

Author's Response Letter

Dear Editor and Reviewer,

we are very thankful for the time and effort put into this manuscript that largely helped to improve it.

To reply to the comments of the editor and the reviewer (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 3.

Sincerely,

Joshua Marks and Co-Authors

02 Mar 2026

Editor decision: Publish subject to minor revisions (review by editor)
by [Damien Bouffard](#)

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

Dear authors,

As you can see, the reviewer is satisfied with your responses and the updated manuscript. The reviewer has suggested a few minor edits, which should not require significant effort. I will not be sending the manuscript out for further review.

Best

Thank you for the feedback and effort put into the revision. We addressed the remaining points of the reviewer and also added the acknowledgements as well as created a DOI for the published code and changed that in the code and data availability and added it to the references.

Review of Marks et al.– HESS

I thank the authors for the work they put into addressing my comments. The sensitivity analysis and the TEOS-10 model version included in the Appendix are valuable additions. Please ensure the code repository is updated accordingly.

[We appreciate the positive feedback and are thankful for the helpful comments. The code repository is updated.](#)

I only have some remaining minor comments, listed below in roughly decreasing order of importance.

- 1 Example of stability criteria in action. Thank you for including the new equation 8. This is an important aspect of the paper that was severely diluted in the previous versions. Figure R1 in the response letter shows that the in-situ density at the macroscale responds only to pressure. This was a point stressed by various reviewers in previous

revisions. The stability check established by eq. (8), I believe, is the missing key aspect to guide the reader in following your approach. Please include some examples of density gradient profiles in the main text that do not satisfy eq. (8) (with the x-axis on a log scale if needed), accompanied by the before and after stabilization of potential temperature (e.g., last panel in Figure R1). This will help the reader understand how the model is designed to handle the water-column information.

We think showing density gradient profiles will not aid the understanding of Eq. 8 because of the minimal differences (which hardly can be visualized). Visualizing the density gradients for the whole profile is complex and potentially misleading in assessing the stability. More appropriate for visualization would be the stability frequency (see Fig. R a below), which is a similar visualization of the stability check. Here, the instability after diffusion and before stabilization is easily visible. We did not add this Figure to the manuscript because we think the information in this figure is not enough compared to the text to justify an additional figure. Additionally, the change in potential temperature for the same time steps as requested by the reviewer is already presented in the manuscript in Fig. 2.

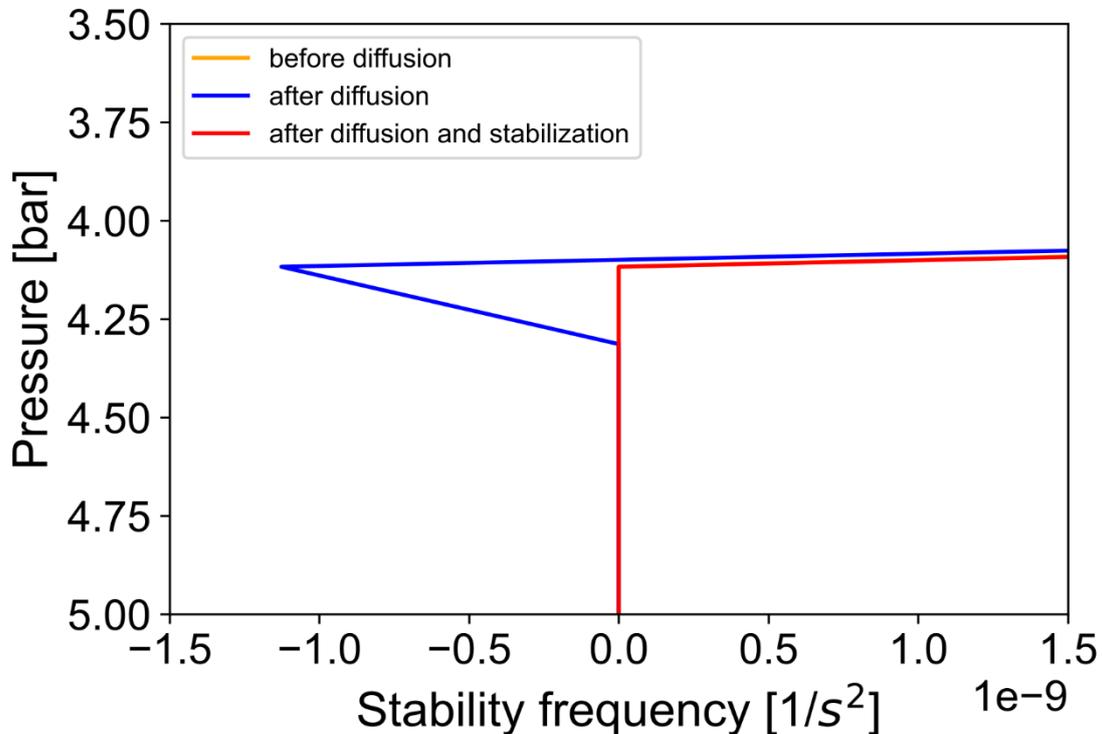


Figure R a: Stability frequency during one time step close to the depth of the intersection between the temperature profile and the T_{md} line. The profiles before diffusion (orange), after diffusion and before stabilization (blue), and after stabilization (red) are shown. Due to the stability check and corresponding mixing with the whole water column below the difference between the orange and red curve is 0 below the instability occurring in the blue profile due to stabilization. Only directly after diffusion a negative stability is present around T_{md} which then induces the deep mixing.

- 2 Neumann no-flux boundary condition. I understand the authors chose not to add more complexity with the bottom boundary condition, as it does not significantly affect the current case due to the lack of geothermal flux and its distance from the surface where the forcing is applied. However, I encourage the authors to reconsider it. In a 1D explicit numerical scheme, a no-flux boundary condition is very easy to implement, and, most importantly, if the bottom were closer to where the temperature profile intersects the T_{MD} , an incorrect implementation could cause more serious problems. We know the authors want to keep things simple and that the paper is only inspired by Lake Shikotsu,

but it is not necessarily realistic, etc., etc. However, since the title includes the word “conceptual”, I see no reason to have this specific aspect of your numerical method conceptually wrong in purpose :-)

As mentioned in the previous response letter we already applied a no-flux condition in the model by not allowing any exchange between the water column and the lake bottom. We now explicitly mention the no-flux condition in the manuscript: „[...] while we did not allow any exchange at the lake bottom (no-flux boundary condition) to exclude geothermal heating and subsurface flows.”, lines 156-157.

- 3 Effective diffusivity. Appendix A clearly explains how you obtain the background diffusivity. But I am still wondering about the combined effect of the diffusion and stabilization modules. The flux-gradient method of Powell and Jassby (1974, *WRR*), as noted in the previous revision, can be useful for calculating this effective diffusivity. Since the background diffusion is already relatively high (the average oceanic diffusivity is $\mathcal{O}(10^{-5})$ m² s⁻¹; Waterhouse et al. 2014 *JPO*), I would appreciate it if the authors verify that, during periods of active diffusion-induced cabbeling, including the effects of the stabilization module, the effective diffusivity stays within a reasonable range.
We specifically want to abstain of including the cabbeling induced deep water circulation within the effective diffusivity since the clear separation from diffusion is the purpose of this publication. As in our case the convective cell extends over most of the vertical domain, a diffusivity approach is not appropriate. Additionally, if we would include the convective mixing in the effective diffusivity the same limitations of the stability algorithm, which we discussed in Section 4.2, would come into play and would distort the resulting value of the effective diffusivity (if it is desired to include instability driven convection in there in the first place).
- 4 Concept of exchange volume. I now understand how the authors treated diffusion in their numerical approach. I see why you call it exchange volume, although it is a dimensionless parameter, but isn't it more like an exchange fraction of the volume defined by the grid discretization?
We changed it to exchange fraction in the manuscript and changed the first introduction to “[...] v being the exchange fraction, the exchanged part of the volume of each layer.”, lines 179-180.
- 5 Figure 1. Sorry for the inconvenience, but many readers might not be familiar with the location of Lake Shikotsu in Japan. Including a (non-pixelated) map of Japan showing the lake's location, as in the previous version, would be helpful. Also, a scale bar would be useful. I would suggest a less shiny colormap and a smaller colorbar, so the profiles from (b) and (c) stand out better. Finally, is the presented conductivity the raw conductivity? If so, could the authors present specific conductance instead? Normalizing at 4°C (e.g., Carmack and Vagle, 2021 – *JGR:ES*) would be desirable given the low temperatures discussed in the paper. [You may find a reference to your own papers in the way Carmack and Vagle normalize conductivity to 4°C, which I am not totally sure is a correct interpretation].
We improved Fig. 1 correspondingly.
- 6 Figure 2. I suggest the authors correct this figure to display depth increasing downwards.
Done.

- 7 Figure A1. Shouldn't the ylabel be "pressure [bar]"?
[The reviewer is right, we corrected this.](#)

- 8 Figure A2. To be consistent with the rest of the paper's figures, please change the ylabel units from [m²/s] to [m² s⁻¹].
[Done.](#)