Answer to:

RC1: 'Comment on egusphere-2025-1195', Anonymous Referee #1, 21 May 2025

Thanks for your review and your useful comments, we really appreciate this.
(The original comment in greyed out and italic and our response is black)

This work is one of the very few attempts to understand and characterize the sequence of events leading to circulation—manifested as the cooling or warming of very deep water—in thermobaric, deep freshwater lakes using a simplified 1D model. The philosophy behind using this simplified 1D approach is to isolate the effects of thermobaricity and cabbeling, rather than focusing on wind-driven energy input or the complex hydrodynamics associated with realistic 2D or 3D bathymetry. That being said, the model successfully identified how the variation of the temperature of maximum density (Tmd) with depth under significant pressure alone (thermobaricity) can drive mixing in a deep lake.
Thanks very much for the positive statements and briefly citing the focus of our work.

The model was applied to a deep, cold Japanese caldera lake (Lake Shikotsu), where the hydrodynamics are believed to be predominantly vertical. It also demonstrates how using potential density at the surface may lead to completely different results compared to using stability criteria.
We would not claim that we applied a model to Lake Shikotsu; we are clearly aware of the shortcomings for a realistic simulation (exchange processes at surface, lake basin, inflows / outflows, salinity). We rather used thermobarically stratified Lake Shikotsu as an inspiration and used few field observations to guarantee realistic boundary conditions.

- *The abstract would benefit from additional concluding sentences that elaborate on the key outcomes of the model, particularly the main physical features identified.*
  Our main conclusion is “Our results emphasize the feasibility and necessity of the implementation of thermobaricity in numerical lake models.”, but we agree that we should include a sentence on reproducing thermobaric effects in a strictly one-dimensional model and its behavior (in addition to the smaller points that have been listed).

- *A clear distinction between thermobaric instability, thermobaricity, and cabbeling is needed, as these concepts are often confused. This clarification should be addressed consistently throughout the manuscript, including the conclusion. It would also be valuable to highlight that, in this case, cabbeling appears to result from eddy diffusion across Tmd at different depths—a particularly novel observation that, to my knowledge, has not been previously reported. As I understand it, this process involves the diffusion between a parcel of water already at a warmer temperature of maximum density (Tmd) and colder water, ultimately producing water at a lower Tmd. This mechanism deserves emphasis given its potential implications for deep mixing processes and how is it compared with “thermobaric instability”.*
  We fully agree! In theory, thermobaricity and cabbeling are clearly separated: the previous covers effects deriving from the non-zero second order derivative of in-situ density after temperature and pressure, while the latter covers effects connected to the second order

derivation of in-situ-density twice after temperature. Despite this clear separation, cabbeling appears in our simulation of thermobaric effects and is recognized as the driving force for the deepwater convection. The resulting deepwater temperature (determined by the T_{md} transition of the temperature profile) is a typical thermobaric feature. This means: we agree fully with the reviewer's observation; the separation can be difficult, but we promise to do our best to be clear in the new version of the manuscript.

- *There has been brief but noteworthy scientific debates regarding the appropriate criteria for evaluating stability, which merit mention. For instance, Georg Wüst (1932) and V. W. Ekman (1934) discussed the use of potential density—specifically, surface-referenced potential density—as a means of assessing stability. However, it is important to clarify that potential density referenced to an intermediate depth has since been recognized as a more reliable indicator.*

  We trust that these old oceanographers had understood already 100 years ago how stability should be calculated: the additional compression of the deeper layer contributes to density but this does not add to stability. Even though the potential density at an intermediate depth is better than the potential density at the surface, it is still an approximation of using the in-situ density at every single point of the water column and still different to our approach. If stability is calculated at in-situ pressure then we achieve a good representation of thermobaric effects.

  This approach closely resembles what is being applied here, but at a common depth corresponding to the lower parcel,

  It is similar in so far, as the higher pressure effect on the deeper layer is removed by using the same pressure reference. However, our approach is fundamentally different, as our model uses the local pressure for stability calculations; hence, a different pressure for each stability calculation instead of one reference pressure for the entire model domain in space and time. Only with this approach a representation of thermobaric effects in a numerical model is done properly.

  and is supported by studies including Peeters et al. (1996), which also deserves mention. Finally, when considering which density measure to adopt, it may be useful to briefly reference the concept of quasi-density and explain why it has been excluded from the present analysis to contribute to the ongoing knowledge on the topic! It is very satisfying to see a comparison done with potential density at the surface, which I also believe one of the novel parts of this work.

  This "quasi density" of Peeters is a complicated quantity. We did not use this approach and hence we have not cited it. We will check again what it can be used for. However, we must avoid connecting "quasi density" to the simple conclusion of this paper: stability considerations based on in-situ density represent thermobaricity.

- *Why is the stability criterion being expressed in terms of density rather than simply using potential temperature, especially since salinity is excluded? (Gill, 1982; Imboden and Wüest, 1995). This approach might avoid the complications of selecting an appropriate density reference.*

  We are fully aware of this shortcut using potential temperature (Boehrer PPNW contribution in Lake Tahoe, 2008). However, the goal of this paper is not the reproduction of a temperature profile. The purpose of this manuscript is dealing with the theoretical side. We clearly prove that basing stability considerations on in-situ density covers thermobaricity. Building up on this, salinity can easily be included in a next step, and a proper numerical lake model will be used.

On that note, as mentioned in your manuscript (line 202), in-situ density is largely dominated by pressure, and there has been a brief scientific debate on the validity of using in-situ density for stability evaluation (A.H. Lee and G.K. Rodgers, 1972; Thomas Osborn and Paul LeBlond, 1974), ultimately ruling out its use. I believe what you are referring to in this publication is potential density at a common reference depth (at the lower parcel depth, not at the surface), which is conceptually like using an intermediate depth. It is not in-situ density, otherwise potential density at the surface is also in-situ density but the in-situ density at P2=0.

In our understanding / convention, potential density refers to density at one reference pressure, which remains the same in the entire domain of the simulations and observations (in time and space, especially depth), while in-situ density represents the density at any given pressure (in-situ density can be calculated for depths other than the current location of the water parcel). We thought this is convention, but this comment tells us, we should explicitly write the definition out in the manuscript.

We would assume that for Lee, Rodgers, Osborn and LeBlond accounts the same as for above mentioned Wüst and Ekman: We may cite them in a general statement that they already have pondered how to evaluate stability from density profiles.

An important consideration is what happens when this comparison crosses the Tmd line, as this transition is critical in our case: the compensation depth, which is defined relative to Tmd, governs the overall flow structure.

“Compensation depth” is commonly understood as the depth where a displaced water parcel starts moving downwards as a consequence of its in-situ density compared to the in-situ density of its horizontal neighbouring water. This expression comes from the understanding of deepwater circulation being accomplished by displacing cold near surface water (by wind) in the vertical which is based on horizontal gradients. This is closely tied to the understanding of deepwater formation in Lake Baikal or similar cases. Our model is strictly one-dimensional and hence we exclude any horizontal gradients even in parametrized form. Therefore, and because in our one-dimensional model the deep circulation always starts at the intersection of the T_{md} line and the temperature profile in contrast to the cases where the compensation depth is used we do not use the term compensation depth in our one-dimensional model.

Also, I believe more justification is needed for the choice of evaluating density using the speed of sound (which is not measured, or maybe you have measurements not mentioned?), rather than the TEOS-10 approach utilizing potential temperature and salinity? As mentioned, it is mentioned that TEOS-10 “which includes the effect” compared to potential density, but still, potential density “at the surface”.

We used the sound speed because it is directly connected to the compressibility (sound velocity squared is equal the reverse ofthe compressibility times density). We will check whether this needs more explicit mentioning. In our formulation, the sound speed part represents the compression. Additionally, TEOS-10 is designed for ocean conditions. We believe that the pure water is better described by the formulas we use, since our model uses pure water.

- *I think it is worth defining the compensation depth. You later refer (line 71) to the intersection of the temperature profile with Tmd, which could be described as the compensation depth. It may help with clarity to introduce and use this term consistently throughout the manuscript.*
  As mentioned before, the "compensation depth" only makes sense when there is an environment to flow relative to. We think, it is generally difficult or even misleading to introduce expressions only to distance ourselves from them. We will check what might make sense.

- *It is mentioned that the temperature profile remains isothermal throughout. Is this monitored using thermistors or a CTD, and what is the measurement accuracy of this isothermal profile? For example, is the variability within 0.1 °C or 0.5 °C?*
  In the model, isothermal means isothermal = same number and convective cells shaded in gray in Fig. 4a have identical temperatures. In the measured profiles (Fig. 1), the homogeneous deepwater shows temperature variations in the vertical in the range of a few Millikelvin (this is visible from the thickness of the lines); in numbers +-0.002 Kelvin. However, this paper does not aim at a realistic representation of the situation in Lake Shikotsu: the gist of the paper is the conclusion that basing stability considerations on in-situ density represents thermobaric effects (already one-dimensional) and the approach is feasible and the effects are obvious and important for the circulation of deep lakes.

  Clarifying this would help assess the significance of the observed isothermal conditions compared to the observed cooling/warming of the bottom water and also compare with other lakes. Additionally, where is the surface water temperature (model forcing) measured, and at what exact depth? In other lakes I believe it is usually at least 3-5 m deep in the surface mixed layer (I mean the shallowest thermistor).
  The surface water temperature was measured roughly at 1.5 m depth (also subject to water table variation) at the end of the piers (we used existing structures in the protected national parc for placing the sensors). However, in winter, temperature differences in the surface water are very small. We will mention the sampling depth in the new version of the manuscript.

- *Can you provide a specific analysis or statistic isolating how much of the observed changes are driven by diffusion leading to "cabbeling" or "thermobaricity"? Additionally, how would changing the diffusion coefficient affect the overall process, since it seems like the main driver?*
  Right; our model aims at the representation of thermobaric effects. The diffusion implemented gives the vertical length scale. High diffusion results in a thicker surface layer. As we do not aim at a realistic representation of the situation in Lake Shikotsu, but at reproducing thermobaric effects in a numerical model, the vertical length scale is not essential. Also, the vertical length scale does not change the behavior of the described deep mixing in the model. In detail, the results show that in winter the vertical length scale is too small, i.e. diffusion in the model is much smaller than in Lake Shikotsu, while in summer, heat is forwarded too fast into the deep water and hence diffusion in the model during summer is assumed much higher than in Lake Shikotsu. However, this is fully disconnected from the

scope of our paper. This can be dealt with in future investigations.

It is also unclear how the surface layer remains stable while convection occurs just beneath it that is (I believe) driven by cabbeling induced through diffusion? Clarifying this mechanism would help improve the physical interpretation.

The overlying water is not included in the deep convection cell as its density is lower.

- *Why are some profiles perfectly following Tmd, and are they considered stable according to the used stability criterion? Because I would think that maybe again turbulent diffusion perturbations might deem these profiles unstable. That would be interesting to think about.*

  As long as temperatures are higher than $T_{md}(z)$, profiles are stable, because the expansion coefficient alpha is positive. In this range, turbulent diffusion does not contribute to instability, it rather stabilizes the overall picture.

- *I think you need to clarify more the particular use of ±0.4 °C for different climate scenarios, the selection of a three-year spin-up period, and the chosen value for the diffusion coefficient.*

  These simulations are not really climate scenarios. Instead, the different winter temperatures have been chosen to demonstrate that the intensity of the mixing in the deepwater depends on the winter conditions. The system returns into the typical stratification within few years. There is no justification for the +-0.4 Kelvin. Still, the simulation does not attempt to produce a realistic representation of the circulation in Lake Shikotsu.

  Also, the method of mixing during the 1hour time step, is it sweeping downwards?

  For each time step the whole water column is checked for stability bottom up. If two neighbouring layers are unstable they are mixed. Afterwards, this mixed part is compared with the neighbouring layers below the same way and so on until it is stable again. By this the whole water column is stabilized during each time step.

  When does the algorithm stop?

  As a consequence, the stratification is stable after each stability check. A repeated mixing is not required.

- *The discussion needs more comparison with previous studies especially with the closest model (Piccolroaz 1D model in 2013).*

  We will check what can be added.

Specific notes:

Line 60: "Admittedly" I am confused from the structure of this sentence, what is being admitted?

We will check this sentence.

Line 76: Potential density "at the surface ". I think it is worth stating this whenever mentioned.

We are not sure what the remark "at the surface" should indicate here,. This sentence in line 76 is correct since the pressure dependence gets lost by using the potential density no matter at which depth it is referenced.

Line 105: So, this is the oscillation frequency using potential density at a common depth, not using in-situ density as it appears. Because in-situ means in its place, but you are evaluating both at P2, so it is *confusing. Using actual in-situ density gradient to evaluate N^2 would give a misleading sign as it is always dominated by pressure, hence again it is worth noting that this is not the in-situ density gradient, but the potential density or the density at a common reference that is the lower parcel depth.*

As mentioned before, the in-situ density can be calculated at different pressures as well. When using the (conventional) potential density only one or a few certain values for the pressure are used for the whole water column to get rid of pressure influences. However, we calculate the (in-situ) density for every pair of cells directly at their point of interaction, which we would consider in-situ. You are right, we use a common pressure for this comparison, but since this is different for every comparison it is the in-situ density and includes the compression of each water parcel even for the smallest movement to ensure correct stability considerations. That’s why we stick to in-situ density to emphasize the inclusion of the compressibility in our calculations.

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