Answer to:

RC2: 'Comment on egusphere-2025-1195', Anonymous Referee #2, 22 May 2025

Thanks a lot for your useful comments that help to improve our manuscript.
(The original comment is greyed out and italic and our response is black)

Thermobaric circulation in deep freshwater lake by Marks et. al.

In this work, the authors undertake a numerical process study to demonstrate the effects of a thermobaric circulation in a cylindrical domain. This domain is inspired by Lake Shikoku, which has been previously observed to undergo thermobaric circulation. The authors employ a 1D column model to explore the vertical transport of heat over several simulated years. The main crux of the argument is that by considering the in-situ density (as opposed to the potential density which excludes thermobaric effects a priori), the authors identify the process by which thermobaric effects effectively mix the water column. Overall, I thought this article was put together well, and interesting. I have a few concerns, however, that should be addressed prior to publication.

Thanks for the short summary of our manuscript and the positive feedback of the topic.

Main points

- *I am certainly empathetic to the process study approach utilized by the authors, and I'm happy to read work that uses an idealized approach to learn about different process in isolation. I am, however, wondering about the relative importance of thermobaric effects to other, potentially more vigorous, dynamical effects, especially those ignored in this study. I think a discussion on this topic by the authors would help the framing of the work.*

  The reviewer is right, other effects like fluxes at the surface are as important for the circulation in the lake at least at some depths. The parameterization of surface fluxes has been the extensive investigation of recent years and decades. However, the possible dominance of e.g. surface fluxes requires their exclusion from the demonstration of thermobaric effects, otherwise 1) thermobaric effects could be mis-interpreted as possible secondary effects of other driving forces and 2) the effects of thermobaricity could not be clearly demonstrated when other effects of similar importance are interfering. We are thankful for the advise and we will check, whether the arguments need clearer statements in the text.

- *Related to the above point, on line 111, the authors comment "this kind of deep water renewal is suspected to have a significant influence in this lake", and I'm wondering if they could clarify if they think this based on the observational data, or for some other reason, such as the depth.*

  Right, the statement is based on observational data: the homogeneous water below 4 °C in the deep layers of Lake Shikotsu, and the intersection of the T_{md} line defining this temperature. Probably we should add "see Fig. 1b)" in the text to make this clear.

- *While I was reading this work, I kept asking myself "What is the specific thing that thermobaricity is doing that's different"? It wasn't until I read section 3.5 that things (sort of) cleared up for me, though I'm still not quite sure.*
    - *In my opinion, the argument the authors try to make could be strengthened by first using section 3.5 as a straw man, and then discussing the new results (i.e. the results WITH thermobaricity). I think the authors even have their conclusions laid out this way already. Related to this, I encourage the authors to add a picture similar to figure 4, but for the "non-thermobaric" case. I think that would strengthen their argument for "what thermobaricity does".*

      Thanks for the suggestion of restructuring our manuscript in favor of better clarification. We will try to optimize the structure and also emphysize the new features we have found. Regarding a similar figure to figure 4, this would be figure 6. If you meant figure 3 we will consider this input in the revised version.

    - *I sort of understand what the authors are getting at in the "Convective Mixing" section, but I think some sort of schematic explaining the convective cell detached from the surface looks like, or maybe an arrow placed on figure 4(b) describing what they mean. (This would certainly aid in my understanding).*

      Thanks for pointing out this issue. We will try to clarify this in the manuscript.

Minor Points

There are typos in a few places (eg lines 73, 77, 112, 118, and a few more). Please carefully check the manuscript

Thanks for pointing out, we will check that.

Line 36: Can the authors clarify what they mean? This sentence beginning with "Ultimately…" is confusing and I'm not sure what the authors mean.

We wanted to state: At the surface, water below 3.98°C is more dense than slightly colder water, but from a certain depth, this density difference reverses. We will reformulate this.

Can the authors provide a little more info on how they arrived at equation (6). It's not so clear to me, but I think they've taken the derivative of rho_pot (rearranged from equation (3)), and then made the approximation that rho_pot \approximation rho_in-situ in the denominator of equation (6)?

In principal that connection can be made, but we did not used this derivation directly. We will clarify the derivation in the revised manuscript.

The authors mention that p=0 corresponds to atmospheric pressure at the surface (line 140), but this convention is employed (equation (4)) before it is mentioned in (section 3). Please mention this convention upon first usage.

We will correct this.

Lines 94-97: it's not clear to me what you're saying here. Is this maximal deviation the deviation that occurs over 360 m, or between 3 and 4 deg(C), or something else? The sentence in line 96 seems to imply it's something else.

It is actually the maximum deviation between the linear approach (Eq. 4) and the tabled more exact values. We will reformulate this sentence.

Line 133: Can the authors clarify what they mean by this sentence? I'm getting confused by the use of the words in the parentheses.

Mixing layers receive the average temperature, except if mixing includes the surface layer; then the temperature of all mixing layers is set to the surface temperature. (We will clarify this in the manuscript.

Line 203: "Stability frequency" is used. Is this standard? "Brunt-Väisälä frequency" is used in the abstract. I would standardize the usage throughout the paper.

Yes, we will standardize this in the manuscript.

In figure 1, pressure on the vertical axis is positive, but on the subsequent figures, it's negative. I would suggest that it be standardized to one or the other, or clarified in the text.

Thanks for pointing out this. We will correct figure 3 so that every figure has the same (positive downwards) pressure y-axis.

The authors model convection in a phenomenological way (i.e. all heat is exchanged between adjacent layers instantaneously). For the purposes of this work, I think it's probably fine, but I don't really know. Can the authors comment on whether they think that their approach is actually a good representation of the true convective processes going on in a lake? I.e. are the timescales appropriate? Is there evidence of a lake-wide circulation?

As you correctly mentioned we only aimed for a phenomenological representation of the lake mixing. Hence, our time scales regarding the mixing are not aimed to be realistic in the short term. During the year we think that the mixing patterns are represented conceptually correctly, if compared to the measurements of Lake Shikotsu. To get a more realistic time scaling we would need to implement more features into the model that would complicate it. We think that the phenomenon of the mixing is different to reality with regard to the exact length in time for the different phases, but not in the form of the conceptual phases themselves. The lake wide circulation can be assumed because of the measured profiles during the mixing phase in Lake Shikotsu.

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