

## Author's Response Letter

Dear Damien Bouffard and reviewers,

Thank you all for your great input and comments that have helped us to improve our manuscript. We really appreciate the time and effort you have put into this. Your comments stress (demonstrate) the interest in the topic and we found them very encouraging. We gave our best to implement and answer all of your comments and hope this is another helpful step in finalizing our manuscript.

To reply to the comments of the editor and the reviewers (black) we added our replies and changes in between (blue). The line numbers of the changes made in our replies refer to the revised manuscript while the line numbers stated in the comments of the reviewers refer to the manuscript version 2.

Sincerely,

Joshua Marks and Co-Authors

15 Jul 2025

**Editor decision:** Reconsider after major revisions (further review by editor and referees) by [Damien Bouffard](#)

**Public justification (visible to the public if the article is accepted and published):**

The reviewers have provided extensive comments, and your responses offer a good framework for the revised manuscript. I would like to emphasize the need to incorporate all reviewer comments into the manuscript revision itself (not only address them in your response letter). Please pay particular attention to the concerns raised by Reviewer 1. I believe this revision presents an excellent opportunity to more clearly integrate the findings and theoretical framework established by previous researchers. Please also give careful consideration to the comments regarding diffusive and cabbeling processes. Finally, I remain unconvinced by your response regarding TEOS-10 (see Pawlowicz and Feistel 2012). I look forward to seeing the revised manuscript addressing these issues. This work tackles an interesting and understudied topic

Thanks for your comment. We agree in incorporating the reviewers' comments into our manuscript to improve it. We think the revised version has hugely benefited from the reviewer comments and we tried to focus on the points you mentioned.

Regarding TEOS-10: We felt and we still feel that TEOS-10 is not the appropriate approach for our purposes. While our model is simplistic and we use pure water (no salts) as the liquid in our model and we only use potential density and compressibility, TEOS-10 is designed for ocean water and it is a complex instrument to cover many properties of physical chemistry. We do not know of any indication that TEOS-10 is more accurate than Tanaka and Belogolskii (for fresh water, of course). However, we admit, that for realistic simulation of a real lake, a density approach that includes salts must be implemented. (We expanded our answer in the corresponding part of our answers in RC1. Also, Pawlowicz and Feistel (2012) showed that adaptations to the salinity are beneficial for limnological applications.)

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

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 ( $T_{md}$ ) with depth under significant pressure alone (thermobaricity) can drive mixing in a deep lake. 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.

Thank you for your positive statement and the short summary of our work. Regarding Lake Shikotsu, we only used this lake as inspiration for our model and did not aim for a realistic representation of this lake. Our model focuses on the conceptual behavior of such a deep lake under the influence of thermobaricity.

- The abstract would benefit from additional concluding sentences that elaborate on the key outcomes of the model, particularly the main physical features identified.

We added the main physical outcomes by changing the last part of the abstract as follows: “We were able to identify key features of the deep water circulation: cabbeling occurs at the intersection of the temperature profile with the  $T_{md}$  (temperature of maximum density) line due to diffusion and induces thermobaricity driven deep water circulation; this deep water circulation cell is detached from the surface, but can extend over hundreds of meters to the lake bed; the deep water stays isothermal; and after the winter stratification the temperature profile aligns with the  $T_{md}$  line. [...] in numerical lake models by basing stability (Brunt-Väisälä frequency) on in situ density.”, lines 11-18.

- 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  $T_{md}$  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 ( $T_{md}$ ) and colder water, ultimately producing water at a lower  $T_{md}$ . This mechanism deserves emphasis given its potential implications for deep mixing processes and how is it compared with “thermobaric instability”.

We fully agree and tried to improve the distinction between the different processes and to emphasize the influence of cabbeling more. Several changes were made in the manuscript: “cabbeling occurs at the intersection of the temperature profile with the  $T_{md}$  (temperature of maximum density) line due to diffusion and induces thermobaricity driven deep water circulation”, lines 12-14 (in the abstract); “Here, water mixes across  $T_{md}$  due to diffusion and produces denser water (Grace et al., 2023a, b). This diffusion induced cabbeling can only occur because of the change of

$T_{md}$  with depth, i.e. thermobaricity.”, lines 198-199; “Due to the missing change of  $T_{md}$  with depth there is no cabbeling during the winter stratification below the surface and no successive downwards circulation that would be further driven by thermobaricity.”, lines 259-261; “[...] were (1) diffusion induced cabbeling at the intersection of the temperature profile with the  $T_{md}$  line and its control of the deep water temperature by thermobaricity driven downwelling, (2) the period when surface water and deep water convection cells are separated, (3) the isothermal deep water, as well as [...]”, lines 285-287; “[...] dynamics were exclusively driven by diffusion induced cabbeling around  $T_{md}$  and successive vertical convective mixing controlled by thermobaricity below, [...]”, lines 310-311; “[...] cabbeling occurred [...]  $T_{md}$  line due to diffusion [...]”, line 319.

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

We fully agree with the statement of Ekman (1934). His point of using intermediate depths for comparison is the same approach that we use with the difference, that we always refer to the depth (pressure conditions) where we evaluate the stability between two neighbouring layers. We tried to clarify our statement by adding sentences in the introduction: “For proper stability considerations, the densities of two neighbouring water parcels need to be compared at intermediate identical pressure conditions, i.e. at the pressure at which the two water parcels are and interact (Ekman, 1934). For this, the adiabatic change of the water parcels from their origin towards the comparison pressure has to be considered (Osborn and LeBlond, 1974). We show that using in situ density for stability considerations is a proper approach if this adiabatic change is considered.”, lines 52-56, and adapting the first paragraph of section 2.2: “Potential density  $\rho_{pot} = \rho_{pot}(T)$  is a function of (potential) temperature at a certain reference pressure. In this paper, we follow the usual oceanographic and limnological practice of referring to atmospheric pressure (though sometimes other pressure references have been recommended for use, e.g. Ekman, 1934; Osburn and LeBlond, 1974). On the contrary, densities calculated at other pressures than atmospheric are referred to as in situ density  $\rho_{in-situ} = \rho_{in-situ}(T, p)$  in this paper.”, lines 98-101.

We also acknowledge the work of Peeters et al. (1996). However, we think that the “quasi density” and the “neutral tracks” do not contribute to the understanding of the convective circulation dealt with in our paper. Therefore, we abstain from referring to quasi density, but we refer to this work in relation to their correct definition of the Brunt-Väisälä frequency: “Similarly, the Brunt-Väisälä frequency of a displaced water parcel can be calculated by subtracting the adiabatic density change due to pressure changes from the water column (in situ) density gradient: the Brunt-Väisälä frequency is positive (the water column is stable) when the adiabatic density change of the

displaced water parcel exceeds the surrounding water column (in situ) density change (Peeters et al., 1996). This is done straight forward by using the in situ density as [...].”, lines 128-132 and stated our different result as well: “Hence, the adiabatic compensation of the pressure difference formally results in comparing  $\rho_{\text{in-situ}}$  of the two water parcels at a common pressure. As common pressure we chose the depth of one of the water parcels.”, lines 134-136.

- 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 the possibility to refer to potential temperature. However, the gist of the paper is the demonstration that thermobaric features are represented properly by basing stability on in-situ density. In conclusion, salinity can be easily included, which will be the next step for the implementation in a proper lake model, in contrast to only referring to the potential temperature.

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  $P_2=0$ .

Sure, the densities of two water parcels must be compared at the same pressure. The cited references also state that this pressure of comparison should be close to in-situ pressure. Our definition defines any density at any pressure as in-situ density  $\rho_{\text{in-situ}}$ . Hence, stability in Equation (6) is based on  $\rho_{\text{in-situ}}$ . We tried to clarify this topic in the introduction and section 2.2 as mentioned above and also cited the interesting old references.

An important consideration is what happens when this comparison crosses the  $T_{\text{md}}$  line, as this transition is critical in our case: the compensation depth, which is defined relative to  $T_{\text{md}}$ , governs the overall flow structure.

The compensation depth is originating from wind induced surface water displacement. It is calculated by comparing the in-situ density of the displaced water with the in-situ density of the surrounding water. In our one dimensional model a compensation depth does not exist. In our case, cabbeling creates denser water across the  $T_{\text{md}}$  line, which can proceed downwards due to thermobaricity.

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 speed of sound since it includes the temperature dependency of the compressibility and therefore offered a simple theoretical approach to derive a formulation for the in-situ density also including this dependency. We offer a direct and easy derivation: Tanaka for potential density and Belogolskii for sound speed for compressibility, which are the best approaches for pure water. The complexity of TEOS-10 is not appropriate for our needs. (Note: We changed sound velocity to sound speed in our manuscript to be clear that we only use the scalar quantity)

- I think it is worth defining the compensation depth. You later refer (line 71) to the intersection of the temperature profile with  $T_{md}$ , which could be described as the compensation depth. It may help with clarity to introduce and use this term consistently throughout the manuscript.

We argued about the use of the term compensation depth in the comment above. A compensation depth does not exist in our one dimensional model.

- 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? 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).

For the inspiration of Lake Shikotsu, this was measured with a CTD (actually two for backup). The temperature variations in the homogeneous (isothermal) deep water were in the range of a few Millikelvin: about  $\pm 0.002$  Kelvin and the two probes were also in agreement within the range of 0.025 Kelvin (see Boehrer et al., 2009). However, we did not aim for a realistic representation of Lake Shikotsu: cooling/warming of the bottom water as presented in the manuscript should not be interpreted as realistic values for this lake. The surface water temperatures were measured at about 1.5 m depth, while they were also subject to water level variations. Still, during the winter the surface temperature differences are very small at the surface. We added the information of the measurement depth in the manuscript: “[...] measured surface temperature from Lake Shikotsu at a depth of about 1.5 m with one measurement every minute from 18 October 2023 to 8 May 2024 [...]”, lines 147-148.

- 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? 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 only forcing of the model is the surface temperature and it only induces the convective mixing at the surface. During the winter stratification no deep mixing would occur without diffusion since the surface water circulation cannot cross the  $T_{md}$  line, also not during the summer warming. Therefore, the whole deep water circulation is created by cabbeling at the  $T_{md}$  line, which would not occur without some diffusive

or turbulent mixing, which is involved in our model through the diffusion. In general, the strength of the diffusion does not change the behavior of the deep mixing at all, it only defines the vertical scale of the mixed surface layer by controlling how much cold/warm water during winter/summer is transported downwards. In reality stratification and turbulence would influence this diffusive mixing, but in our model, we did not aim for a realistic representation of those processes.

We tried to clarify this in the manuscript by changing lines 198-202: “Here, water mixes across  $T_{md}$  due to diffusion and produces denser water (Grace et al., 2023a, b). This diffusion induced cabbeling can only occur because of the change of  $T_{md}$  with depth, i.e. thermobaricity. The denser water then circulates the water column below due to thermobaricity to the depth where lower temperatures stabilize the water column. Since the surface water is further cooled in this phase, cold temperatures diffuse downwards to the intersection with  $T_{md}$ . This deepens the intersection and ever colder water mixes into the deep water.” and lines 318-320: “During winter stratification, cabbeling occurred at the intersection of the temperature profile with the  $T_{md}$  line due to diffusion which induced convective mixing below due to thermobaricity.”

- Why are some profiles perfectly following  $T_{md}$ , 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.

During the summer warming, the surface temperature rises and when it is between the deep water temperature and 3.98 °C the surface circulation stops at the  $T_{md}$  line. Below, the temperature profile is stable. Further increasing surface temperatures will also mix until they reach the  $T_{md}$  line. This is why the temperature profile is so close to the  $T_{md}$  profile. The temperature profile is slightly higher than  $T_{md}$  (otherwise it would not be stable). Diffusion can only move it further away from  $T_{md}$ : Hence diffusion can only stabilize the profile.

- I think you need to clarify more the particular use of  $\pm 0.4$  °C for different climate scenarios, the selection of a three-year spin-up period, and the chosen value for the diffusion coefficient. Also, the method of mixing during the 1 hour time step, is it sweeping downwards? When does the algorithm stop?

The surface temperature input changes are no climate scenarios. We only chose them to demonstrate the influence of the winter conditions on the deep water circulation. There is no justification of the exact values of  $\pm 0.4$  °C, but we do not aim for a realistic representation of the circulation in Lake Shikotsu.

For each timestep the whole water column is restabilized bottom up. Two neighbouring water parcels are mixed if they are unstable and then the mixed part is compared with the next lower water parcel. After stabilizing the next higher neighbouring water parcel is checked again. The algorithm stops when the whole water column is stable. We tried to clarify this in the manuscript in lines 164-169: “[...] two layers, respectively. This comparison is done bottom up. If the stratification is stable, there is no action and the next upper layer is checked. However, if water in the lower layer is less dense than in the upper, the layers are mixed and the temperatures of the two layers are averaged. The mixed layers are then iteratively compared and mixed with all layers below in the water column until they are stable again. Then again, the next upper layer is checked. If the layers are unstable up to the

temperature controlled surface layer, the temperatures of all corresponding layers are set to the prescribed surface temperature. By doing so, all instabilities are removed over the whole water column in each time step.”

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

We added the sentences “While Killworth et al. (1996) used a two dimensional model to include the wind in combination with a one dimensional model for vertical tracer distributions, Piccolroaz and Toffolon (2013) parametrized the wind forcing to calculate the forced downwelling before assessing the stability. In contrast to them, we excluded forced plume downwelling while allowing for cabbeling to present the existence of thermobaricity controlled deep water mixing that is purely one dimensional and not driven by wind. Additionally, we wanted to emphasize the possibility of directly using in situ density for stability considerations to include thermobaricity in lake models by comparing two water parcels at a common depth. Piccolroaz and Toffolon (2013) also used a common depth for comparison but only considered the adiabatic change in in situ temperature, while we could avoid this additional calculation by referring to potential temperatures only.”, lines 61-69, to make the differences clear.

### **Specific notes:**

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

You are right, the formulation was unfitting and the sentence was reformulated: “Even though Lake Shikotsu is a particularly nice representation of thermobaricity and the necessity of including this effect is obvious, we hope to convince [...]”, lines 81-82.

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

In section 2.2 we now clearly define potential density as density at a certain reference pressure, in our case atmospheric pressure; densities at any other pressure are called in-situ densities.

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.

Densities at pressures other than one certain reference pressure are called in-situ densities. Hence, stability (Eq. 6) is based on in-situ densities. (compare section 2.2 and comments above)

**Citation:** <https://doi.org/10.5194/egusphere-2025-1195-RC1>

### **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.

[Thank you for the short summary of the manuscript and the positive feedback.](#)

#### **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 excluded effects, e.g. wind, would add other dynamical mixing behaviors like stronger mixing at the surface at least into a certain depth. Hence, the idealized approach was necessary to guarantee that thermobaric effects were not misinterpreted as possible secondary effects of other driving forces and that no other effects interfere with the effects based on thermobaricity. To pronounce this, we added the sentences "In our model, we wanted to ensure that there was no interference of other effects with the effects of thermobaricity and no misinterpretation of thermobaric effects as secondary effects of other driving forces." in lines 70-71 and "The exclusion of other driving forces prevented their interference with and the misinterpretation of thermobaricity driven effects." in lines 146-147.](#)

- 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.

[Yes, it is based on the observational data from Lake Shikotsu: the isothermal deep water colder than 4 °C and the intersection of the temperature profile with the  \$T\_{md}\$  line. To clarify this, we added "\(compare Fig. 1b\)" in line 140.](#)

- 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 for better understanding. We think that a jump from explaining and using in-situ density in the model description to potential density and then back to in-situ density would be disadvantageous. Instead we would like to stay with the implementation of the in-situ density in the model first and then directly showing its outcomes. However, we agree with the reviewer, that a more direct comparison between the simulation results using in-situ density and potential density without the section 3.4 Interannual Variations would be beneficial. Therefore, we swapped section 3.4 and 3.5 (including swapping Fig. 5 and 6 in numbering) and added “[...] interannual variability for the simulation using in situ density, [...]” in line 265 in the new section 3.5. Regarding Fig. 4, the similar graphic is given with Fig. 5 (in the revised manuscript). Adding a similar figure to Fig. 3 would not be beneficial, since it would be only the normal surface water stratification on top of an isothermal deep water at 4 °C throughout the year.

- 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).

We had the discussion, whether or not to include circulation arrows in the grey fields of Fig. 4 and 5; hence we can relate to this idea. However, we decided against and we confirm this decision.

## 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 the hint, we checked the manuscript.

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 reformulated the sentence to hopefully clarify the meaning: “Ultimately, water colder than 3.98 °C is less dense than slightly warmer water at the surface but denser from a certain depth where  $T_{md}$  is low enough.”, lines 40-41.

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  $\rho_{pot} \approx \rho_{in-situ}$  in the denominator of equation (6)?

We did not use our definition of the in-situ or potential density but rather the definition of calculating the Brunt-Väisälä frequency by subtracting the adiabatic density change due to pressure changes from the density gradient to ensure correct stability considerations. To clarify this we changed lines 128-132 as mentioned earlier in this response letter to: “Similarly, the Brunt-Väisälä frequency of a displaced water parcel can be calculated by subtracting the adiabatic density change due to pressure changes from the water column (in situ) density gradient: the Brunt-Väisälä frequency is positive (the water column is stable) when the adiabatic density change of the displaced water parcel exceeds the surrounding water column (in situ) density change (Peeters et al., 1996). This is done straight forward by using the in situ density as [...]”.

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.

Thanks for pointing this out. We added this in lines 110-111: “Here, in situ density and sound speed depend on temperature, salinity, and pressure while potential density, here with the reference pressure of 0 bar at the water surface, only depends on temperature and salinity.” and also mentioned it beforehand in lines 98-100: “Potential density  $\rho_{\text{pot}}=\rho_{\text{pot}}(T)$  is a function of (potential) temperature at a certain reference pressure. In this paper, we follow the usual oceanographic and limnological practice of referring to atmospheric pressure (though sometimes other pressure references have been recommended for use, e.g. Ekman, 1934; Osborn and LeBlond, 1974).”

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.

The maximum deviation mentioned is for both, the depth range and the temperature range. We reformulated the sentences to clarify this: “We used the surface and bottom values for linearization of the modeled depth range up to 360 m (compare Sect. 2.3). Hence, the maximum deviation occurs at mid depth, regardless of the temperature. For the interesting temperature range close to the temperature of maximum density  $T_{\text{md}}$ , between 3 and 4 °C, a maximum deviation of  $2.13 \cdot 10^{-12} \text{ s}^2 \text{ m}^{-2}$  of the sound speed occurs for 3 °C. This deviation is small relative to the change of  $\frac{1}{c^2}$  with respect to temperature or pressure, which are in the order of  $10^{-9} \text{ s}^2 \text{ m}^{-2}$ .”, lines 118-122.

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.

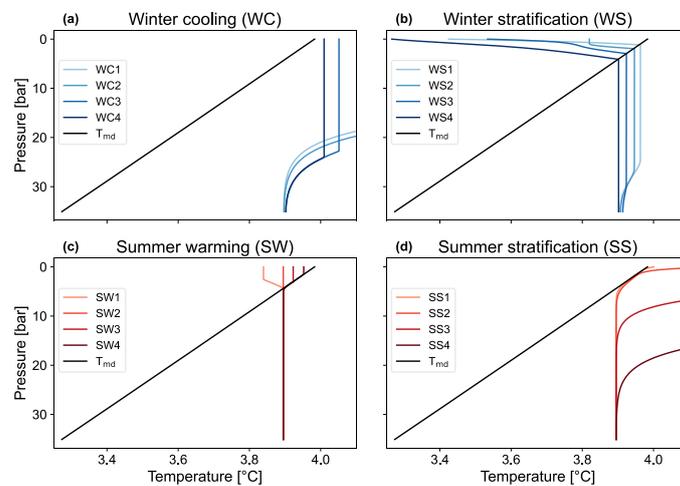
We tried to clarify this. The sentence now reads: “If the layers are unstable up to the temperature controlled surface layer, the temperatures of all corresponding layers are set to the prescribed surface temperature.”, lines 167-168.

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.

We changed it to “Brunt-Väisälä frequency”.

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.

It was a mistake in Fig. 3, where the pressure was negative instead of positive. We corrected that figure, so that all figures now have positive pressure downwards. Here is the corrected Fig. 3:



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 we only aimed for a phenomenological representation of the mixing processes in the lake based on thermobaricity, we created no model with realistic timescales. To have realistic timescales more input and more complex mechanisms would have to be included into the model. However, we think that the general behavior of the mixing is correctly represented by our model, because we see the same characteristics in the measured profiles in Lake Shikotsu (compare Fig. 1b). This is the case for the vertical extent of the mixed surface water and the isothermal deep water as well as for the conceptual different mixing phases throughout the year. Even though their spatial and temporal extent differs from reality the principal behavior was represented solely by our simplified model. We added the sentence “These extensions would produce more realistic timescales for the mixing and its different phases.” in line 300 to clarify this. Also, our model and the profiles in Fig. 1b indicate a lake wide circulation.

**Citation:** <https://doi.org/10.5194/egusphere-2025-1195-RC2>

**RC3:** ['Comment on egusphere-2025-1195'](#), Anonymous Referee #1, 03 Jun 2025

This manuscript uses a vertical 1D idealized model to capture thermobaric effects on the seasonal evolution of thermal stratification. The aim of the manuscript is to highlight physical processes that dominate the seasonal cycle. The model implements a novel estimate for gravitational stability, as well as simplified vertical diffusion and surface thermal forcing. Using these three simple concepts they are able to reproduce the basic characteristics of

observed thermal stratification in Lake Shikotsu, Japan, a caldera lake whose thermal dynamics are believed to also be mostly vertical 1D.

Thanks for summarizing the aim and results of our work.

Much is made of the novel implementation of gravitational stability, but little is said of what previous modellers have done, eg Killworth et al (1996) and Piccolroaz and Toffolon (2013). How is the formulation derived here different and what are its advantages over other, existing formulations?

We added the following sentences in the manuscript: “While Killworth et al. (1996) used a two dimensional model to include the wind in combination with a one dimensional model for vertical tracer distributions, Piccolroaz and Toffolon (2013) parametrized the wind forcing to calculate the forced downwelling before assessing the stability. In contrast to them, we excluded forced plume downwelling while allowing for cabbeling to present the existence of thermobaricity controlled deep water mixing that is purely one dimensional and not driven by wind. Additionally, we wanted to emphasize the possibility of directly using in situ density for stability considerations to include thermobaricity in lake models by comparing two water parcels at a common depth. Piccolroaz and Toffolon (2013) also used a common depth for comparison but only considered the adiabatic change in in situ temperature, while we could avoid this additional calculation by referring to potential temperatures only.”, lines 61-69.

The distinction between instability induced by vertical displacement of a stable profile to a depth where it becomes unstable (“forced plume downwelling”) and mixing of waters (“cabbeling”) is an important one. While it shows up in the introduction (Line 40-50), it gets a bit lost in the rest of the text. For example, line 50 seems to equate cabelling with simply “thermobaricity”. I recommend clearly and consistently delineating and labelling these two processes throughout the ms. To me, the most interesting result of this work is the focus on how surface convection interacts with the Tmd line subject to cyclical surface forcing.

We fully agree. We changed the sentence to: “[...] we excluded forced plume downwelling while allowing for cabbeling to present the existence of thermobaricity controlled deep water mixing that is purely one dimensional and not driven by wind.”, lines 64-65. Also, we made several changes/additions to our manuscript to highlight the influence of cabbeling and the distinction between the two processes. We listed these changes in the 2<sup>nd</sup> bullet point of RC1 above.

I encourage the authors to say more about the surface forcing. You use an hourly timestep to resolve the diurnal evolution. Cite or specify some details about the surface measurements (eg depth, sampling interval, instrument details). Why was it important to resolve the diurnal cycle? Do the results change if you use daily averages?

We added more details of the measurement: “The surface temperature input is also based on the measured surface temperature from Lake Shikotsu at a depth of about 1.5 m with one measurement every minute from 18 October 2023 to 8 May 2024 with a temperature sensor RBRsolo<sup>3</sup> T (RBR, Canada), [...]”, lines 147-149. We wanted to include the diurnal cycle to include the higher temperatures during the day and the lower temperatures during the night. The conceptual mixing phases throughout the year would be similar for daily averages, but it would change the vertical structure due to the corresponding change of the diffusivity caused by the different time steps.

I would like the authors to say more about the two mixing processes built into the model (convective readjustment and diffusion) and how they interact. Currently the manuscript focuses on calculation of stability and subsequent convective readjustment, but says little about the effects of what appears to effectively be a background constant diffusivity set to a rather high value, especially for the deep waters of a deep lake with relatively small surface area. How sensitive is the model to the chosen value of diffusivity (or (time step)/(grid size) ratio)? How does the diffusivity interact with convective instability correction? Why did you even add diffusivity? Presumably the results are very different without it.

The diffusivity acts as the vertical length scale. Higher or smaller values would change the vertical extent of the temperature profiles due to larger or smaller exchange in the water column, which would also influence the deep water temperature, but the general behavior of the deep circulation would stay the same. The general inclusion of diffusion ensures the possibility of cabbeling and other potential processes that are not directly driven by external forcing. Without it the temperature profiles would be pulled towards 4 °C and only the surface layer would follow the forced surface temperature. We chose a reasonable constant value knowing that in some areas it will be too low and in others too high to exclude the possibility of interfering mixing behaviors with those we wanted to observe. We added “We included diffusion to ensure exchange within the water column itself and include processes that are not directly driven by external forcing like cabbeling.” in line ??? and “This stabilization takes place after the forcing at the surface and successive diffusion.” in lines 158-160 to clarify this.

I find the use of “in situ density” to describe the stability model to be misleading. The formulation for stability developed in this ms seems to be a discrete approximation of potential density using a reference pressure at the lower of the two grid cells being compared. Put another way  $\rho(T_1, p_2)$  can be said to be potential density from cell one evaluated at a reference pressure of cell two. I would be more comfortable saying either stability was estimated by “accounting for compressibility effects using local temperature and pressure”, or “using potential density with local reference pressure”, or something like that. I appreciate that the authors have written the formulation of stability in terms of density and  $d\rho/dp$  (or  $c$  or  $1/\text{bulk modulus}$ ) rather than temperature and  $\alpha$  (thermal expansion coefficient), and there isn't really a word for “compressibility effects” in this novel density formulation in the same way there is a thermal expansion coefficient (ie  $\alpha$ ) for a temperature formulation.

We now provide a clean definition of potential density and in-situ density in section 2.2. (see also comments to RC1)

Minor/editorial comments

Title: should include words like model and 1D.

We changed it to: “Thermobaric circulation in a deep freshwater lake: A conceptual 1D model”

Abstract: The abstract includes a lot of introductory and methodology, but no results. This reads more like an aspirational conference abstract, rather than a complete work published in a journal.

We added further information. See the answer to the 1<sup>st</sup> bullet point in RC1 above.

Line 29: “The effect deriving from this property is called thermobaric effect”. This sentence is not very helpful in defining what you mean by “thermobaric effect” or “thermobaricity”. This is a good place to clearly define it, especially if you plan to use it to differentiate from “cabbeling” (Lines 39-44) or “forced plume downwelling” (line 50)

We combined that sentence with the previous one: “[...] including the temperature dependence of the compressibility in the water properties, which is called thermobaricity (McDougall, 1987).”, lines 32-33. The definition is now given in this sentence and further details are then given in the following paragraph.

Line 40: “Cabbeling originates from thermal bars...” seems misleading and not very helpful. One might also say thermal bars originate from cabbeling. I recommend simply saying “Cabbeling occurs where ...”

The sentence was changed: “Cabbeling occurs where mixing of two water parcels across the temperature of maximum density results in even denser water, which itself drives convective circulation, e.g. in the case of thermal bars (Ivey and Hamblin, 1989; Shimaraev et al., 1993).”, lines 45-47.

Line 43: “Although deep water renewal in some lakes is controlled only by thermobaricity, also cabbeling may be involved in the deep mixing...”. Without a definition of thermobaricity it is not clear what you mean by “some lakes”. Which lakes? What are their properties? Give an example of one that is controlled only by thermobaricity and not cabbeling.

We modified the sentence to clarify this: “Although deep water renewal in deep lakes experiencing surface water temperatures below 3.98 °C is controlled mainly by thermobaricity, also cabbeling may be involved in the deep mixing and deep water formation.”, lines 47-49.

Line 47: Define “compensation depth”. Also, “proceed” where?

As we explained in our answer to the 4<sup>th</sup> bullet point in RC1 we would abstain from using the compensation depth in our manuscript. However, we changed the sentence for clarification: “[...] can proceed sinking due to its higher in situ density compared to the surrounding water up to a depth with equally dense water.”, lines 58-59.

Line 44: state explicitly the “convenient property of potential density” you are referring to.

Added: “[...], namely having one reference pressure, [...]”, line 51.

Line 49 and 50: These two sentences together are very confusing. You are contrasting deep water mixing from wind forced downwelling under conditions of thermobaricity (ie “forced plume downwelling”) with “thermobaricity”. What is the difference? How are these not both “thermobaric effects”?

You are absolutely right, they are both thermobaric effects, but we did not consider the wind forcing as driving mechanism. Hence, the formulation with “instead of” was unfitting. We corrected this by changing the sentence to: “[...] we excluded forced plume downwelling while allowing for cabbeling to present the existence of thermobaricity controlled deep water mixing that is purely one dimensional and not driven by wind.”, lines 64-65.

Line 50: Who are “them”

We changed the structure: “While Killworth et al. (1996) used [...], Piccolroaz and Toffolon (2013) parametrized [...]. In contrast to them, we [...]”, lines 61-64.

Line 67: “temperatures” should be “water temperatures”

Added: “[...] surface water temperatures [...]”, line 88.

Line 75: “my” should be “by”

Corrected

Line 90: Tell us why it is ok to ignore the effects of local limnic chemistry that “must be included”

The reason for this is given in the two previous sentences. However, we also added: “[...] for a fully realistic representation.” in line 115.

Eqn 6: Highlight in the text that  $\rho_1$  is evaluated at  $p_2$ . This is key to the whole scheme and could easily be missed by the reader. This might also be a good place to say something about  $\rho_1$  (in situ) evaluated at  $p_2$  isn't really “in situ” anymore, but effectively potential density using a grid specific reference pressure.

We argued about this topic before and changed the manuscripts in section 2.2 correspondingly. (see also comments to RC1 above)

Line 118: “May” is misspelt

Corrected

Line 125-135: More information about the numerical scheme is warranted to help understand the results. What is the order of operations? From the text it looks like the surface boundary condition is updated first, then diffusion occurs, then stability is considered. If this is the order, say so. Are the diffusion and stability calculations done in an upward or downward sweep? Also, this would be a good place to explain why diffusion is needed in the model. What are the implications of neglecting it? How sensitive is the model to time step and layer thickness, which controls the effective diffusion, e.g. why is half the volume exchanged each hour?

We included several changes for this as mentioned above in this response letter: “We included diffusion to ensure exchange within the water column itself and include processes that are not directly driven by external forcing like cabbeling.”, lines 158-160; “[...] two layers, respectively. This comparison is done bottom up. If the stratification is stable, there is no action and the next upper layer is checked. However, if water in the lower layer is less dense than in the upper, the layers are mixed and the temperatures of the two layers are averaged. The mixed layers are then iteratively compared and mixed with all layers below in the water column until they are stable again. Then again, the next upper layer is checked. If the layers are unstable up to the temperature controlled surface layer, the temperatures of all corresponding layers are set to the prescribed surface temperature. By doing so, all

instabilities are removed from the whole water column in each time step. This stabilization takes place after the forcing at the surface and successive diffusion.”, lines 164-170.

Line 170: It is worth pointing out that “summer warming” occurs over 25 hours.

Even though the heating happens quite fast and into a depth that is similar to the measured profiles shown in Fig. 1b, we do not aim for highly realistic timescales regarding short term heating and cooling events but rather conceptual circulation patterns. Hence, we would abstain from focusing on specific time scales of some processes, since a more realistic model approach would be needed for this (e.g. vertical variation of the diffusion due to summer stratification).

Line 173: “WS2” I think should be “SW2”

Corrected

Line 188: Who are “They”?

Clarified: “These small rewarming events [...]”, line 226.

Line 195: I think you mean “SW3 and SW4” here

Corrected

Line 225: If breakdown of prior strong summer stratification is important, then results will be sensitive to the linear interpolation of summer temperatures from May through October. In particular the summer peak will be missed. Would it make a difference if you interpolated linear to an estimated summer peak surface water temperature?

In principle it would change the strength of the summer warming and could influence the following winter circulation similar to the warmer surface temperature scenario. However, we only wanted to demonstrate the conceptual behavior of the winter circulation regarding different surface water temperatures and not realistically represent the circulation changes. In general, including the summer peak (which is not included in our input data) would neither change the conceptual behavior of the circulation patterns nor the variability to warmer or colder surface water temperatures.

Line 225: “Strong winter period” is unclear

Changed to “cold winter period”, lines 278 and 279.

Line 233: I don’t understand what you mean by “every transition of maximum rho\_pot”

Added: “[...] maximum  $\rho_{pot}$ , which is equivalent to the surface water temperature crossing 3.98 °C.”, lines 256-257.

Line 248: “similar lakes” Similar how?

Clarified: “[...] (and other deep lakes with deep water temperatures below 3.98 °C) [...]”, line 291.

Line 266: What is the difference between “diffusion and vertical mixing”?

We tried to clarify this by adding “[...] due to instabilities in the water column [...]” in line 219 and changing lines 310-311 to “[...] dynamics were exclusively driven by diffusion induced cabbeling around  $T_{md}$  and successive vertical convective mixing controlled by thermobaricity below, [...]”. In addition we added “convective” for every occurrence of mixing that is driven by instabilities as well as indicating diffusion as driving force for the mixing where this is the case for better separation in section 3.2 and afterwards (see the manuscript version with tracked changes for detailed locations).

Line 277: “the depth of the crossing” is unclear

Clarified: “[...] depth of the intersection of the temperature profile with the  $T_{md}$  line determined [...]”, lines 321-322. We also now only use the term intersection throughout the manuscript for consistency.

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