This paper summarizes research to determine evaporation rates from two Antarctic lakes. The energy dynamics of these lakes are poorly documented so it is good to see the effort to try to measure these fluxes and examine the mechanisms that control them. There are some interesting results. The winds are amazing. An average of 8 m/s in summer! There are many who would be surprised to see evaporation rates this high so early in an open water season from such a cold location. The novelty of these measurements makes me think the paper should be published. Before that, the story needs improving to ensure the paper is impactful as it could be. I understand that this version might be a revision and the authors have responded adequately to previous comments. There remain some large issues with presentation and scientific robustness (these problems are intertwined) and overall messaging. Some are relatively minor, such as those about phrasing, and some are major, such as those that concern how the results are presented and their broader significance. I realize that the depth of these comments may be disheartening at this stage, but I hope my comments come across as constructive. I have not separated my comments in major or minor, but present them sequentially.

Line 22: change to “containing”

Line 32: change “often serve” to “provide”

Line 50: It is not quite correct that combination methods are empirical, but the ones the authors refer to use simplifications of combination methods which do involve empiricism. Perhaps it would be more correct to say "There are a wide diversity of approaches to estimate lake evaporation. Simplified, empirical, versions of combinations methods (e.g., Shuttleworth; Borghini, etc.) have been applied to Antarctica lakes even though ...."

Further to this point, there are numerous approaches to estimate lake evaporation. The authors would do well to dig deeper and mention these (e.g., Morton, Meyer, Priestley-Taylor, Penman, Bowen, Dalton types). The two references currently used are older and very general. Some papers that would be valuable for the authors to read and incorporate:

Meng et al 2020. Water 12, 3491.

Morton, 1983. Journal of Hydrology 66, 77-100

Islam et al 2024. Proceedings of the Canadian Society of Civil Engineering Annual Conference 2022, Lecture Notes in Civil Engineering 367, https://doi.org/10.1007/978-3-031-35471-7_65.

Nordbo et al. 2011. Journal of Geophysical Research 116 D02119, doi: doi:10.1029/2010JD014542,

Spence et al., 2003. Journal of Hydrometeorology 4, 694-701.

Granger and Hedstrom 2011. Hydrology and Earth System Sciences 15, 167-277.

Gan and Liu 2020. Journal of Geophysical Research: Atmospheres 125, e2019JD032334.

Blanken et al 2003. Journal of Hydrometeorology 4, 680-693 (and references within)

Oswald and Rouse 2004. Journal of Hydrometeorology 5, 129-144

This list is not exhaustive. The authors could even mention lakes where evaporation has been measured in warm locales such as Australia and Israel. The authors need to place their results in the broader context of lake evaporation rates observed elsewhere on the planet. More could be done in the paper to show why Antarctic lakes are so distinctive from elsewhere. The same processes govern evaporation here as elsewhere (e.g., winds, vapour gradients), but the combination of local conditions is what results in evaporation so early after breakup. Other cold regions lakes take time to warm up because of the significant heat capacity needed to be overcome. The authors measurements imply these two lakes start evaporating immediately. On these Antarctic lakes, it seems the wind may be the explanation, even though the authors suggest it is the vapour pressure gradient. I suggest the authors dissect this some more because wind may play a bigger role than the paper suggests, and this is different than anywhere else. There is much research coming out of China on lakes on the Tibetan Plateau (a couple examples above), and Canada with their cold lakes (again, listed above) that should be included in this paper for context. Some of these are listed above. My advice for how to evaluate the importance of wind vs gradients is further explained below.

Line 57: This is not a complete sentence.

Line 58: It is more correct to say the mass transfer coefficients can be calculated by measuring hydrometeorological conditions coincident with latent heat fluxes with eddy covariance (EC) measurements.

Line 61: This sentence needs rephrasing.

Line 64: These two thoughts do not constitute a full paragraph and could be deleted or moved to other places in the paper.

Line 76: change “featured by the” to “often subjected to”

Line 102: change “has been” to “becomes”

Line 123: This paper would be better if the methods of Shevina et al were included here. There is room. The paper is not too long already. Further to this point, in order for the reader to have confidence in these surprising observations, the authors need to present more detail on how they were taken.

Line 129: Please provide details of how these terms were measured. There is no mention of low frequency air temperature/vapour pressure measurements.

Line 133: These equations need to be presented separate from the text, either as equations or in a table.

Line 154: These are presumably the equations listed above. Perhaps a table is best, which can be referred to here.

Line 154: It was not immediately apparent that these parameterizations were from Heikinheimo et al. (1999), Andreas (1986), Arya (1988) and the observations. I suggest a table for these and the equations.

Line 158: These equations too should be presented separate from the text. But, I suggest below that maybe this exercise is not necessary for the paper.

Figure 3: The line width is different in the legend than in the graph. Because each lake was only instrumented for one summer, I suggest adding the year to the legend (e.g., ZB (2018) LG (2019-2020).

Figure 4: Some of the temperature data extend off the graph. Perhaps only show data for the period described in Section 3.1. It would make it simpler on the reader.

Line 200: "was" not "is".

Line 209 – 215. This would fit better in Methods.

Figure 5: Please state which camera's photos are in each of b), c) and d)

Line 221: This first sentence is not needed.

Figure 7: What are the percentages in the wind rose? Percentage of time from that direction? It is very difficult to see the green line on the east axis. Maybe make it thicker?

Line 229 – 234: Much of this too should be Methods.

Figure 8: The units here are different than those presented in the text.

Line 240: I think these data are from Glubokoe. It would be appropriate to include a paragraph on Zub.

Line 255: The authors need to provide some data of the ice covered fraction, as they mention this was calculated in the Methods section and it provides important context for the high evaporation rates.

Line 257: This interpretation of the data in Figure 9 is not correct. As VPD goes up, E goes down at Zub. But the relationship is less apparent than at Glubokoe, I admit. While I like Figure 9, another good way to show these relationships are in plots such as those in Peter Blanken's papers on Great Slave Lake and Lake Superior (Blanken et al 2000 Figure 6; Blanken et al 2003 Figure 5; Blanken et al 2011 Figure 9). Such figures would be very helpful here to dissect the major controls of evaporation form these lakes and compare them to other lakes. This goes for the bulk transfer coefficients as well. Guseva et al (2023), which the authors already cite, is a great resource the authors can use for comparing different lakes to their.

Blanken et al 2000. Water Resoures Research 36, 1069-1077.

Blanken et al 2011. Journal of Great Lakes Research 37, 707-716.

Table 3: Is the reference the same as "this study" in the last line? It is unclear what that is.

Line 298-301: This can be deleted because it is already in the figure caption.

Line 330: The word “lake” appears twice.

Line 394: I am not sure if you mean lake stratification here or not. But that also brings up the subject of lake heat storage - which should be discussed in the context of the combination methods that do not account for it. I know the authors did not measure it, but some discussion on the matter is needed in the paper.

Line 409: More context is needed for the reader about why this water balance estimate is important.

Line 420: I am not sure what this paragraph provides for the paper. I recognize the portion about solar radiation was in response to a reviewer comment. It may have been better placed in the Methods section.

Line 436: This is very important. I apologize if I am wrong, but wasn't the coefficient calculated with the same data used to test it? This is inappropriate and brings into question conclusions that a bulk aerodynamic method would be more accurate than others approaches. I recognize that this is large part of the interpretation and outcome of the work. That said, I feel the paper would be more impactful if there were more explanation of what controls evaporation from these lakes, and how that compares and contrasts with other lakes around the globe. I am not suggesting removing the comparisons with the combination methods, but don’t make the outcome of the paper an evaluation of methods. Instead, use those estimates (i.e., when they work and when they do not) to interpret what is necessary and what is not when it comes to predicting lake evaporation in Antarctica. Again, compare this to the results found elsewhere on the planet. Then the authors could bring in the content discussing solar radiation, as recommended by a previous reviewer. Which is important, because it controls the surface temperature of the water. However, if one looks at the bulk transfer relationship:

$$\lambda E = \rho_a \cdot \lambda \cdot C_E \cdot u \cdot (q_w - q_a)$$

incoming solar radiation is nowhere to be seen. Incoming solar radiation is important at a seasonal or annual scale, but with the large heat capacity of lakes, there are only specific situations where incoming radiation is the main driver, and that tends to be in shallow warm lakes. Which these are not. Finally, the authors could then bring the discussion back to the local situation of what is important to predict conditions around these lakes.

Again, compelling work. I hope these comments are useful for improving the paper.

Christopher Spence