Author’s Response Letter

Dear Editor and Reviewers,

we would like to thank for the helpful comments and we used them to further improve our manuscript. We really appreciate the invested time and the continuing interest in this topic. Following the recommendations, we reworked the current manuscript and tried to implement required changes and answer all concerns.

To reply to the comments of the editor and the reviewers (black) we inserted 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

19 Nov 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):
Dear authors,

Two reviewers have now evaluated your revised manuscript. Both reviewers still have significant comments that must be addressed in your next revision. To help guide your efforts, I propose that you focus on the following key areas:
Thank you for the feedback and providing guidelines for our revision.

A. Conceptual visualization: Provide a clear schematic diagram that illustrates the physical processes being modeled and demonstrates how each component of your model (diffusion, stabilization) contributes to representing these processes in the water column.
As requested, we added Fig. 2 as a conceptual model description and for describing the model workflow (Section 2.3.4). We use this figure to present the deep water circulation induced by vertical cabbeling at the T_{md} crossing and expanded the model description and its processes: Section 2.3.

B. Diffusion coefficient and sensitivity analysis: Both reviewers have expressed concerns regarding the diffusion coefficient used in your model. Please provide: (a) a clear explanation of how the reported value (5.5×10^{-4} m² s⁻¹) was obtained, (b) justification for this value, and (c) a sensitivity analysis examining how different diffusion coefficients affect your results, particularly the generation of convective mixing through cabbeling. An alternative is to have a section on “limitations and futur development” stating the current limitations (see also point 4)
(a) As request, we evaluated the effective diffusion in the model quantitatively which resulted in $D = \frac{l \cdot 0.5vl}{t} = 2.7 \cdot 10 - 4\ \mathrm{m^2 s^{-1}}$, with D being the diffusivity, l the layer size, v the exchange volume ($v = 0.5$), and t the time step size. We removed the scaled estimate.

(b) For justification we now use Fick's second law. We chose this value to ensure a good presentation of the different mixing phases as well as staying in a realistic range of the value. We also cited models using similar values: Piccolroaz and Toffolon, 2013; Wood et al., 2023, lines 185 and 500-503.
(c) We now have provided a detailed sensitivity analysis in Appendix A. Different diffusivity values do not change the behavior of the model or the cabbeling induced thermobaric circulation because cabbeling occurs even for only molecular diffusion. The amount of mixing occurring depends on the diffusion coefficient mainly because of its control of the intersection depth. But this depth is in reality influenced by different processes like wind induced turbulence and (de)stratification, which is why we did not aim for a realistic simulation of this depth only using diffusivity while still trying to not differ too much from reality. We discussed this in the new Section 4.2, lines 412-422, and also in Appendix A.

C. In situ density formulation and equation of state: Both reviewers remain uncertain about how your in situ density expression is derived and applied. Please also provide figures showing density profiles (both in situ and potential) before and after the stabilization algorithm, particularly at depths around TMD during winter stratification when cabbeling would be expected.
We tried to clarify our use of in-situ density in comparison to potential density and its correlation: lines 100-105. In the manuscript, we use the temperature profiles for visualization (Fig. 2) and mention that this is the best way to do so. Potential density does not reflect the pressure effects, while the change in the in-situ density from temperature changes is too small to be helpful for visualization (see lines 217-222). However, we added the required figures below: Figure R 1. Also, we added the description of how exactly we apply in-situ density for stability considerations in our model: Section 2.3.3.

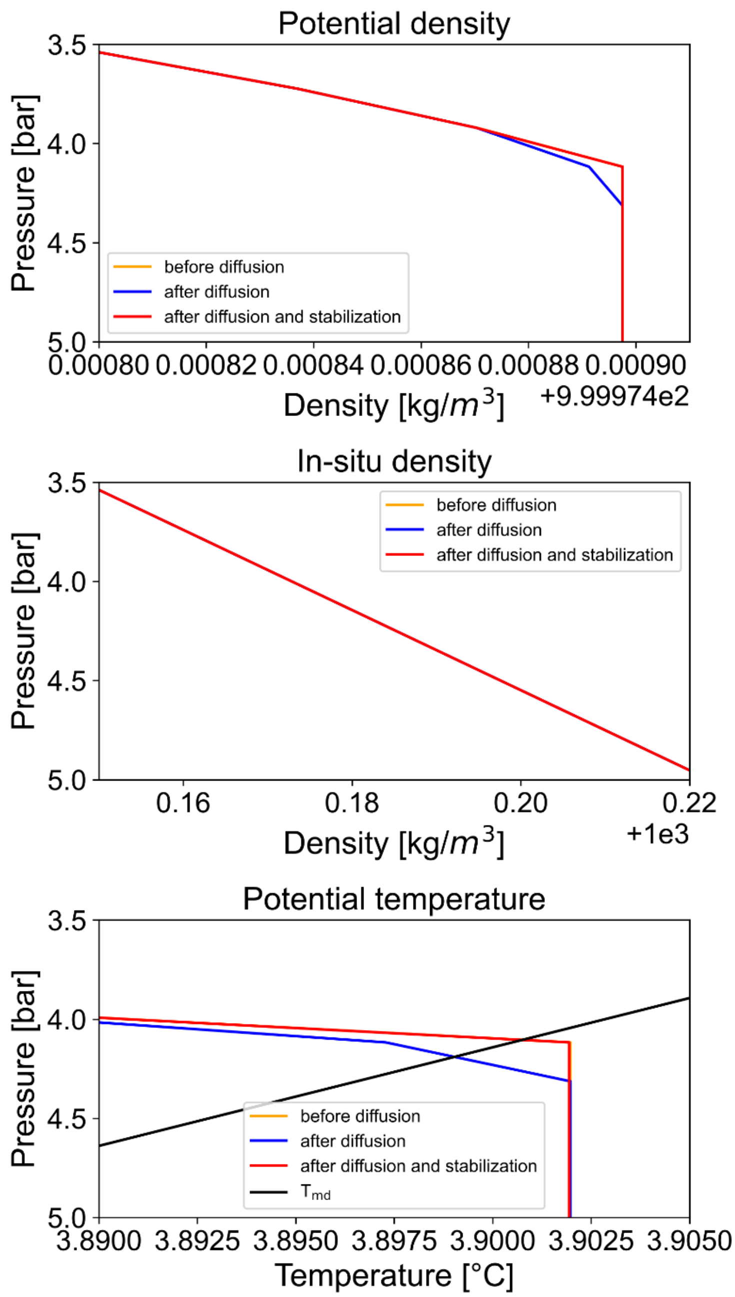

Figure R 1: Potential density, in-situ density and potential temperature profiles during one time step close to the depth of the intersection between the temperature profile and the T_{md} line. The profiles before diffusion (orange), after diffusion and before stabilization (blue), and after stabilization (red) are shown. Due to the stability check and corresponding mixing with the whole water column below the difference between the orange and red curves are really small, but the isothermal water below the intersection is slightly lighter/colder for potential density/temperature after stabilization. The differences in the in-situ density due to temperature changes are really small compared to the pressure effect and hardly visible (the last profile basically overlies the previous two profiles) but play a significant role for stability because the pressure effect is compensated for.

D. Numerical implementation of diffusion: The absence of a time-implicit solution for the diffusion equation is a major source of concern for Reviewer 3. I see two options for addressing this issue:

Option 1: Rework the code to implement a proper time-implicit formulation for solving the diffusion equation, as is standard practice in similar 1D vertical models. This would also allow you to properly prescribe diffusivity coefficients and apply mathematically rigorous boundary conditions.

Option 2: Add a comprehensive section explicitly addressing the limitations of your current averaging-based approach, including potential amplification of artificial mixing, and clearly outline the next steps needed for future work.
Additionally, please provide a more complete mathematical description of your model in the manuscript itself, situate your findings within the context of previous modeling studies on this topic.
Thank you for offering both options. We decided for option 2.
We added the stability criterion $D < \frac{l^2}{2t}$ (l being the layer size and t the time step) for explicit diffusion schemes and showed that our volume exchange approach remains safely within the stable region. See lines 180-182 and Eq. A4 in Appendix A. We also added a paragraph that discusses the limits of the applied diffusivity for a realistic simulation with variable diffusion coefficients and mention the benefit of implementing an implicit diffusion scheme for a more realistic model, lines 412-422.
We also improved the mathematical description of our model: Section 2.3 (Section 2.3.2 for diffusion); and added Section 4.1 for the discussion of previous models.

Further major changes:

- Additionally to the discussions about the diffusivity, we added a more detailed discussion of the current limits of our model and plans for future work in Section 4.2
- As requested by reviewer 3, we also implemented the density approach of TEOS-10. Results are shown in Appendix B. In the main body of the paper, we retained the pure water density (as the simpler and more direct approach for lakes) for consistency, as this paper deals with thermobaricity of pure water.

Report #1 – Submitted on 18 Oct 2025 by Anonymous referee #1

Round 2- Review of Thermobaric Circulation in a deep freshwater lake by Joshua Marks, Kazuhisa A. Chikita and Betram Boehrer.

Thank you for the revised version. The new edits address the majority of the points that required clarification, and the paper has significantly improved in terms of clarity and readability.
We appreciate the positive feedback.

- The discussion would benefit from a clearer explanation of how the proposed hypothesis differs from existing interpretations of deep-water ventilation. Specifically, the conclusion that deep water can be renewed independently of wind forcing contrasts with numerous studies that associate renewal with thermobaric instability - a concept not addressed in this paper - and its dependence on wind-induced dynamics.
  We thank the reviewer for emphasizing the need of better situating our findings. For this purpose, we added Section 4.1.

- The use of the term *in-situ* density is somewhat confusing. Linguistically, “in situ” refers to conditions in the original place, yet your description involves moving a water parcel relative to others. Moreover, in line 248 you note that the in-situ density (the original one) is dominated by compression and therefore provides little insight into stability.
  We added “[…] visual insight […]”, line 294.
  This inconsistency may be confusing, particularly for readers who are not familiar with deep-water physics. We accept that you are resorting to the term in-situ as a mean of distinction

of potential density, but as mentioned, this sentence goes back to the basic intuitive confusion. In equation 6, the paper switches from continuous to discrete functions, and the function with a discrete difference clearly shows that water parcel density is calculated between elevations 1 and 2. If the definition of in-situ density is the one given on line 103, this is not the commonly accepted definition.
We accept that perspectives of what is the proper terminology can vary (we understand it as where the parcel currently *is* not necessarily the original place). Most importantly, terminology must be consistent within a text and lie within the general use of words. Throughout this manuscript, we follow the common practice and use “potential” (temperature or density) for the conditions at atmospheric reference pressure (in general the density a water parcel *would have* at a reference pressure), while “in-situ” indicates the pressure condition at a certain location (“in-situ”; the density the water parcel *has* at given pressure conditions, which is the case for stability considerations since the water parcels are compared *locally*).
Even though, in-situ density is unable to visualize the stability it is still the needed quantity for checking for stability since for this process the water parcels actually are at the same pressure before they mix (due to infinitesimal small distances) and the in-situ densities have to be compared. Only due to the discretization it seems to be a kind of potential density.
We reformulated lines 100-105, added “[…] common local pressure […]” in lines 143-144, and added section 2.3.3 regarding this.

- It would be helpful to clarify, when considering the application of this model to commonly used commercial lake models, whether you recommend using the speed of sound approach instead of TEOS, as is typical in ocean models. Including this point in the discussion would strengthen the practical relevance of your work. Additionally, the conclusion would benefit from a more detailed description of the next steps required to implement this approach in commercial models. You mentioned that TEOS is relevant to the ocean, what is the basis of excluding it from usage in lakes, and is that a firm conclusion?
  The purpose of this paper is the demonstration of thermobaricity of water and how it affects/dominates the circulation in a natural water body. This paper is not promoting a model nor a model approach. We agree that TEOS-10 provides a much more comprehensive approach for thermodynamics and can also be applied to lakes (e.g. Pawlowicz and Feistel, 2012. We have written a model version using TEOS-10 and we present the results in Appendix B. Of course, TEOS10 is good enough to represent thermobaricity. However, in the main body of the paper, we retain the more exact and simpler pure water approach for consistency, as the paper deals with thermobaricity of pure water. We tried to clear this up in our manuscript and extended our discussion (lines 430-438).

- The choice of ±0.4 K remains unjustified. While it may not be intended to reflect climate change, it appears arbitrary, and thus the conclusion -that warmer water leads to fewer intrusions - feels self-evident.
  As mentioned previously, it is true that there is no justification for the exact values of ±0.4 °C. The values are meant to only conceptually represent possible variations of general winter intensities. We chose their magnitude to enable visible changes in the circulation patterns. Even though the conclusion may seem self-evident, we thought that these simulations would emphasize the dominating influence of the surface temperature input as well as the possible variation of the mixing intensities based on the current winter and the previous surface temperature history. Since this is, as the whole model, only supposed to be conceptually, we do not see the need of choosing highly specific values for specific years (if that would be seen

as an appropriate justification), since it would not benefit or change the conclusions and their presentation and maybe even create the impression of an intended realistic forecast.

- Since the study site is a caldera lake, it would be beneficial to discuss the potential influence of geothermal heat fluxes. These should also be acknowledged among the factors that were not included or explicitly represented in the model. It is totally understandable that you are not targeting this lake per se, but it still a factor that may affect stability, like salinity and wind.
  We mention geothermal heat fluxes in the introduction (line 75), model description (line 156), the discussion (lines 393-395 and 429) and the conclusion (line 445) with other external influences that we excluded since we aim for the demonstration of thermobaricity and how it affects/dominates the circulation in deep lakes without those external influences.

- It is unclear whether “diffusion” refers to turbulent or molecular diffusion. The stated rate suggests turbulent diffusion, yet you later indicate that turbulent diffusion is neglected. This inconsistency should be clarified. Also if no there is no wind forcing, it is represented through turbulent diffusion. (Lines 313-314)
  Yes, the reviewer is right: it is turbulent diffusion. We tried to clarify this throughout the manuscript. The ambiguous statement in the discussion has been removed. We clarified the connection of wind and turbulent diffusion (lines 409-411) as well as discussed the used turbulent background diffusivity (lines 412-422).

- You state that the timescale is not important; however, the model assumes that an unstable gradient will mix within one hour. Since the model is one-dimensional, this represents complete mixing rather than re-sorting (though re-sorting could, in principle, be implemented as in the Piccolroaz model). This assumption could substantially affect the results if realistic timescales were considered, even within a 1-D framework. It will represent unrealistically high convective velocities. Moreover, the model has not been validated and assumes that deep-water ventilation can occur without thermobaric instability—only thermobaricity—which warrants caution. The assumption of one-hour mixing also implicitly represents turbulent diffusion (or convective velocities), which you state is ignored. Instead of altering surface temperature alone, it might be more informative to perform a sensitivity analysis on the magnitude of the turbulent diffusion coefficient. Bottom line: the model may seem very fast in mixing compared to the actual process which merely depends on “diffusion” ?
  Correct, the model implements complete mixing instead of re-sorting. We clarified this in Section 2.3.3 and 2.3.4 lines 209-214. On purpose, we did not allow for deep water renewal (as e.g. in Piccolroaz and Toffolon, 2013). We wanted to demonstrate the circulation in cases when no paths are available for deep water renewal. It is the purpose of this publication to demonstrate thermobaric circulation beyond the partial deep water renewal. We use Lake Shikotsu as proof that cases like this exist. Sure, velocities are unrealistic, but this is anyway the case in models that are horizontally homogeneous (1D). Horizontal homogenization results in current speeds even more unrealistic than vertical homogenization (tens of kilometers versus hundreds of meters in one time step). However, in this paper we focus on the cabbeling inducing the deep water convection, not the convection itself. Hence, we listed the limits of the representation of the convection (4.2 lines 423-429).
  Contrary to what the reviewer says, the thermobaricity (and therefore thermobaric instabilities) of water is fully represented in our density approach. The term thermobaric instabilities was previously only used by others for the setting of wind induced displacement

of cold surface water (storm surges, internal waves). Nevertheless, vertical cabbeling also induces thermobaric instabilities (this was also mentioned by Weiss et al., 1991 in just one sentence, while also totally focusing on wind induced downwelling: page 666, left side, middle part) as our model shows. We added Section 4.1 for a better discussion of the differences between the cabbeling driven thermobaric circulation versus thermobaric instabilities.
Also, we improved the description of the model for a better distinction of the different processes of diffusion and mixing in Section 2.3.
We have added the turbulent diffusivity analysis in Appendix A.

- You mention that profiles parallel to the T_{md} line are stable (line 217), such as those observed at the onset of summer stratification. Is this apparent stability a result of the model lacking perturbations, and is it stable under diffusion?
  Those parallel part would also develop if perturbations were present (compare profiles of Lake Shikotsu). If a perturbation would shift the water upwards it would restabilize itself close to the T_{md} line, and if it would be shifted downward it would stay stable but be slightly displaced below the T_{md} line but still parallel to it. It is also stable under diffusion (as it is also present in the model) as long as the deeper water is colder than the upper water, in contrast to the winter stratification, which is the case in this period.

- The concept of vertically induced cabbeling by diffusion is novel and quite distinct from the classical notion of cabbeling, which involves horizontal mixing between two water parcels. In the vertical case, pressure effects - and thus thermobaricity - become relevant, as you also note. However, this concept, although frequently mentioned, would greatly benefit from further clarification and illustration in the discussion, perhaps with supporting figures or equations. Given its novelty and its central role as the main driving mechanism in your model, a more detailed explanation would strengthen the paper substantially.
  Thank you for emphasizing highlighting this finding. We accept that this was a shortcoming in the previous version. Hence, we added Fig. 2 and describe the phenomenon in the model description (section 2.3.4) as well as mention it in the conclusions (lines 455-457 and 460-463).

- The discussion would benefit from a comparison with previous modeling studies - at least the three or four existing ones on this topic -even if the present work focuses solely on isolating thermobaric effects. As mentioned earlier, this is a novel and potentially transformative approach that challenges several established interpretations. However, the current discussion is quite brief and mainly centered on the model itself. While it is understandable that you aim to isolate a new mechanism, doing so should not come at the expense of situating the findings within the broader context of previous literature and modeling efforts.
  We agree in general. However, our results do not challenge previous studies, since the wind induced downwelling is the more dominant process in the most cases. Instead, we want to add on this knowledge and emphasize the presence of the purely 1D process. For this we added Section 4.1 in the discussion.

Specific notes:

Line 13: The amount of semi columns ; is little bit confusing.
We changed it to numbering the key features: “circulation: (1) […], (2) […], (3) […], (4) […].”, lines 12-15.

Line 53: replace “neighbouring” with “vertically separated”
Done.

Line 59: you mention compensation depth without defining it.
We changed it to “[…] compensation depth, where it becomes denser than the ambient water, and from there it can proceed sinking due to its higher in situ density up to a depth with equally dense water.”, lines 58-59.

Line 95: Unclear what “it” is, and “after” what.
We added “[…] the T_{md} line […]” and “[…] after the end of the winter stratification.”, lines 92-94.

Line 155: specify that you are linearly interpolating between May 2024 and October 2023 to represent summer surface temperature
We added “[…] linearly interpolated from May 2024 to October 2023 to a duration of one year.”, line 169.

Line 213-218: Why are you only showing 24 hours of summer warming? How does the surface stratified layer evolve from WS4 to SW1? And why isn’t the bottom temperature evolving due to diffusion-driven cabbeling?
We only show about 24 hours because this is the first time the surface temperature surpasses the deep water temperature and aligns with the T_{md} line. From WS4 to SW1 the profiles look like SW1 but with colder surface temperatures and the convection cell at the surface erases the inverse winter stratification.
The bottom temperature is evolving due to cabbeling until SW2, where cabbeling stops as mentioned. Due to the small time between SW1 and SW2 the existing difference in the deep water temperature is hardly visible in the plot.

Line 314: Specify that Cabbeling is driving the deepening.
Cabbeling alone is not driving the deepening but rather the combination of ongoing cooling from above (driven by diffusion) and the cooling of the deep water by cabbeling induced mixing. We clarified this in lines 455-457: “The depth of the intersection of the temperature profile with the Tmd line deepens with time due to ongoing cooling from above and associated cabbeling induced mixing of the deep water and therefore determines the resulting deep water temperature and the lower extent of the circulation cell.”

Report #2 – Submitted on 13 Nov 2025 by Anonymous referee #3

Review of Marks et al. – HESS

I want to state that I did not take part in the first round of reviews, and aside from providing my own feedback, the editor asked me to assess the authors’ performance in the first round of revisions.

Presented is a conceptual study of thermobaric effects in a deep freshwater lake in the absence of wind forcing. The work introduces an expression for in situ density to account for compressibility effects at depth and uses it to examine the destabilizing effect of diffusion across $T_{MD}(p)$ as a driver of convective mixing. The authors’ model appears to reproduce this type of freshwater cabbeling during winter stratification, when it would be expected. However, the absence of a mathematical parameterization limits its applicability and generalization. The paper is also hard to follow because several ideas and methods are not adequately explained or supported by analyses/figures. That said,

I agree with the other reviewers and the editor that this is an interesting paper that addresses a relevant and understudied topic in deep, freshwater systems.
Thanks for the summary and this positive statement. We did not plan this model to be directly applied otherwise but rather present a general approach and physical phenomenon which pattern has not been described previously. We deal with the detailed points of critique below.

I do not see significant improvements over the initial version. The authors missed the chance to address much of the feedback they received, as reflected in the few changes made to the manuscript. Furthermore, I have rarely seen a paper returned with major revisions that does not modify any graphical displays (one reviewer explicitly asked for a schematic or at least a modification to one of the figures, which was not done). The authors' approach in the Open Discussion and revision seemed quite dismissive, particularly regarding comments about the choice of equation of state. Many responses were limited to underdeveloped arguments. Including calculations and figures to support those arguments would have strengthened the discussion. Overall, I would have liked to see more scientific engagement from the authors.
We try to improve our discussion and take more space. We took and take all comments seriously. Hence, if we not included the proposed changes directly, we looked for a compromise and included some of the new material in the appendix when it was required by the reviewer (e.g. as we deal with pure water TEOS-10 does not seem to be the appropriate choice. However, we added Appendix B regarding this.).

Below, I provide feedback and comments that may seem extensive for a paper in its second revision round. My goal is for the authors to engage with the proposed discussion and ultimately include some relevant points in a new section addressing the limitations of the study. I also strongly urge the authors to include a schematic drawing of the studied process, illustrating what occurs in the water column and highlighting how the model's different modules contribute to representing it. This will help the community in following your ideas.
We really appreciate the time and effort for this revision and the intention to improve the paper. We added the mentioned points: Section 4.2, Fig. 2, and Section 2.3.

Comments on TEOS-10 discussion

The gray text in this section details comments or viewpoints that I could not refrain from stating. But I would only expect a response and modifications concerning the part in black.
Thank you for your comments. We will answer on every comment since you mention fair points and we will also use them to improve our manuscript.

The authors are correct in noting that TEOS-10 is an instrument developed to deal with the properties of seawater. But I would like to stress that the work by Pawlowicz and Feistel's (2012) is highly relevant because it provides a method to extend the thermodynamically conservative framework of TEOS-10 to freshwater systems when accounting for lake-specific ion content. Expanding the study of freshwater thermobaric effects in environments with salinity gradients would be challenging without such a framework, and I believe there's a reasonable trade-off between possible loss of accuracy/precision and the advantages of a robust framework that enables consistent calculation of all relevant thermodynamic quantities.
We fully agree with your statement and appreciate the work of Pawlowicz and Feistel, 2012. We did not include their work in our manuscript extensively because it mainly focusses on salinity, while we explicitly wanted to exclude this quantity. However, their approach is

recommended if lake models plan to include salinity and use TEOS-10. We added citations at suitable places in line 398 and line 532.

The TEOS-10 thermodynamic equations have been validated for pure water, and the temperature ranges for cold pure water discussed in this manuscript are well within the 'oceanographic funnel' defined by McDougall et al. (2003 JAOT) and implemented in the TEOS-10 package. It may well be that Tanaka et al. (2001)'s density formulation for pure water is more accurate, but the differences from TEOS-10 are minor (see figure below), and I remain unsure whether that greater accuracy offers a real benefit for the manuscript's purpose (see following paragraph).

We know that TEOS-10 is also supposed to apply for pure water. Regarding the mentioned "oceanographic funnel" we disagree that the pure water values used in the manuscript are "well within". The funnel described by McDougall, 2003 strictly only includes salinity=0 at a pressure of 0 bar, but not for any higher pressures. We also see that the pressures in our manuscript are really small compared to the pressures used in the oceanographic funnel and we agree that the funnel can be justified to be applicable to our setting. However, Moreira et al., 2016 has shown the density differences for several freshwater lakes. Even though we agree, that the greater accuracy of Tanaka et al., 2001 (and Moreira et al., 2016, who uses Tanaka et al., 2001) is not necessarily a huge benefit considering the simplifications, 1) we see the benefit in using a much simpler formulation of the density and a more straight forward approach in calculating the density in lakes that is not based on oceanographic values. 2) We see an unnecessary complication in arguing that the ocean-based formulations are also applicable, which they certainly are with the help of e.g. Pawlowicz and Feistel, 2012, but without using the benefit of the completeness of the description of the equation of state and excluding essential parts of it like salinity. Using Tanaka et al., 2001 offers a much easier access for everyone to understand the physical phenomenon without focusing too much on theoretical aspects that are not crucial for the presented process. 3) We must remain consistent within our publication: referring to thermobaricity of pure water makes the use of pure water properties mandatory. Nevertheless, we also stated the possibility of using TEOS-10 as reasonable approach for implementing thermobaricity depending on the purpose and prerequisites in Section 4.2 (lines 434-435), the conclusion (line 467) and Appendix B. The conclusion out of this is that of course TEOS10 is also good enough for reflecting the thermobaric effects we detect using pure water properties.

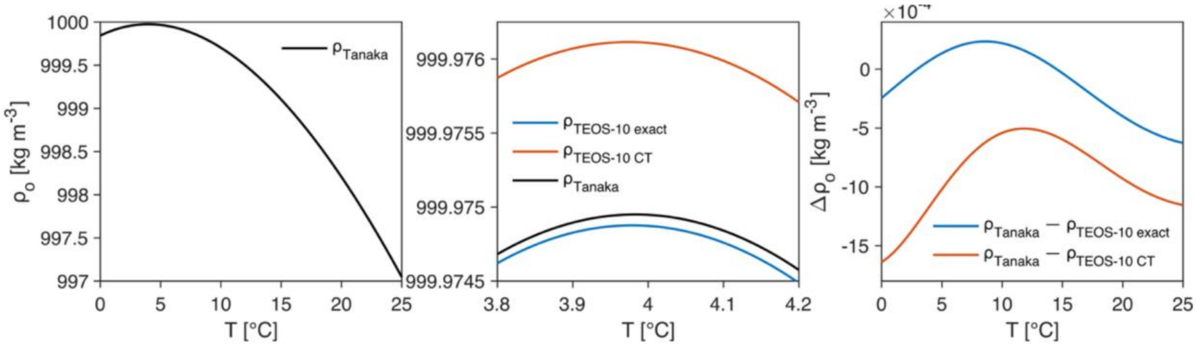

Figure 1: Comparison of Tanaka's with TEOS-10 densities at atmospheric pressure. The $\rho_{TEOS\text{-}10\ CT}$ corresponds to the density calculated from conservative temperature (CT) and absolute salinity (SA; in this case, SA=0) using the computationally efficient expression (75-term equation) for specific volume in terms of SA, CT and pressure (Roquet et al., 2015 OM). For this calculation, CT was obtained from in situ temperature and SA=0 at p=0, but the result is plotted as a function of the

original (in situ) temperature.
As mentioned above, we agree that the difference between the two formulations is not significant for the phenomenon described in the manuscript. But because of that we prefer to 1) use the much simpler formulation of Tanaka et al., 2001, 2) not use conservative temperature (see also comments below) to make it more accessible for limnologists, and 3) remain consistent within the paper.

Anyhow, the constructive criticism raised by the reviewers, in my opinion, has nothing to do with the accuracy of the density value at atmospheric pressure, but rather with the use of conservative temperatures for pure water, as defined in TEOS-10, which provides a thermodynamically conservative framework that could benefit the study. McDougall and Barker (2014 JGR: Oceans) argue that operating on a quantity like in situ density, which is neither potential nor conservative, causes unwanted nonconservative production or "artificial" mixing, with cabbeling being a known example (potential temperature will also pose problems). At the narrow range of temperature fluctuations in which thermobaricity may play a role in freshwaters, this artificial mixing could be significant.
We agree that mixing for example in-situ density would state problems if mixed as stated by McDougall and Barker, 2014. However, using it as stability criterion similar to the adiabatic levelling method, as we do and now clarified in the manuscript (142-144), is appropriate. As it is described in the manuscript, the potential temperatures are mixed, not the densities. Additionally, the potential temperature in our model is quasi conservative temperature. McDougall, 2003 states that potential temperature is not conservative because of the dependency of the heat capacity and total differential of enthalpy on salinity variations. However, we exclude any salinity in our model. Figure 2a in McDougall, 2003 shows that for a constant salinity, especially salinity=0, there are only small differences between potential and conservative temperature and essentially no nonconservative production of potential temperature during mixing. Also, we use a constant heat capacity and hence our potential temperatures are conservative (within the given accuracy). We discussed this in the paper in lines 131-134, 396-398, and 523-526 (Appendix B).
I understand that Piccolroaz & Toffolon (2013 JGR) correct for artificial mixing after their stabilization algorithm. In the present manuscript, the authors have not considered this type of correction. Since both their diffusion and stabilization (or mixing?) algorithms rely heavily on averaging, I am concerned that the cabbeling they report, although a plausible process, may be significantly amplified by their choice of equation of state. I strongly recommend discussing this potential limitation in the manuscript.
Regarding the stabilization algorithm of Piccolroaz and Toffolon, 2013: We do not see that they correct for the difference between potential and conservative temperature. As far as we understand, they use the in-situ temperature and correct for the adiabatic change when the densities of the parcels are calculated at the same pressure. Also, for the mixing they include the adiabatic change which accounts for potential temperatures and does also differ from the conservative temperature used in TEOS-10.
Regarding the influence of the equation of state on the principle of cabbeling: in our model the used density formulations are precise enough to realize diffusion induced cabbeling around T_{md}, especially since essentially no nonconservative production of temperatures occurs (see above). We discuss this in the manuscript: Section 4.2 lines 412-429 and Appendices A and B.

The buoyancy frequency described in the paper is nearly equivalent to the widely used "adiabatic leveling method" (see e.g., McDougall and Barker, 2014 JGR: Oceans, eq. 10 therein). This method, attributed to Millard et al. (1990 DSR), is the reference cited by Peeters et al. (1996) when defining buoyancy frequency, so I think it deserves mention. But most importantly, it is because eq. 10 in McDougall and Barker (2014) is equivalent to their eq. 1f using potential densities at the same locally referenced pressure, that the leveling method is adequate to calculate buoyancy frequency. Also note that strictly speaking, the furthest right-hand side of the present manuscript's eq. 6 is not directly equal to the rest of the equation. Using, e.g., i and i+1 indices instead would be preferable to avoid abuse of notation.

Thank you for pointing out this source. We fully agree to cite their work which we have done: line 144. We are also aware that this calculation uses densities at different pressures. With our use of the terms we stay with the formulation of saying "in-situ density at a specific pressure" since the water parcels are compared locally (see also answer on the 3rd bullet point in the comments of reviewer 1), which is equivalent to potential densities and therefore should not state a problem in usage. It is consistent with the mentioned previous work. We tried to clarify this in lines 100-105 and 142-144. We changed the indices in Eq. 6.

Proposed expression for in situ density

Working with in situ density is undesirable as this quantity is neither potential nor conservative.

In-situ density is the appropriate quantity to check for stability. However, mixing and diffusion is transporting temperatures (of course not in-situ density). McDougall and Barker, 2014 (as referenced by the reviewer above) state that the adiabatic levelling method is adequate to check for stability, which is basically what we do. We improved the wording: lines 142-144.

That said, my main problem is that I do not follow the way the authors calculate the integral in eq. 3. The provided information does not allow the reader to assess the deviation of the Taylor expansion around p=0 (eqs. 4 and 5) for depths far away from the surface. Since you have an expression for the speed of sound as a function of potential temperature and pressure, why don't you integrate eq. 3 numerically? Did you verify your derived expression against such an integral?

The calculation is straight forward: Insert Eq. 4 into Eq. 3 and do the integral. We made a linear fit, to prove to the reader that numerics do not import any structure into the model, that could be responsible for the observed circulation. We did compare the result with the formulation of Chen and Millero, 1986 (see Figure R 2) and did not see the need to do a numerical integration since the important characteristics (maximum density anomaly of water and thermobaricity) are well represented.

If I plot the in situ density of pure water for a potential temperature profile similar to your winter stratification data at great depths, I only see an increase in density with pressure (as in the Fig. 2 inset of Peeters et al., 1996). For 2 m bins in your model, I struggle to imagine any temperature fluctuation that would overcome such pressure dependency when analyzing the stability of the water column using the resulting in situ density.

Correct, the difference due to water properties is small compared to the pressure effect. But the fact that it is not visible on this scale does not mean that it is not present (see also Figure R 1). Still, using the adiabatic levelling method (see comment(s) above) the pressure dependence gets compensated for. Hence, the temperature fluctuations do not have to overcome the pressure dependency but instead the temperature differences change the compressibility (and therefore the pressure dependencies) and therefore the adiabatic change of the in-situ density which gradient needs to be compared to the in-situ density gradient of the ambient water as stability criterion: $\frac{d\rho}{dz} - \left(\frac{d\rho}{dz}\right)_{ad} > 0$ (stable) (e.g. Peeters et al., 1996 or Piccolroaz and Toffolon, 2013). We tried to improve our explanation in the manuscript: Section 2.3.3 and lines 217-222.

There are validated polynomial expressions for in situ density that can be used for pure water, assuming S = 0 (S_A = 0), such as Chen and Millero (1986 L&O) (or TEOS-10).

How does your in situ density expression (eq. 5) compare to that of Chen and Millero (1986; eq. 1 therein) at different pressures? Please provide a graph in your response.

Please be aware that Chen and Millero, 1986 is not good at low salinities. We are aware of the other polynomial expression like the one by Chen and Millero, 1986 (and TEOS-10 which we discussed above) but decided to use specific formulations for fresh water due to the results of Moreira et al., 2016. We did compare our results with Chen and Millero, 1986, see Figure R 2. As one can see, the differences at all depths are small (order of 10^{-3} kg/m^3). The important characteristic of a changing maximum in temperature with depth (termobaricity) is quite similar and absolutely sufficiently represented in our formulation for the purpose of the paper.

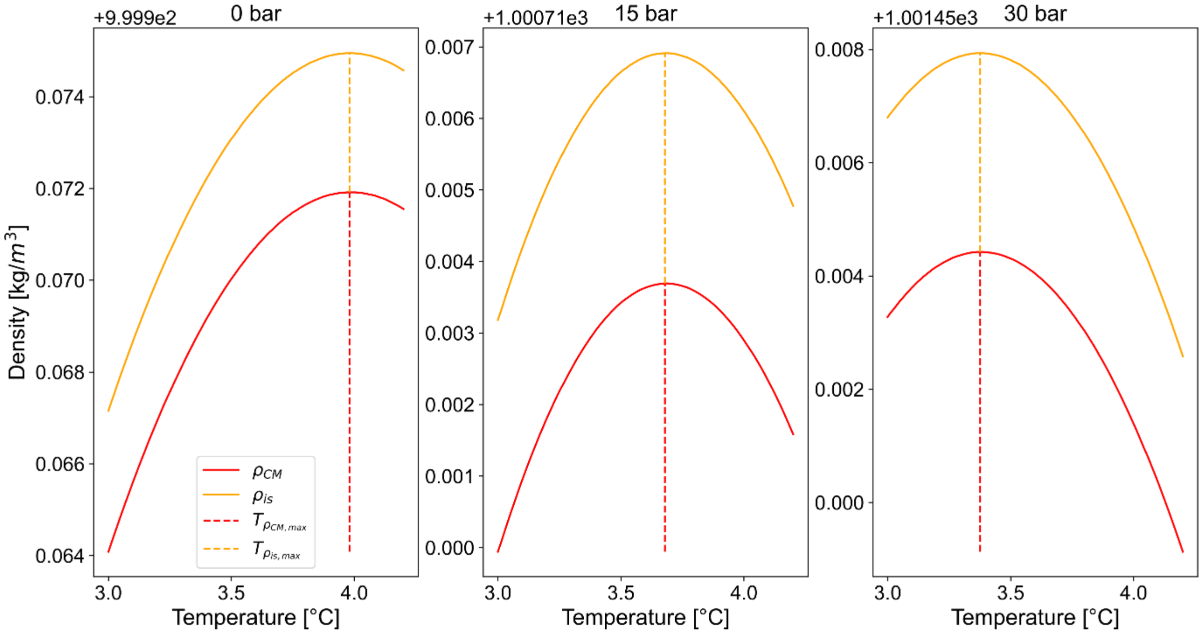

Figure R 2: Comparison of in-situ densities at different depths (0, 15 and 30 bar). The in-situ density formulation from our paper Eq. 5, ρ_{is} in orange, is compared with the in-situ density formulation from Chen and Millero, 1986, ρ_{CM} in red. Their corresponding maxima are marked by the dashed lines.

Following this comment, it would be of tremendous help for the reader if, in the paper, you show plots of in situ (and potential) density before and after the "stabilization" algorithm. Particularly at depths around T_{MD} when diffusion would be expected to generate cabbeling during the winter stratification. A waterfall plot showing several sequential profiles of in situ (and potential) density and potential temperature, before (e.g., dashed line) and after (continuous line) stabilization, with a constant time-dependent offset, could be informative for understanding what the model does.
Accepted: We included Fig. 2 conceptually showing the development of the potential temperature and explained it in Section 2.3.4. This shows what happens around the intersection of the temperature profile with the T_{md} line in one numerical time step. However, showing the entire density profiles does not help, as potential density is misleading for thermobaricity and should be avoided, in-situ density is dominated by the pressure effect (which makes the change hardly visible in general even though it is present): we added lines 217-222. The temperature profile change is also really small and can only hardly resolve a single step. It is necessary to accumulate the effect of several (many) time steps: please see Fig. 4. We present the proposed plots in Figure R 1 (see answer on editor comment C).

Numerical model

I really struggle to follow the modeling procedures because the manuscript provides no mathematical description of the model. It is nice that you shared your code, but that does not mean the reader needs to go through your code to follow your work. I understand that diffusion and convection are modeled sequentially, with the latter implemented via the stability algorithm (a schematic of the algorithm would be really helpful to the reader). However, the lack of a description of the numerical implementation made me wonder about diffusion modeling and boundary conditions (I'm guessing you intend Dirichlet at the surface and no-flux Neumann at the bottom boundary?).

The reviewer is right and we tried to improve the (mathematical) description of the model in the manuscript (Section 2.3 and its subsubsections) and added Fig. 2. We also clarified/added the boundary conditions: lines 155-156. For the bottom it is true that we excluded any flux but we did not specify a zero gradient which is why we would not call it no-flux Neumann.

Upon reviewing the code, I noticed that the diffusion algorithm implemented is a form of curve smoothing through averaging rather than a straightforward numerical solution of the diffusion equation. In my opinion, this approach lacks justification because (i) implementing a time-implicit solution of the diffusion equation is not difficult, and (ii) the prescribed diffusivity coefficient may significantly affect the development of an unstable profile due to diffusive transport across the T_{MD} as a function of depth. If diffusion is, for example, unreasonably amplified at every time step, there is no way to discern whether the model generates artificial mixing through diffusion-induced cabbeling.

Regarding the diffusion we again tried to keep the model as simple as possible and though of exchanging a certain volume (see Section 2.3.2 and Eq. 7) as an appropriate implementation of turbulent diffusion. (i) Even though it may not be difficult to implement a time-implicit formulation, we did not see the necessity to do so since the phenomenon of cabbeling and the following deep mixing would not change its characteristics due to it and an volume exchange method is simple to follow. (ii) In our understanding the applied diffusion mainly controls how deep the cold surface water in winter gets and hence determines the intersection of the temperature profile with the T_{md} line but does not influence the conditions for cabbeling to occur and the corresponding mixing due to instabilities. By only exchanging a certain amount each time step it is ensured that the exchanged water for diffusion is not more than the water in each cell which prohibits artificial generation or mixing based on this (we added the stability criterion for explicit diffusion schemes: lines 181-182). Additionally, water *always* mixes across the T_{md} line if the temperature profile intersects it due to diffusion, no matter its strength or kind of implementation, which is exactly the described phenomenon we are focused on. Hence, cabbeling induced downwelling always occurs except for a perfectly vertical temperature profile where diffusion (also the applied one) does not change anything in the temperature profile. It is true that the amount of diffusion would change the amount of cabbeling and hence also controls the deep water temperature but the phenomenon, that we want to emphasize, is not subject to change conceptually. And again, we do not aim for a realistic circulation model. We tried to improve the discussion on the diffusivity in Section 2.3.2, 4.2 lines 412-422, and in Appendix A.

Also, regarding (i), I am surprised this was not noted in the first round of revisions, and I firmly believe that validation against a numerical solution or benchmarking against a known theoretical 1D diffusion problem with a theoretical solution is largely missing. Previous 1D vertical minimal models, such as those from the group at the University of Trento, solve the diffusion equation numerically using a time-implicit method, and the authors should have built on those models for their work (the group at the University of Trento has many codes available publicly online).

Again (i): We explicitly did not want to do any benchmarking against other models since our aim is not to emphasize to use our model to accurately simulate lakes. It is meant to conceptually show the phenomenon of diffusion induced cabbeling with subsequent thermobaricity driven deep water circulation which previous has been overlooked as well as emphasizing the possibility of including thermobaricity in more elaborate and complete models, which should be used for accurate and more realistic modelling, with which our model does not aim to compete. For this we chose the approach of building a model as simple as possible from bottom up instead of using more elaborate models and reduce them to our purpose (which would be also fine since we believe that e.g. the model of Piccolroaz and Toffolon, 2013 also includes cabbeling induced thermobaricity driven mixing, but it is not visible due to the more dominant effect of wind induced downwelling).

We tried to clarify these points in the manuscript and extended the discussion with section 4.1, 4.2 and added Appendix A as mentioned above.

I acknowledge that the approach seems to work even without explicitly modeling diffusion as I would expect. However, I am uncertain whether cabbeling is adequately incorporated with the chosen approach. Providing more details, such as figures, showing what the model does at each timestep would be helpful. That said, the work could greatly benefit from implementing diffusion using a time-implicit method. This approach would also establish a mathematical framework for properly applying boundary conditions. Additionally, by testing how sensitive the simulation is to different prescribed background vertical diffusion coefficients, the authors could better assess the role of cabbeling instability in the generation of convective mixing.
We tried to improve the explanation of the model processes (see Section 2.3 and Fig. 2). See also the comment above. We agree that a variety of applied diffusivities would strengthen the driving role of cabbeling since it does always occur and the phenomenon does not change. Hence, we added Appendix A.

Reviewer 1 asked about the effect of changing the diffusion coefficient. I find the author's response deficient because they argue about something their current model is unable to assess, as it cannot prescribe the diffusivity coefficient. In the same response, the authors argue that "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." If this were true, then it would imply that the stability model dominates the generation of mixing. But, in the context of studying thermobaric effects, isn't the goal of a stabilization procedure to resort the water column following the adiabatic path and leave mixing to turbulent diffusion? Going back to the artificial mixing mentioned earlier, this statement raises more concerns than certainties.
The diffusivity can be indirectly prescribed in the model by choosing the exchange volume (and also the time step size as well as the grid size). We agree that this was not well enough communicated in the manuscript. Hence, we extended the model description (Section 2.3.2).
Our statement from the last response letter remains true. The included turbulent diffusion accounts for general mixing due to kinetic energy in the water column that is not based on instabilities. Instabilities are then also generating mixing, which is the deep mixing/circulation we are referring to. We try to improve the separation of those two different mixing types in the manuscript as we see a risk of confusion. However, the resorting the reviewer mentioned is mainly used for wind driven downwelling where larger water masses can intrude into the deep water. Here, mixing of the downwelling water mass is also considered (see e.g. Piccolroaz and Toffolon, 2013). In our model, cabbeling induces small instabilities that lead to only small intrusions of water into the deep water instead of plume downwelling. This is why we chose to simply mix the instable parts instead of resorting. Again, implementing downwelling and time-implicit mixing would enhance the realistic scales of the deep mixing, but would not change the general behavior (the deep water would only be filled up with cold water bottom up instead of getting equally colder by mixing, depending on the strength of the applied mixing). We added a discussion of this in Section 4.1 and 4.2 and the more realistic implementation of this deep mixing in a 3D model is currently in progress for Lake Shikotsu.

Regarding the model's diffusivity reported in the manuscript (5.5×10^{-4} m^2 s^{-1}), I guess this is an effective value calculated a posteriori, right? But could the authors mention how they obtain this value in their methods? Is this obtained using something like the flux gradient method from Powell and Jassby (1974 WRR) for the model output?
The reviewer is right that our diffusivity value was not explained and justified enough. Previously, we

estimated it by the fact that $D \sim \frac{dx^2}{dt}$ (D: diffusivity, dx: diffusion length/grid size, dt: time step), but unfortunately slightly underestimated the influence of the exchange volume. We are thankful for drawing our attention to methods like the flux gradient method to calculate it more consistently. We now applied a similar method using Fick's second law and corrected our estimation of the diffusivity value based on this (which only leads to minor changes: from 5.5×10^{-4} m^2 s^{-1} to 2.8×10^{-4} m^2 s^{-1}): see Appendix A.

I do not see why the authors cite an article by Saber et al. (2018), which has nothing to do with thermobaric convection, to support the reported value. Also note that a diffusivity value of 5.5×10^{-4} m^2 s^{-1}, although not excessively large, is still larger than the average oceanic diffusivity ($\mathcal{O}(10^{-5})$ m^2 s^{-1}; Waterhouse et al. 2014 JPO), so I would suggest discussing a bit more the reported value. What would be the resulting effective diffusivity if only the diffusion or stabilization module were to be active in the simulation?

We chose to cite Saber et al. (2018) because there a rather general estimation of the diffusivity was done. However, values used in thermobaric convection studies would better support this value, which is why we added sources with values from other more relevant lake studies: lines 184-185 and lines 500-503, 505-506 in Appendix A. We also extended the discussion for the diffusivity (lines 412-422 as mentioned above). For the background diffusivity value, we only accounted for turbulent diffusion and not for the mixing based on instabilities. With only the latter nothing would happen: starting at a homogeneous profile of 4.2 °C the whole profile would move to 3.98 °C when the surface temperature cools down and afterwards the controlled surface temperature value is the only remaining change in the model.

Finally, regarding the stabilization model, I would appreciate a discussion on why the authors consider it appropriate to prescribe the surface layer temperature for a large portion of the water column during stabilization. I can see how instabilities are removed, but wouldn't this alter the water column's potential energy in an undesirable way?

In case of instabilities at the surface, where the upper part of the water column is set to the surface temperature value, it could be thought of too large heat fluxes. To account for this, we used the measured surface temperatures to account for realistic heat fluxes. As one can see in the profiles from lake Shikotsu (Fig. 1) the upper water is almost isothermal in case of instability based mixing, justifying the surface temperature value for the mixed upper water column. We added the corresponding discussions in Sections 2.3.1 lines 167-168 and 4.2 lines 383-388.

Minor comments

1. Clarify potential misinterpretation in the title. The current version is an improvement, but the reader may still think that the paper covers wind-induced thermobaric instability. I'd suggest being explicit about the absence of wind forcing.
   We changed it to: "Thermobaric circulation induced by cabbeling in a deep freshwater lake: A conceptual 1D model" This highlights the presented process and distances from wind induced downwelling.
2. In Fig. 1, because of the color choice (bright blue versus pale tones) and display order, the lake's bathymetry stands out. While you argue that the lake's bathymetry justifies the 1D approach, I believe the most relevant part of the figure is the profiles measured in April 2005, which show T_{MD} crossing and a deep, nearly isothermal characteristic. Also, the map looks pixelated and sloppy.
   We improved the bathymetry map: see Fig. 1.

3 What is the point of Figure 2? If the goal is to show the stratification inversion, then it could be more targeted to the first 100 m. If you want to show what's happening across the whole water column, perhaps contour plots of $T–T_{MD}$ would be more appealing (the $T–T_{MD} = 0$ crossing during winter stratification should depict where cabbeling is expected – if pursued, a diverging colormap, pivoted at 0, would also be recommended).
The point is to show the extend of the temperatures in the vertical which demonstrates the recirculation of the water column during winter. We added "[…] indicating deep water circulation.", line 232. The inverse stratification is shown in Fig. 4. Although the proposed contour plots may be an option, we think the information would double with Fig. 5.

4 Line 98: "Potential density $\rho_{pot} = \rho_{pot}$ (T) is a function of (potential) temperature at a certain reference pressure". While I understand what the authors mean, I do not fully agree with this statement. I prefer the TEOS-10 definition "Potential density is the density that a fluid parcel would have if its pressure were changed to a fixed reference pressure in an isentropic and isohaline manner". The way these concepts are presented in the paper is somewhat misleading, since, theoretically, potential density is defined as an integral function of in situ density and compressibility. In the manuscript, it appears as if potential density comes first—I am guessing for practical reasons. Yet the wording of that section could be improved.
We agree with the definition of potential density mentioned by the reviewer. However, considering a formulation of in-situ density incorporating compressibility (and its temperature dependency), potential density is the in-situ density calculated at a certain pressure with potential temperature (in our model or conservative temperature in general, see comments above) (and the parcels salinity) accounting for the adiabatic (and isohaline) change (no integral needed): $\rho_{pot}(T)=\rho_{in\text{-}situ}(T, p_{ref})$. We see that the wording and the order in our manuscript was not optimal. We changed it, see lines 100-105. We left out the isohaline part, as we generally exclude salinity in the manuscript.

5 An isothermal initial condition of 4.2 °C for potential temperature in the model would imply that the surface and bottom in situ temperatures differ slightly. Is this the intended setup?
The constant potential temperature profile is intended to demonstrate that thermobaric conditions develop in the model without preconditions of thermobaric stratified profiles. So yes, the constant potential temperature profile is intended because it is compared to the T_{md}.
I'd really like to see in the paper the Salinity of profiles shown in Fig. 1b (or was conductivity below the detection limit?). While I fully understood that the paper does not deal with salts, it would help to clarify the isothermal characteristic shown in Fig. 1b, which is then used as the initial condition for the model. Also, the data shown in Fig. 1b is expressed as potential temperature, right?
We added the salinity profiles in Fig. 1 (Fig. 1c), but Fig. 1b is not used because of or to represent the isothermal deep water in the measurements from Lake Shikotsu.
Since the use of potential temperature is clarified later in the manuscript, I suggest noting the use of potential (or in situ) temperature in the labels and caption of Fig. 1b. Using more common notation like θ and ρ^{θ} for potential temperature and potential density, respectively, would help avoid confusion (e.g., Peeters et al. 1996)
Since we now clearly state which temperature we are referring to we think it is not necessary to change the notation in the manuscript as potential temperature is often used and referred to in limnological papers as T as well.

6 There are two recent papers by Eddy Carmack on thermobaric effects in cold, deep lakes that I think are worth mentioning.

- Carmack, E., & Vagle, S. (2021). Thermobaric processes both drive and constrain seasonal ventilation in deep Great Slave Lake, Canada. Journal of Geophysical

Research: Earth Surface, 126, e2021JF006288. https://doi.org/10.1029/2021JF006288

- Carmack, E., Vagle, S., & Kheyrollah Pour, H. (2024). Seasonal temperature and circulation patterns in a hybrid Polar Lake, Great Bear Lake, Canada. Journal of Geophysical Research: Earth Surface, 129, e2024JF007650. https://doi.org/10.1029/2024JF007650

We added them in line 43.

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