

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.

- 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.
- 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. 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.
- 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 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.
- 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.
- 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 there is no wind forcing, it is represented through turbulent diffusion. (Lines 313-314)

- 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” ?
- 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?
- 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.
- 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.

Specific notes:

Line 13: The amount of semi columns ; is little bit confusing.

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

Line 59: you mention compensation depth without defining it.

Line 95: Unclear what “it” is, and “after” what.

Line 155: specify that you are linearly interpolating between May 2024 and October 2023 to represent summer surface temperature

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?

Line 314: Specify that Cabbeling is driving the deepening.