Comment on tc-2021-218

Anonymous Referee #1

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

General comment:

This work presents an interesting in-situ investigation of evaporation from a glacial lake in Antarctica. The scientific question behind the manuscript is of great interest, the authors provide data from an EC station, which are used as reference for comparison with other well-known formulas for the estimation of evaporation. I appreciate the effort, the topic, as well as the EC data, which are crucial in this context, but I believe that some more work should be done in order to gather more useful and general results.

To my understanding, the authors aim at estimating the contribution of evaporation from summer time ice-free glacial lakes, so far underestimated or neglected in climatic models. Nevertheless, what they provide is a comparative study of different models for the estimation of evaporation on a single case study. Based on the daily estimates of evaporation from Lake Zub/Priyadarshini in three months (December 2017, January 2018 and partially February 2018), they conclude that "evaporation is a major term of the water balance of glacial lakes", but they do not mention the other terms of the water balance (a part from mentioning precipitation only once in the abstract and in the conclusions), nor in their case study or in other lakes. In this regard I recommend the authors to clarify themselves which is the real aim of their contribution, and improve the focus of the paper accordingly.

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.

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.

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.

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.

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.

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.

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?

L85-86: please revise the English language here.

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.

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

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).

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.

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

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.

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).

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?

 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.

L237-239: the verb is missing here.

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.

L244: Please clarify what are ψ_m and ψ_q for.

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

L254: depend**ing**

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.

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

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.

L270: please either provide an English reference for the s-sigma criterion or explain the method.

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

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.

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.

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.

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

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? Did they try testing the other station? Did they use a mean daily value or an instantaneous value? Please clarify.

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.

L315: estimated with what meteo data and what water temperature? please clarify

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.

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.

Figure 8: please add a regression line in black and in red with the R^2 coefficients.

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.

L353: What about water temperature under wind gusts? What about the filtering based on wind direction performed on EC data? Was it done also for winds measured at the two

stations?

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.

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

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

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.