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.

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.

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 :-)

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.

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?

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].

6 Figure 2. I suggest the authors correct this figure to display depth increasing downwards.

7 Figure A1. Shouldn't the ylabel be "pressure [bar]"?

8 Figure A2. To be consistent with the rest of the paper's figures, please change the ylabel units from [m^2/s] to [$m^2\ s^{-1}$].