Answer to:

RC3: 'Comment on egusphere-2025-1195', Anonymous Referee #1, 03 Jun 2025

Many thanks for your input. It will surely improve the manuscript.
(The original comment in greyed out and italic and our response is black)

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 the 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 the short summary 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?

For pure water in the limnic temperature range, the appropriate density formulation is either Tanaka et al. (2001) or Kell (1975). The compressibility of pure water in the limnic range is most accurately concluded from sound speed tabled by Belogolskii et al. (1999). The special feature of Chen and Millero is the inclusion of salinity in their density formulation. However they base the salinity on ocean composition and as a consequence the density contribution of salts is badly underestimated for most freshwater lakes (see Moreira et al. (2016)). Hence Chen and Millero is not the appropriate approach for our model set-up. We use the proper approach for pure water.

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 ("cabbelling") 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.

You are right, we will improve the distinction between the different instabilities to clarify the processes identified in our model. Regarding the cabbeling, we will clarify that this is the initiating process that induces the downwelling at the intersection of the T_{md} line with the temperature profile, and separates the deep downwelling from the surface circulation, while thermobaricity sustains the downwelling into the deep water.

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 will add the depth of the surface water temperature sampling. We resolved the diurnal cycle to include the lower temperatures at night and the higher temperature during the day.

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 provides the vertical length scale of processes. Smaller values would still provide a similar picture but on a smaller vertical scale (and hence also smaller temperature differences as a consequence of thermobaricity). We used a reasonable value and conclude that at some locations (times) it is too small and at other locations (times) it is too large. However, we refrained from using varying values (in time or space) to exclude the possibility of circulation or resulting temperature profiles possibly being created by this. Diffusivity was added, because otherwise the entire temperature profile would be pulled onto 4 °C and remain there, except the surface layer following the implemented surface temperature.

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(T1,p2) 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 drho/dp (or c or 1/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 are absolutely consistent in our density convention: potential density is rho(T,p_0) where p_0 represents a pressure reference, which is kept constant over the model domain in space and time. In-situ density is rho_{IS}(T,p) at any pressure p. Maybe we should write this out somewhere, but we thought this is textbook knowledge.

Minor/editorial comments

Title: should include words like model and 1D.

We did not use the word model in our title so far because we do not want to give the impression of creating a realistic model for a lake but rather give a conceptual impression of the thermobaricity driven deep water circulation. But we will think about optimizing our title.

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.

Our key outcome is mentioned in the last sentence but we will check what other results to add in more detail in the abstract.

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)

Thermobaricity is explained shortly after (line 32and following) it was mentioned first, but we will think about optimizing the sentence.

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 …"

We will check this.

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 will try to add information here in the revised version of the manuscript.

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

We will clarify this.

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

We will check this, see also above.

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 right, both are thermobaric effects. Our formulation here is not optimal and we will change this.

Line 50: Who are "them"

"Them" are the previous mentioned models of Killworth et al. (1996) and Piccolroaz and Toffolon (2013). We will change this in the manuscript.

Line 67: "temperatures" should be "water temperatures"

We will change this.

Line 75: "my" should be "by"

We will change this.

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

We ignore salinity in our considerations (lines 90 / 91)*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_(in situ) evaluated at p_2 isn't really "in situ" anymore, but effectively potential density using a grid specific reference pressure.*

We will check the description. Of course, it is possible to calculate in-situ density of a water parcel without taking it there (see definition of in situ density).

Line 118: "May" is misspelt

We will change this.

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?

The order you mentioned is correct, we will add this. The diffusion is done simultaneously for the whole water column, while the stability is checked bottom up. For the latter the unstable layers are mixed downwards again so that only one stability check generates a stable water column. The influence of the diffusivity is explained above. We will check how to improve the manuscript here.

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

We do not aim for a realistic model (neither in time nor space) and we do not use a realistic representation of the diffusivity in time and space (compare above). Therefore, we would refrain from pointing this out since it has neither a specific implementation in the circulation characteristics nor a realistic basis.

Line 173: "WS2" I think should be "SW2"

We will correct this.

Line 188: Who are "They"?

The small rewarming events. We will improve this.

Line 195: I think you mean "SW3 and SW4" here

We will correct this.

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?

It would change the strength of the summer warming and could, regarding the strength, influence the following winter circulation as explained in the text. However, since we do not have a realistic representation of a lake with this model and only aim to conceptually show the influences of the different surface temperatures the usage of the linear interpolation is sufficient in this case. Using the summer peak (which is not included in our input data) would not change the in the manuscript described behavior.

Line 225: "Strong winter period" is unclear

We mean a colder winter. We will clarify this.

Line 233: I don't understand what you mean by "every transition of maximum rho_pot"

This is every time the surface temperature crosses the temperature of maximum density at the surface, about 3.98 °C, where the potential density is at its maximum.

Line 248: "similar lakes" Similar how?

Deep lakes with deep water temperatures below 3.98 °C during summer stratification.

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

We mean the vertical mixing induced by instability. We will clarify this in the manuscript.

Line 277: "the depth of the crossing" is unclear

The crossing of the temperature profile and the T_{md} line. We will clarify this in the manuscript.

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