explanations. In the revising manuscript we implemented as much as it is possible from this review and extended discussion on those issues which we believe that should be held. The revised version of the manuscript was checked by a professional English corrector, therefore we hope that the new text includes less language related issues.

We were following the detailed comments given by the referee while revising the manuscript.

Detailed comments:

L14-17: why reporting only numbers from January 2018 and only from EC and the bulk aerodynamic methods? Please revise these lines with meaningful results.

We agreed that the abstract should also include estimations from other methods. We changes the abstract as following:

"   The study provides estimates of summertime evaporation over ice free surface of Lake Zub/Priyadarshini located in the Schirmacher oasis, Dronning Maud Land, East Antarctica. Lake Zub/Priyadarshini is the second largest lake in the oasis, 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 open water table of the lake was estimated after the eddy covariance (EC) method, the bulk aerodynamic method and Dalton type empirical equations. We used special meteorological and hydrological measurements collected during the field experiment carried out in 2018 in addition to the standard observations at the nearest meteorological site. The EC method was considered as the most accurate given a reference for other estimates of evaporation over the lake water surface. We estimated the evaporation over the ice free lake surface as 114 mm in the period from 1 January to 7 February 2018 (38 days) after the direct EC method. The average daily evaporation is estimated to be 3.0 mm day^{-1} in January 2018. The largest changes in the daily evaporation were driven by the 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 over 30 % less than the EC method. This method is much better in producing the day-to-day variations in evaporation compared to the Dalton type semi-empirical equations, which underestimate the evaporation over the lake open water table for over 40–72 %. We also suggested a linear empirical relationship to evaluate the summertime evaporation of Lake Zub/Priyadarshini from the observations at the nearest meteorological site and surface water temperature. After this method, the evaporation over the period of the experiment is 120 mm, and it is only 5 % larger than the result according to the EC method. We also estimated the daily evaporation from the ERA reanalysis, which suggested the average daily evaporation during austral summer (December – February) 2017–2018 to be 0.6 mm day^{-1}. It is only one fifth of the evaporation estimated with the direct EC method."

L18-19 precipitation is not analyzed in the paper, nor the other terms of the water balance, thus this sentence is not supported by the results. Another option is to include the water balance in the manuscript, since the authors say that they have measurements of water level and discharges. This would improve a lot the impact of the *manuscript.*

We agree to remove the sentence about the precipitation from the text. The scope of this manuscript is evaporation, and it does not provide detailed analysis of the precipitation and other terms of the water balance equation. Such analysis is presented in another manuscript: “Meteorological parameters and water balance components of Priyadarshini Lake at the Schirmacher Oasis, East Antarctica” and P.R. Dhote, P. K. Thakur and E. Shevnina are among others co-authors of the manuscript. The manuscript is now published, and it is included to the list of references. This study addresses estimation of the uncertainties inherent in the indirect methods applying to evaluate the evaporation over the lakes in Antarctica.

L19-20: anticipate that the ERA5 data were also analyzed in comparison with EC and other estimates (e.g. L11), now it’s only mentioned at the very end of the abstract.

We compared the efficiency of the indirect methods (EC, bulk-aerodynamic, semi-empirical equations) applied to evaluate the evaporation with 3 well known indexes (Table 5). It actually means that we analyzed the estimates of the methods against each other. 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.

L20: “clearly demonstrated the need to add glacial lakes in the surface scheme of ERA5”: I don’t see this clear need, the authors only quantified the differences but did not evaluate how this underestimation affects the overall water balance of Antarctica regions. Either improve this aspect in the manuscript or smooth the sentence.

Yes, we agree. The sentence is now removed.

L57: Reference Guide, 2008, as well as Guidelines 1969 and others are guidelines for various topics and are in Russian only. I also note that 30 references in this paper are in Russian only. In this regard I suggest the authors to limit the non-english references to those cases where international literature is really lacking, otherwise it will be very difficult for the international readers to fully benefit from this paper.

The list includes 92 references: 75 in English (81 % of total amount), 16 in Russian and 1 in Portuguese. We selected those sources which are needed to show what is already known on the glacial lakes located in the Schimacher oasis (East Antarctica), their thermal regime and water balance. The Schirmacher oasis has been discovered by Russians since the late 1960s, and it is natural that many references are given in Russian **only** in this regional study. Therefore, we would refrain from removing those references from the list. Nevertheless, we further excluded the references given in Russian from the list because they duplicated each other or only given the Russian translation of the English version or duplicated the information given in other references in Russian. These are:

Brutsaert, W: Evaporation into the Atmosphere: theory, history and applications, Leningrad, Gidrometeoizdat, p. 53-60, 1985. (in Russian)

Burba, G., Y. Kurbatova, O. Kuricheva, V. Avilov, and V. Mamkin: Handbook for the Method of Turbulent Pulsations. LI-COR Biosciences, IPEE, Russian National Academy of Sciences, Moscow, Russia, 223 pp. ISBN: 978-0-692-57831-5, 2016. (In Russian)

Guidelines to calculate evaporation from water reservoirs. Leningrad, Gidrometeoizdat, 1969, 83 pp. (in Russian)

Report of 31 Soviet Antarctic Expedition: Novolazarevskaya scientific station, 186 pp. 1986 (in Russian).

In the revised version of the manuscript over 89 % of references are in English.

L62-64: Please make a selection of the most important references or provide a comment to them, in order to *discriminate among the 7 listed works.*

Yes, we agree with the reviewer and the list was limited to 3 works: “Stannard and Rosenberry, 1991; Blanken et al., 2000; Aubinet et al., 2012”

L74-75: “which results in errors…”: this sentence is too strong if no demonstration is provided. What about substituting “errors” with “inaccuracies” and smooth the concept? Has this ever been demonstrated? If yes, provide a reference. If this is one of the aims of the paper, then clearly anticipate this. Are these inaccuracies expected only in summertime? Which is the order of magnitude of these inaccuracies? To what extent does this underestimation affect the overall water balance of the two polar regions?

We agreed that the sentence is too strong and it needs to be smoothed. To our knowledge, the range of “inaccuracies” may be connected to seasonally-existing glacial lakes that have not been estimated yet. In this study, we do not evaluate the “inaccuracy” of the ERA5 in estimation of the evaporation because of the glacial lakes. It would need a separate study. Further, we moved all material connected to the impact of glacial lakes to the land surface - atmosphere exchange to the section of the discussion.

“Seasonal presence of the liquid water (ie. in lakes and iced “swamps”) over the ice/snow covered land surface affects the surface-atmosphere moisture exchange. A proper description of the land cover is a crucial element of numerical weather predictions (NWPs) and climate models, where the overall characteristics of the land cover are represented by surfaces covered by ground, whether vegetation, urban infrastructure, water (including lakes), bare soil or other. Then, various parameterization schemes (models) are applied to describe the surface-atmosphere exchange (Viterbo, 2002). Lakes have been recently included in the surface schemes of many NWPs (Salgado and Le Moinge, 2010; Dutra et. al., 2010) with known external parameters (location, mean depth) available from the Global Lake Database, GLDB (Kourzeneva, 2010;Kourzeneva et al., 2012). The information on only a few glacial lakes is included in the newest GLDBv3 version, and not any lakes found in Antarctica (Toptunova et al., 2019). Over 65 thousand glacial lakes have been detected over the East Antarctic coast via satellite remote sensing in austral summer 2017, and most of them have spread over the ice shelf (Stokes et al., 2019). For example, the total area of the glacial lakes in vicinity of the Schirmacher oasis was over 72 km^2 in January 2017 (Fig. 7), two largest glacial lakes are of the similar size as the Schirmacher oasis itself. Such amount of the liquid water may contribute to the additional source of the uncertainties inherent in the estimations of the regional evaporation after the NWPs. Estimates on evaporation are also available from climate and NWP models and atmospheric reanalyses. The most recent global atmospheric reanalysis is ERA5 of the European Centre for Medium-Range Weather Forecasts (Copernicus Climate Change Service, https://climate.copernicus.eu/, last access 09.07.2021; Hersbach et al., 2020). As other reanalyses, ERA5 does not assimilate any evaporation observations, but evaporation is based on 12 h accumulated NWP forecasts applying the bulk aerodynamic method. The results naturally depend on the presentation of the Earth surface in ERA5, and in the Dronning Maud Land, the surface type is ice and snow with no lake. Therefore the estimate of the evaporation does not include evaporation from liquid water surface. We estimated the daily evaporation also from the ERA5, and the results suggest that the evaporation during summer (DJF) 2017–2018 was 0.6 mm day^{-1}. It is only one fifth of the evaporation estimated with the direct EC method.”

We also added a new figure to illustrate the text above.

L85-86: please revise the English language here.

Yes, the text was changed as follows: “The oasis is the ice free area elongated in a narrow strip around 17 km long and 3 km wide in West–Northwest to East–North-East, and its total area is 21 km^2 (Konovalov, 1962).”

Figure1: I don't understand the figure. Why is that blue square highlighted in subplot (b)? And where is Lake Zub/Priyadarshini here? Please consider skipping one of the subplots (e.g. b) and include the shape of the lake

where measurements were taken.

We modified Fig. 1: the subplot (b) is now moved to Fig. 7; and we add the subplot (c) showing the temporal observational network in the catchment of Lake Zub/Priyadarshini.

Figure 2: please explain the figure better and clarify the color code.

Yes, we extend the explanation of the results given in Fig. 2 in the text: "To plan the field experiment we used 6 hour synoptic observations at the Novo site available from the British Antarctic Survey Dataset (https://www.bas.ac.uk, last access 14.12.2018) covering the period 1998–2016 to calculated the wind direction and frequency of wind speed anomalies over the multi-year means for eight ranges (Fig. 2). The positive anomalies in the wind speed suggest that the observed wind speed is higher than the mean value. In Fig. 2, the prevailing wind direction is ranged from 120 to 140º, and the positive wind speed anomalies are typical for this range. We accounted for these circumstances in choosing the location to deploy the EC measuring systems, to aim the Irgason sensor, and to design its maintenance system to sustain the local winds."

Figure 3 and commenting text: please report the reference heights in the two stations for temperature, relative humidity and wind sensors and clarify if data have been manipulated for comparison e.g. transformations due to different heights. Is solar radiation only available at Novo site (subplot d)? Please clarify in text (see also comment on L187-189).

We added a new table with the information on the elevation (or the reference height?) of the temperature, relative humidity and wind sensors installed at Irgason and Maitri sites given in Table 2. We also provided the temporal resolution of the data available in the supplemental dataset. We further explain our transformations of the data in the text: "In our calculations with the Dalton-type equations we applied the data measured by the meteorological sensors installed at the Maitri and at the Irgason sites. These sensors are located at different elevations (heights): the Irgason sensor is at the height of 2 m over the lake water table/stage, and the humidity sensor is mounted on the mast of the Maitri station at the height of 6 m. The difference in heights above the ground for these sensors is over 20 m (Table 2). It requires applying the transformation to the wind speed measured at the Maitri site before using these measurements in the Dalton-type equations. Further, we used the logarithmic approximation of the wind profile to correct the wind speed data measured at the Maitri site, where the roughness length constant equaling to 0.0024 m (as suggested: https://wind-data.ch/tools/profile.php?lng=en, last access 15.10.2021). We did not use any transformation for the data on the relative humidity and air temperature since their changes with elevation are negligible in our case (Tomasi et al., 2004)."

Yes, the solar radiation is only available at the Novo site. We did not use these data in further calculations, therefore we excluded this variable from Table 1.

L128-145: the entire section should be moved at the beginning of section 2. And what about the lake depth? Please also describe the small lake close to Lake Zub where the Hobo sensor is installed and explain why is it considered it a good proxy of the main lake water temperature.

We agree to move subsection 4.1 at the beginning of section 2. We add the explanation on why the Hobo sensor is installed in the neighbouring lake. The following text provides the explanation: "The Hobo sensor was deployed in the mouth of the stream inletting the lake neighbouring to Lake Zub/Priyadarshini. This stream originated from Lake Zub/Priyadarshini, and it is over a couple of meters in length. There is not a big difference in the measurements collected by the Hobo and iButton (Fig. 4). However, the measurements by the Hobo sensor are of better accuracy than those by the iButtom sensor, and the Hobo's observations cover the period longer than those by the iButton. Therefore, we further used the Hobo measurements of the water temperature in calculating the daily evaporation applying the indirect methods."

Figure 4: I can't see snowpacks in subplot (a), is the legend correct?

We also improve Fig. 4 (which is now Fig. 3).

Table 2: Please add a column specifying the reference height (for weather stations) and depth (for water temperature sensors) and the time resolution of the measured variables.

Yes, we add the elevation (the reference height ?) of the sensors in Table 2.

L187-189: are wind, air temperature and rh in figure 5 plotted as they were measured without any transformation? Are all the variables at Maitri measured at 6 m a.g.l.? If yes, then these variables should be reported at the same height as Irgason station for a fair comparison (the same applies to Novo station).

In our calculations after the Dalton-type equations we applied the data collected by the meteorological sensors installed both at Maitri and Irgason sites. The meteorological sensors are installed at different heights: the Irgason's sensors are deployed at the height of 2 meters over the ground (the lake water table), and the sensors at Maitri site are mounted on the mast at the height of 6 meters over the ground. It requires applying the transformation to the wind speed measured at the Maitri site before using these measurements in the Dalton-type equations. Further, we used the logarithmic approximation of the wind profile to correct the wind speed data measured at the Maitri site, where the roughness length constant equaling to 0.0024 meter (as suggested: https://wind-data.ch/tools/profile.php?lng=en, last access 15.10.2021). We did not use any transformation for the data on the relative humidity and air temperature since their changes with elevation are negligible in our case (Tomasi et al., 2004).

Reference: Tomasi et al.,Mean vertical profiles of temperature and absolute humidity from a 12-year radiosounding data set at Terra Nova Bay (Antarctica). Atmospheric Research, 71, 3, p. 139-169, doi: 10.1016/j.atmosres.2004.03.009, 2004..

L200-203: If I understood correctly, Solinst station is located at 3.9 m depth while the other two sensors are at 0.2 m below the surface. The authors ascribe the difference in water temperature to the effect of cold water inflow, but what about the thermal structure of the lake?

Yes, the thermal sensors Solinst, Hobo and iButonts were installed at different depths. In our opinion, it is the result of miss-understanding happening during the field experiment. Unfortunately, the location and type of the Solinst sensor will not agree well, and ES expects the temperature data measured by a chain of the sensors installed on the other side of the lake.

The text was modified accordingly: "The thermal sensors Solinst, Hobo and iButonts were installed at different depths, and it is the result of a misunderstanding between the hydrologists working in the field. We expect that the lake is measured by a chain of sensors, and it will be able to tell how the water temperature changes with the depth. I thought that this chain is deployed in the central part of the lake (between the Irgason and In2 in the Fig. 1c). We guess that Lake Priyadarshini is well mixed due to strong katabatic winds during the ice free period, and it is in accordance with the data shared by Sinha and Chatterjee (2000)."

Reference: Sinha R., Chatterjee A.: Thermal structure, sedimentology, and hydro-geochemsitry of Lake Priyadarshini, Schirmacher oasis, Antarctica. Sixteenth Indian Expedition to Antarctica, Scientific Report, Department of Ocean Development, Technical Publication No. 14, 36 pp., 2000.

This figure is given in Sinha and Chatterjee (2000), and it shows the temperature profiles measured in Lake Zub/Priyadarshini in January-February 1997.

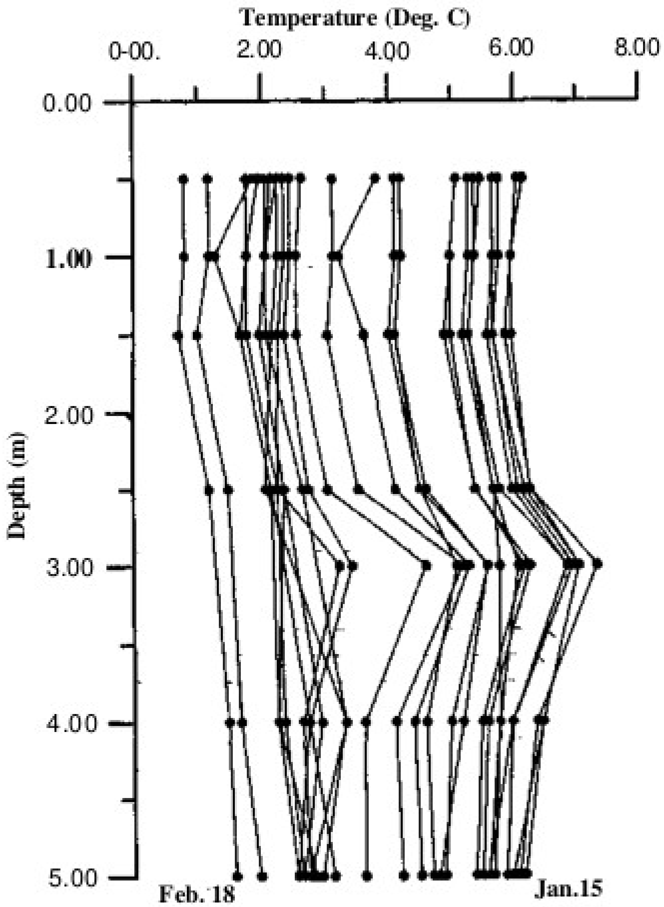

Fig. 2(b) Daily variation of temperature with depth from 15th Jan. to 18th Feb. 1997.

L208-209: is the 0.4°C difference due to the different accuracies? Hobo is located in the neighboring lake, how can the authors be sure that such difference is not due to this? What about the morphological characteristics of this second smaller lake? The authors say they use the "longest time series": is it the Hobo one? Please clarify and explain the decision.

The Hobo sensor was installed on the neighboring lake coast at a place located at the mouth of the incoming stream. We guess that its measurements are representative for the water level and water temperature in the stream more than for the neighboring lake itself. In the new text we extended the details on the lake morphology. We further used the data measured by the Hobo sensor, which gives the longest daily time series.

L237-239: the verb is missing here.

Instead of a missing verb, there was an extra "and" word. We have removed it so that the sentence reads: "For the turbulent transfer coefficient for moisture under neutral stratification (CEzN), we applied the value of 0.00107 based on measurements over a boreal lake (Heikinheimo et al., 1999; Venäläinen et al., 1998)."

L240: what do the authors mean with "taking into account the effects of thermal stratification"? Please mention which are the conditions of thermal stratification in their case study.

We cannot simply use the C_{EzN}, because the stratification is not always neutral. Hence, we need to take into account the effects of stratification on the turbulent transfer coefficient. The effects are presented via the ψ_m and ψ_q functions. We have clarified the text: "As the stratification is not always neutral, we took into account the effects of stratification on the turbulent transfer coefficient C_{Ez} as follows..."

L244: Please clarify what are ψ_m and ψ_q for.

As already written in the submitted manuscript (line 244), the ψ_m and ψ_q are empirical functions characterizing the effect of stratification on CEz. The forms of the functions are as follows but, as references are given, we do not see a strong reason to add the forms in the manuscript.

Businger et al. (1971):

$$\Psi_M = 2ln[\frac{1+\Phi_M^{-1}}{2}] + ln[\frac{1+\Phi_M^{-2}}{2}] - 2arctan\Phi_M^{-1} + \frac{\pi}{2}$$

where $\Phi_M = (1-\gamma_1\zeta)^{-1/4}$

and for $\Psi_H = \Psi_E = 2ln[\frac{1+\Phi_{HE}^{-1}}{2}]$, (A27)

where $\Phi_H \simeq \Phi_E = (1-\gamma_2\zeta)^{-1/2}$

Holtslag and de Bruin (1988):

$$\Psi_M = \Psi_H = \Psi_E = -a\zeta - b(\zeta - c/d)exp(-d\zeta) - bc/d \quad \text{(A26)}$$

where a = 0.7, b = 0.75, c = 5 and d = 0.35. The

L253: why e_{200} if it is at 2 m height? I suggest to change it to e_2 or e_z

We have followed the suggestion, and replaced e_{200} by e_2.

*L254: depend**ing***

Corrected.

L255- 256: The authors should explain why they chose these parameterizations among the several formulas available. Have they been adopted for similar climatic areas? This is important as the authors conclude that these models are inadequate in comparison with EC data.

We extended the explanation why these formulas were selected in this study: "In these equations, the empirical coefficients are evaluated from the data gathered in regions with different climates, and therefore they probably will not be applicable for lakes located in Antarctica. The approximations by Penman (1948) and Doorenbos and Pruitt (1975) are among the methods which are most often methods used in hydrological practice (Finch and Hall, 2001), therefore we have chosen them in this study. The method by Odrova (1979) is used to evaluate the daily evaporation over the lakes in Antarctica, however the scope of uncertainties of this method is not estimated previously (Shevnina and Kourzeneva, 2017)."

*L256: Eqs 4-**6** (correct 5).*
Corrected.

L264: were the Igarson data used at a daily basis as well? What about the time resolution of water temperature data? If water temperature is available at sub-daily resolution, the authors could evaluate to what extent the daily cycle of water temperature affects the daily evaporation, as well as they did with wind.
In this study, we focused on the estimations of the uncertainties in daily values of evaporation. We calculate the daily values for the evaporation also from the EC data, and then apply these values as reference for other indirect methods. Water temperature measurements are available every 10 min, we further these series to evaluate the daily average temperature. Our next study will extend the analysis of the sub-daily cycle of evaporation over the glacial lakes. We also plan to evaluate the uncertainties of the evaporation calculated after the surface energy budget method (ie. FLake model, Mironov et al., 2005). We further extracted any sub daily data from this manuscript.

L270: please either provide an English reference for the s-sigma criterion or explain the method.
We included the formula for the s-sigma criterion in the revised text of the manuscript.

L271-272: "therefore not….values" a verb is missing here.
Corrected.

L273-274: please specify that the paired tower method was applied to another case study. In this regard I was surprised the authors chose the Alqueva reservoir for estimating the uncertainties of the EC method to be applied in polar zones. The authors report in the Annex their intercalibration experiment in this large reservoir in Portugal, which is very different from their case study in Antarctica for dimensions, morphology and climatology. Can the authors comment on the reliability of their uncertainty estimations on such a different case study? Few words should be spent on this in the main text, when referring to the Annex.
In our paired-tower experiment we evaluated the uncertainties which are inherent of a particular instrument (namely Irgason with the serial number SN1243), which we used during our field experiment. Since the instrumental error does not depend on the region where the instrument will be used, it may be done either in Finland or elsewhere. We did not find the second Irgason in Finland, therefore we chose one identical instrument placed in Alqueva reservoir, Portugal. In this experiment we estimated only instrumental error, which is one of the contributors to the uncertainties inherent in the EC method (among others).
We modified the text as follows: "To quantify the random uncertainty of the eddy covariance method, three methods are usually applied (Aubinet et al., 2012). In this study, we applied the paired tower method to evaluate the uncertainties of the EC method taking advantage of an intercomparison campaign in Alqueva reservoir, Portugal, during the Autumn 2018. Since the instrumental error doesn´t depend on the region where the instrument will be used, it may be done elsewhere. The relative random instrumental error estimated in this intercomparison campaign was 7 % (see the Annex)."

L281 onwards: This paragraph on ERA5 comes out of the blue. Please consider having a dedicated paragraph for this or move it in a more appropriate part of the section.
We moved this paragraph in the special sub-section connected to ERA5 to the section with discussions: "
L295: Did the wind blow with those features for the all day? Subdaily variations should be mentioned as that the

daily evaporation estimated is strongly sensitive to wind.

Yes, the katabatic winds are consistent for this region. The in-day variations will not be present in the revised version of the manuscript.

L299: What about the lake thermal stratification? How deep is this lake? The authors only say it is "shallow" in the abstract.

Lake Zub/Priyadarshini has a maximum depth of over 6 meter. The lake stays thermally homogeneous during the ice-free period.

In the new text we extended the explanation as follows: " This study focuses on the estimation of evaporation on the ice-free surface of Lake Zub/Priyadarshini, which is the second largest water body of the Schirmacher oasis. The lake's surface area is 33.9 x10^3 m^2, its volume is over 10.0 x10^3 m^3, and maximal depth of the lake is 6 m (Khare et al., 2008). Lake Zub/Priyadarshini occupes a local depression, and is fed by two inflow streams present in warm seasons. The outflow from the lake occurs via a single stream. The lake stays free of ice for almost two summer months from mid-December to mid-February. In this period, the lake has no significant thermal stratification (Sinha and Chatterjee, 2000), and a possible reason for that is persistent katabatic winds mixing water down to the bottom.

… We assumed that Lake Priyadarshini is thermally homogeneous down to the bottom (Sinha and Chatterjee, 2000), and the possible reason for that is the strong katabatic winds allowing mechanical mixing of water. Thermal homogeneity is often for the lakes of similar morphology located in the Larsemann Hills oasis, East Antarctica (Shevnina and Kourzeneva, 2017)".

Section 4.2: this should be the most important section of the manuscript but is very confusing. Please refer immediately to the Table and clarify the text. Moreover, the authors only evaluate the effect of atmospheric data, but what about water temperature used?

We agreed that the section needed to be improved. In the revised version of the manuscript we change the presentation of the results. Now the section included three subsections: the first one is dedicated to the uncertainties inherent in various methods applied to calculate the evaporation over the lake. The second subsection is given the fitting of the empirical model to evaluate the summertime evaporation from the lake water temperature observations and measurements at the meteorological site nearest to the lake. The third subsection is presenting the impact of the katabatic and synoptic forcing in driving the evaporation over the ice free surface of lakes.

Did they try testing the other station?

In this study we did not test the meteorological data from the Novo station, however we plan to perform such analysis in the further study.

Did they use a mean daily value or an instantaneous value? Please clarify.

In this study we focused on the explanation of the day-by-day variations in the evaporation, and therefore we used the daily averaged values for all variables including the wind speed, lake water temperature, air saturation pressure. In our future study we plan to analyze also the in-day variation of the evaporation.

L313-314: "According to the EC method, the daily evaporation rate varied from 0.05 to 5.0 mm day-1 with ***the mean value equal to 3.0*** *± 1.1 mm day-1. Over the* ***period of 38 days****, it results in* ***114 mm*** *of water evaporating over the lake surface." It seems that the authors estimated 114 mm of water evaporated in their period of 38 days by multiplying* ***3 x 38****. Is it just a coincidence or they didn't report the real* *number from EC measurements? If the second, please provide the “real” evaporation by summing all 30-minutes EC estimates for the 38 days.*

We calculated the mean value of evaporation (3 mm per day) by dividing 114 mm a season (which is a sum of 30-min EC estimates) by a number of days (which is 38 in the period covered by the observations). We extended the explanation in the new text as follows: “The average was calculated dividing 114 mm of evaporated water (which is the sum of the 30-min series of evaporation) by a number of days (which is 38 in the period covered by the observations).”

L315: estimated with what meteo data and what water temperature? please clarify
We used two options: the meteorological data measured by the Irgason instrument and the sensors mounted at the mast at Maitri site (Table 2).

L317-318: “In case of using the meteorological observations at Irgason site, the average daily evaporation was 3.0 mm day-1,” please clarify if this number was estimated with Dalton (and which one of the three) or with the bulk aerodynamic method.
We already comment on it, answering the question L313-314."

L328-336: If glacial lakes contribute only to 4-5% of the total area of the grid cells, and only for two months in a year, can the authors estimate the order of magnitude of the model’s error? Can they say, based on their single case study, that ERA5 should implement glacial lakes in a new parametrization? Maybe the authors are right, but this has to be supported by more solid results, otherwise it’s just speculation. I see it is partially attempted in the Discussion section when the study of Nakka 2021 is mentioned, the authors could deepen the analysis there. In the sentence “However…, which total area … ERA5” please revise the English language.
We moved this discussion to the section of the discussion with the text mentioned above.

Figure 8: please add a regression line in black and in red with the R^2 coefficients.
Yes, we modified the plot accordingly. The scatterplots are given in Fig. 5.

L346: as above (L270) please explain the SSC criterion. The authors say they have a good fit with but NSI is always negative, also for the bulk aerodynamic method. Please comment on this.
The formula of the s-sigma criterion is now given in the sub-section describing the methods. We also extended the explanation why the NSI is negative, and why SSI is given a good (or even best) fit: “The values of the *NSS* can range from minus infinity to 1, and *NSS* = 1 indicates a perfect match of the data modeled after the indirect methods to the data after the EC method; and *NSS* = 0 indicates that the indirect methods are as accurate as the average of the EC data”.

L353: What about water temperature under wind gusts? ...

In this study, we calculated the series of the evaporation with the meteorological data averaged over a day, and therefore it is impossible to say something related to the role of wind gusts (which are computed in data measured in minutes).

… What about the filtering based on wind direction performed on EC data?

We explained the main stages of the post-processing procedure applying to the raw EC data. With a single particular study we are not able to provide much details on the transformations, which were applied to the raw data while calculating the evaporation after the EC method. Therefore, we refer to the detailed description of this procedure to Potes et al., 2017.

… Was it done also for winds measured at the two stations?

The wind direction filtering applied to the evaporation obtained with the EC data resulted in over 17 % of data discarded. These gaps in the wind direction were replaced with average values of the neighbour 30-min blocks to obtain the daily evaporation data series.

Discussion: The mentioned works are relevant to this study, but the authors should change the way they refer to previous works, otherwise this section will look like a review of 4 papers rather than a Discussion section. The authors should first comment the main results/limits of their analysis and then interpret the obtained results at the light of the referenced publications. If the authors are interested in quantifying the order of magnitude of the evaporation term in the water balance of their case study, this is the place to do that (much recommended). Then they can extend this to the more general framework of glacial lakes and refer to other works.

We changed the structure of this section. It now first commented the main results of the study, and then placed them into among other regional studies.

L399: How much do these 0.16-0.22 mm affect the overall water balance? Please comment on this.

We do not understand which water balance is mentioned? We further commented these values as follows: “Naakka et al. (2021) estimated evaporation over the Antarctic region from the ERA5 reanalysis for five domains, including the East Antarctic slope where the Schimacher oasis is located. There the average daily evaporation in summer is 0.3 mm day-1, and it is reasonable for the ice/snow covered surface. In summertime, the presence of the liquid water over ice/snow covered surface changes the fraction of the lakes over the East Antarctic slope, and it is 6–8 % of the region in the vicinity of the Schimacher oasis (Fig. 8). The increasing numbers of the glacial lakes over the surface of the East Antarctic slope affects the surface-atmosphere moisture interactions, and it also changes the regional evaporation not accounted for by the numerical weather prediction systems and climate models. We assumed that the 0.3 mm of ERA5 is a fair value for the ice sheet in the East Antarctic slope and that 3 mm is a representative value for the glacial lakes, and it may add up to 0.16–0.22 mm to the regional summertime evaporation over the margins of the East Antarctic slope. These numbers seem to be insignificant for the mass balance of the Antarctic ice sheet and shelf. However, we suggested more comprehensive research to better quantify the impact of glacial lakes to the surface-atmosphere moisture exchange in summer periods.”

L406-407: The tuning of Dalton parameters in glacial lakes is interesting and the authors should consider doing this for their case study, as they have the EC data which are fit for the purpose.

We agreed. The revised manuscript was extended with estimations of the empirical relationship between the evaporation and meteorological parameters measured at the site nearest to the lake. Our results are given in one table and one figure included in the revised version of the manuscript.

The following text explained the table and figure: “The empirical Dalton type equations are limited to be used in the region where the coefficients are obtained, and there are no any suggestions given for Antarctica (Finch and Hall, 2001). We further suggested the regional empirical relationship applying the data on the daily evaporation estimated after the direct EC method and observations collected at the meteorological site nearest to the lake. The evaporation (mm day^{-1}) was evaluated with the linear regression model $a + b_1 w_2 + b_2(e_s - e_2)$, e_s/e_2 is expressed in mbar. We applied these empirical models to approximate the relationship between the wind speed measured at 2 m height (w_2), $(e_s - e_2)$ and daily evaporation using several subsets. The empirical coefficients were evaluated based on the whole observations, for two periods.

To evaluate how well the empirical relationship simulated the observations, the verification with the independent observations is needed. In case of using whole observations in fitting of the empirical coefficients (subset 1), it was not possible to fully estimate the quality of the model since no independent measurements of evaporation were in rest. Therefore, the verification of the fitted regression models was performed only for the subsets 2 and 3 (Table 6) applying the cross validation procedure: the empirical coefficients were estimated with the data collected during the period of 01.01.2018 – 19.01.2018 (19 days), and this linear regression was applied to simulate the daily evaporation for the period of 20.01.2018 – 07.02.2018 (Fig. 7). Then, the procedure was repeated backward: the linear regression was evaluated from the data collected over the period of 20.01.2018 – 07.02.2018, and it was used to model the daily evaporation for the period of 01.01.2018 – 19.01.2018.

… Figure 7. The daily series of the evaporation evaluated after the direct EC method (black) and applying the linear regression with the empirical coefficients estimated from data collected during various periods: 01.01.2018 – 07.02.2018 (grey), 01.01.2018 – 19.01.2018 (blue) and 20.01.2018 – 07.02.2018 (red).

Table 6 shows the estimates of the empirical coefficients in the linear relationship $a + b_1 w_2 + b_2$, which were calculated applying three different subsets of data (Fig. 7). The parameter b_1 is very similar, being estimated from three different subsets. The estimates of two parameters (a and b_2) are also similar for the subsets 1 and 2. The estimates of the parameters varied substantially between the subset 3 and other two subsets. The value of the Pearson correlation coefficient is highest in subset 2, when the value of the residual standard error is minimal.

... Table 6. Estimates of the efficiency indexes (R^2, R) and empirical coefficients (a, b_1, b_2) in the linear regression model to evaluate the daily series of evaporation based on the observations at Maitri site.

Further, the daily series of evaporation were estimated with the empirical coefficients evaluated for the subset 2 (boulded in the Table 6) for the whole of the field experiment. The sum of evaporation over the period of 38 days is 120 mm, and it is 5 % larger than the sum estimated after the direct EC method. The daily evaporation varies from 1.7 to 5.1 mm day^{-1}, with the average taking 3.2 mm day^{-1} and the standard deviation 0.8 mm day^{-1}, and it is only slightly larger than for the EC method."

… This would be also interesting in order to understand why the other Dalton style formulas were providing consistent results.

We explained why these formulas were selected in the text: "In these equations, the empirical coefficients are evaluated from the data gathered in regions with different climates, and therefore they probably will not be applicable for lakes located in Antarctica. The approximations by Penman (1948) and Doorenbos and Pruitt (1975) are among the methods which are most often methods used in hydrological practice (Finch and Hall, 2001), therefore we have chosen them in this study. The method by Odrova (1979) is used to evaluate the daily evaporation over the lakes in Antarctica, however the scope of uncertainties of this method is not estimated previously (Shevnina and Kourzeneva, 2017)."

L412-417: The authors never mentioned precipitation in the whole paper. If they want to make volumetric considerations, they need volumetric estimations. How many m^3 of water are lost to evaporation during summer? How much is it gained with precipitation? What about inflow/outflow streams? If these numbers are not reported in the paper, this sentence is not supported.

As mentioned above the other terms of the lake water budget are given in the manuscript under the revision now. This study aims to evaluate the uncertainties (“inaccuracies”) of various indirect methods applied to evaluate the evaporation over the ice-free surface of the lake. We are not going to extend the manuscript by including the analysis of the water budget of the lake.

Elena Shevnina,

On behalf of authors

Answers to the comment on tc-2021-218

Anonymous Referee #2

Referee comment on "Evaporation over glacial lakes in Antarctica" by Elena Shevnina et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-218-RC2, 2021

General comment:

Anonymous Referee #2 noticed that the manuscript is based on “the absolutely unique database”. However, he/she suggested “a major refocusing of the paper that needs to be done in addition to correcting some aspects of the methodology and improving the analyses”. We implemented most relevant suggestions given by the Anonymous Referee #2 that greatly contributed to the improvement of the manuscript. We agree that the dataset allows us to explore several topics, nevertheless, in our study, we focused on the uncertainties inherent in various methods applied in the estimation of evaporation. We believe that this topic is only first in a series of the research based on this dataset. The manuscript allows partial publication of the hydrological data collected on the glacial lakes located in East Antarctica. Our next case study will be focused on Lake Glubokoe (the Schimacher oasis), and it will be based on the dataset collected during December, 2019 - February 2020.

Detailed comments:

Further, we follow the comments of the Referee in answering them point-by-point:

1) The paper needs to be refocused on its main theme (evaporation from a lake in Antarctica) and avoid repetition.

This study is among other papers focusing on the water budget and thermal regime of lakes located in Antarctica (Shevnina and Kourzeneva, 2017; Shevina et al., 2018; Shevnina et al., 2021). This study is particularly aimed to understand which method is the best in estimation of one of the terms in the water budget of the lake (evaporation). It is a reason why we did not pay much attention to the evaporation itself particularly in the Introduction.

Introduction: too much emphasis on glacial lakes, but not enough on evaporation itself. What is known about evaporation from water bodies at very high latitudes?

We revised the Introduction and now less attention is paid to the glacial lakes themself. We extended the description of the methods applied to evaluate the evaporation, and the case studies direct to evaluation of the evaporation of lakes located in very high-latitudes. We did not find many of them, and they are described in the Discussion in order to place our results into the framework of what is already known in this topic.

ERA5: I understand that ERA5 is not a good tool for estimating lake evaporation in Antarctica (not a huge surprise, let's be honest), but I think it is given too much emphasis in the paper.

To be honest, we have never considered the ERA5 as “a tool for estimating lake evaporation” because of the difference in “a spatial footprint”. Including the ERA5 data only shows the magnitude of difference between the evaporation over the liquid water (which is a lot stored in the glacial lakes spread over the continental ice-sheet and shelf) and snow/ice covered surface (as it is supposed in ERA5 within this region). In the revised version of the manuscript, we shortened the discussion on the ERA5 data in whole text, and finally placed them in the section of the Discussion. We also add the new figure allowing us to extend the explanation.

The following text was included: “Seasonal presence of the liquid water (ie. in lakes and iced “swamps”) over the ice/snow covered land surface affects the surface-atmosphere moisture exchange. A proper description of the land cover is a crucial element of numerical weather predictions (NWPs) and climate models, where the overall characteristics of the land cover are represented by surfaces covered by ground, whether vegetation, urban infrastructure, water (including lakes), bare soil or other. Then, various parameterization schemes (models) are applied to describe the surface-atmosphere exchange (Viterbo, 2002). Lakes have been recently included in the surface schemes of many NWPs (Salgado and Le Moinge, 2010; Dutra et. al., 2010) with known external parameters (location, mean depth) available from the Global Lake Database, GLDB (Kourzeneva, 2010;Kourzeneva et al., 2012). The information on only a few glacial lakes is included in the newest GLDBv3 version, and not any lakes found in Antarctica (Toptunova et al., 2019). Over 65 thousand glacial lakes have been detected over the East Antarctic coast via satellite remote sensing in austral summer 2017, and most of them have spread over the ice shelf (Stokes et al., 2019). For example, the total area of the glacial lakes in vicinity of the Schirmacher oasis was over 72 km^2 in January 2017 (Fig. 7), two largest glacial lakes are of the similar size as the Schirmacher oasis itself. Such amount of the liquid water may contribute to the additional source of the uncertainties inherent in the estimations of the regional evaporation after the NWPs. Estimates on evaporation are also available from climate and NWP models and atmospheric reanalyses. The most recent global atmospheric reanalysis is ERA5 of the European Centre for Medium-Range Weather Forecasts (Copernicus Climate Change Service, https://climate.copernicus.eu/, last access 09.07.2021; Hersbach et al., 2020). As other reanalyses, ERA5 does not assimilate any evaporation observations, but evaporation is based on 12 h accumulated NWP forecasts applying the bulk aerodynamic method. The results naturally depend on the presentation of the Earth surface in ERA5, and in the Dronning Maud Land, the surface type is ice and snow with no lake. Therefore the estimate of the evaporation does not include evaporation from liquid water surface. We estimated the daily evaporation also from the ERA5, and the results suggest that the evaporation during summer (DJF) 2017–2018 was 0.6 mm day^{-1}. It is only one fifth of the evaporation estimated with the direct EC method."

Description of weather conditions during the field campaign. Merge into one section, as they are scattered in various sections.

We revised the structure of the manuscript, and now the description of the field experiment is merged with the sub-section presenting the weather conditions.

Appendix: I do not see the relevance in this paper.

The appendix shows the results of the intercalibration between two instruments in order to estimate the instrumental errors inherent in the particular instrument (namely Irgason with the serial number SN1243), which we used during the field campaign. The instrumental error is one of the contributors to the uncertainties inherent in the EC method and due to its importance we would like to maintain the appendix in the manuscript. We explained the relevance in the following text: "Uncertainties in the estimation of evaporation after any method include the instrumental errors associated with the specific instrument. Aubinet et al., (2012) suggest three methods allowing to quantify the uncertainty of the EC method. In this study, we applied the paired tower method to evaluate the instrumental uncertainties of the EC method taking advantage of an intercomparison campaign in Alqueva reservoir, Portugal, during the Autumn 2018. Since the instrumental error does not depend on the region where the instrument will be used, it may be done elsewhere. The relative instrumental error estimated in this intercomparison campaign was 7 % (see the Annex). The uncertainties of the EC method also include the errors due to filtering the measurements to those covered by the footprint area, and there are 18 % of gaps which can be filled with the mean or median values or excluded from further calculations. The large number of filters and corrections that we applied to the EC data allowed us to reduce the errors and uncertainties. Even the EC method itself has some errors and uncertainties but is the most versatile and accurate method to measure the evaporation."

2) Propose complete figures, without repetition and with a good finish.
Figure 1: To be merged with Figure 4. Detail each color code.
We changed Figures 1 and 4 accordingly.

Figure 2: Not helpful, the compass rose in figure 4c does the job.

This plot shows the climatology of wind speed and wind direction in the study area, and we used it to explain how the location of the Irgason measurement site was chosen. Also, we used the data on the wind speed to design the maintenance system for the Irgason in order to sustain local winds. The wind rose in fig. 4c gave the situation for the period of 38 days of the field campaign, and it did not provide the climatology. We would like to keep the figure in the revised version of the manuscript due to its importance for the instrumental set up (good results depend on perfect set-up).

The following text was added to the manuscript: "To plan the field experiment we used 6 hour synoptic observations at the Novo site available from the British Antarctic Survey Dataset (https://www.bas.ac.uk, last access 14.12.2018) covering the period 1998–2016 to calculated the wind direction and frequency of wind speed anomalies over the multi-year means for eight ranges (Fig. 2). The positive anomalies in the wind speed suggest that the observed wind speed is higher than the mean value. In Fig. 2, the prevailing wind direction is ranged from 120 to 140º, and the positive wind speed anomalies are typical for this range. We accounted for these circumstances when choosing the location to deploy the EC measuring systems and to design its maintenance system to sustain the local winds."

Figure 3: Not useful.

The fig. 3 was excluded in the revised text.

Figure 4: add a footprint analysis to show that the retained data is contained on the lake.

We added a footprint analysis to Figure 3 where a windrose is presented. We also provided the explanations in the following text: "The Irgason was settled at the height of 2 m above the ground, and it allows for footprint lengths less than 200 m (Fig. 3c), with only one exception during the whole experiment. This distance is twice less than those between the Irgason and the shore of Lake Zub/Priyadarshini, in east-southeast direction (Fig. 1c). This condition ensures that the retained data is representative only from the lake and free of contamination from the shores.”

Figure 6a: not useful.

Yes, we agree, and therefore the sub-plot (a) is now excluded from Fig. 6.

Figure 7: add shaded areas indicating +- standard deviation

We finally decided not include the analysis of the intra-daily variability of the meteorological parameters, and therefore Fig. 7 was excluded from the revised version of the manuscript.

In general, there could be more homogeneity in the plots.
We agreed that generally the figures need better presentation, and it is improved in the revised version of the manuscript.

3) Data processing
It is not ok to fill in missing data with medians only, a more robust approach is needed (neural networks, marginal distribution sampling, etc.).
In this study we used two methods to fill those 18% of gaps in the 30 min time-series: by the median and mean values. There is not much difference in the results, and therefore we decided to use the only one value (median) to fill whole gaps, which are 18% of total data. In further study, we would like to test filling gaps by the average calculated from the neighboring values. More robust approaches (neural networks, marginal distribution sampling) can be also applied in studies focused only on the uncertainties inherent in the EC measurements, where such details will be good to provide. This is outside this particular study.

4) Bulk transfer approach
What is the impact of strong katabatic winds on lake surface roughness and consequently on C_{DzN}? What justifies the use of a constant value of 0.00181? Consider discussing this with the additional analysis I am recommending (see below).

In general, C_{DzN} depends on the wind speed. Several empirical forms have been developed to quantify the dependence (e.g., Taylor, 2002), but most of the studies have addressed open ocean conditions, where the state of the wave field is not restricted by the distance from the shore. The situation is different over a small lake, where the waves have typically not reached a balance but are still in a growing state with respect to the fetch and wind (except for very weak winds). We therefore considered it better to apply a C_{DzN} value found good for a small lake instead of applying a parameterization developed on the basis of data from open ocean conditions. From the point of view of C_{DzN}, it does not matter whether a strong wind is of katabatic or other origin.
Reference: Taylor, P. K., Air-sea interaction / momentum, heat and vapour fluxes, In: Holton, J. P., Curry, J. A. ja Pyle, J. (Eds.), Encyclopedia of Atmospheric Sciences. Academic Press, London, s. 93-102, 2002.

5) Elaborate 1-2 analyses looking at the processes controlling evaporation, to better value the dataset. Two suggestions:

What is the impact of katabatic winds on evaporation? I understand that this is not your main study goal, but katabatic winds are there and they definitely play a big role, so they have to be considered!

We added the analysis on the impact of katabatic winds to the revised version of the manuscript. The following text was included to the subsection of the methods: “To understand the role of the local (katabatic) winds in forcing the evaporation over the lake surface. The katabatic winds blow from the continental interior. Fig. 3 b shows that almost all winds come from a direction that would be the direction of katabatic winds. However, it is not guaranteed that all these winds are entirely of katabatic origin. Some may be driven by a combined effect of the local (katabatic) and synoptic forcing. In this study we distinguished between two groups of days of katabatic and predominantly synoptic forcing by calculating mean values and distributions of the evaporation for both groups of days. Then, the contributions of wind speed, air humidity and surface temperature were related to the temporal variations in the evaporation.”

We also extended the section of the results: “ The study region is dominated by winds from the southeasterly sector (Fig. 3 b). This corresponds to the katabatic winds, which the Coriolis force has turned left from the direct down-slope direction. To better understand the impact of katabatic winds, we carried out further analyses on the wind conditions in the study region. We calculated the geostrophic wind fields for each day of the study period from the mean sea level pressure fields estimated from the ERA5 reanalysis. The results demonstrated that the geostrophic (synoptic) wind was mostly from the east, i.e., some 45 degrees right from the mean direction of the observed near-surface wind. This deviation angle may partly result from the Ekman turning in the atmospheric boundary layer, which over an ice sheet with a rather small aerodynamic roughness may contribute some 20 degrees, and from the katabatic forcing. In any case, in most cases the observed near-surface winds resulted from the combined effects of synoptic and katabatic forcing, which supported each other, Hence, it is very difficult to robustly distinguish the impact of katabatic forcing on the near-surface winds over the lake. However, the geostrophic wind direction was distinctly different, 240 – 350°, in the following days: 6, 8 – 10, 19 and 25 – 27 January. These days were related to transient cyclones centered northwest of the lake or high-pressure centers northeast of the region under the study. During the days, the wind speed over the lake was strongly reduced (Table 7), as the katabatic and synoptic forcing factors opposed each other. The lake surface temperature was higher than usual, but the air temperature was lower. The latter is partly because, during events when the geostrophic and katabatic forcing factors support each other (sector 60 – 130°), the strong wind effectively mixes the atmospheric boundary layer. In stably stratified conditions, which prevail over the ice sheet, vertical mixing results in higher near-surface air temperatures (Vihma et al., 2011). In addition, the adiabatic warming during the downslope flow is a major factor contributing to higher air temperatures (Xu et al., 2021). The impact of adiabatic warming is also seen as lower relative humidity in cases when the geostrophic wind is from the sector 60 – 130°. Related to the compensating effects of air temperature and relative humidity, the specific humidity was not sensitive to the geostrophic wind direction. The effect of the wind speed dominated the effect of the lake surface temperature (which controls e_s in Eq. (3)), and evaporation was strongly reduced when the geostrophic wind was from the sector 60 – 130° (Table 7).

…Table 7

The katabatic wind was a quasi-persistent feature during the study period, and the major changes in the evaporation were driven by changes in the synoptic scale wind direction, which affected the local wind speed.

What is the relationship between lake stratification and evaporation?

Lake Priyadarshini is thermally homogeneous during the ice-free period (Sinha and Chatterjee, 2000). The possible reason for that is the strong katabatic winds allowing mechanical mixing of the whole water body. We add the text as follows: “The lake stays free of ice for almost two summer months from mid-December to mid-

February. In this period, the lake has no significant thermal stratification (Sinha and Chatterjee, 2000), and a possible reason for that is persistent katabatic winds mixing water down to the bottom. ”

Specific comments:

L37-38: rephrase 'The authors concluded that over 25%..."

The text is modified as follows: “Estimations of the area for the melting zone in Antarctica are also available from the microwave remote sensors for the summers in the period of 1979/80 - 2005/06, and the melting zone expands over 25 % of the continent at least five times (Picard et al., 2007).”

L57: I disagree, eddy covariance is a direct measure of turbulent water vapor fluxes.

The EC method is a **direct** method providing the estimates of the reference evaporation. We have stressed this circumstance within the whole revised manuscript.

The following text has discussed this circumstance in the Introduction: “ Performing the direct measurements of evaporation is difficult, and various methods are applied in the estimations of evaporation over the land surface including lakes. They are generally indirect because they are “point” measurements by an instrument and/or it is calculated from measured meteorological variables (Guidelines, 2008). Some of these methods are expensive and require special instruments and sensors for humidity, wind speed and temperature (Brutsaert, 1982; Finch and Hall, 2001): turbulence measurements (ie. eddy-covariance, EC method), profile measurements (ie. aerodynamic methods) and measurements at two heights (ie. Bowen-ratio based energy-balance methods). Among others, the eddy covariance (EC) method is recognized as most accurate in estimations of the evaporation. This method has been introduced for more than 30 years (Stannard and Rosenberry, 1991; Blanken et al., 2000; Aubinet et al., 2012). The turbulence (EC) measurements are direct measurements of the vertical flux of water vapour occurring over the lake surface. Assuming that the flux at the measurement height is the same as at the surface (or low), the EC measurements are direct measurements of local evaporation over a lake. In this study, we assumed that the point measurement of the EC measuring system was a direct measurement of the lake evaporation.”

L64: cite examples.

We have added the references further mentioned in the section with Discussions.

L105: A climatology is usually accomplished over a 30-year period. Also, how can the average incoming solar radiation be so high? How is it calculated?

We removed the term “climatology” from the whole text. Now, we directly mentioned the period, which is used in calculating the statistical values given in Table 1 and further.

Elena Shevnina,

On behalf of authors