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Hydrology and Earth System Sciences Discussions
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Interactive comment on "Latitude and bathymetry modify lake warming under ice" *by* Cintia L. Ramón et al.

Anonymous Referee #2

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By application of a circulation model in an idealized domain mimicking the heating of an ice-covered lake of irregular morphometry by solar radiation, the authors arrive at an insightful demonstration of the rotation effects on the radial density flows produced by differential heating between shallow and deep lake areas. Rotational gravity flows are widespread in geophysical fluids and an advance in their quantification makes a valuable contribution to earth and planetary fluid dynamics. The ice-covered lakes represent rare natural examples, where these flows can be observed and investigated in detail at their whole range of scales, undisturbed by more energetic flows, usually persisting in open water, oceans, or the atmosphere. In that sense, the authors discuss an intriguing problem, of interest for a wide research community. The modeling methods are relevant, and the results are presented in a well-structured way.

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I had the opportunity to read the previous comment and generally share the concerns of the Reviewer1: my major criticism refers to the weak connection of the model configuration to the real conditions met in lakes and, as a result, misleading, superfluous, and over-generalized conclusions made by the authors.

Instead of nondimensionalization of the problem with regard to the rotational forces prior applying a numerical model, the authors voluntary choose the domain dimensions of $\mathcal{O}(10^2)$ m and vary the Coriolis parameter within 2 (!) orders of magnitude. It is left for the reader's inspiration to imagine where on Earth $f = \mathcal{O}(10^{-2})\ s^{-1}$ can be observed (Line 139, Table 1). By using *a postreriori* re-scaling based on the Rossby number (Eq. 4, Line 148), a conclusion can be drawn that the ageostrophic regime ($Ro = \mathcal{O}(10^{-1})$, Fig. 3, first column), similar to that described by Ulloa et al. (2019), can be found only in small ponds with an area of several ha. In lakes with characteristic length scales of $\mathcal{O}(1)$ km ($Ro = \mathcal{O}(10^{-2})$, Fig. 3, second column) and longer ($Ro = \mathcal{O}(10^{-3})$, Fig. 3, second column), the shallow near-shore areas are effectively decoupled from the lake interior by rotation. The modeling results do not however provide a final proof for the importance of differential heating even in small ponds: they are typically much shallower than the modeled domain and have the background mixing intensities higher than those adopted in the model (Lines 108-111).

Herewith, the following outcomes of the study must be made clear: 1. For *the vast majority of ice-covered lakes*, differential heating does not contribute to the vertical mixing in the lake interior. 2. The previous findings of Ulloa et al. (2019) must be reconsidered taking into account the new results. 3. All variations of the Rossby number should be clearly related to corresponding variations in lake horizontal dimensions. Any mentioning of latitudinal effects should be removed, since for all seasonally ice-covered lakes $f = \mathcal{O}(10^{-4})$ s^{-1}.

Other remarks:

L68 The geometrical factor G (Eq. 1 and Eq. 8) is of little predictive power as long

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Discussion paper

as the hypsometry (the shape of the basin) is not included. When derived in a strict way, G incorporates a "shape factor" $S = 0..1$, which is found as an integral $S = \int_0^1 D(x)dx$, where $D(x) = 0..1$ is dimensionless depth, $x = 0..1$ is the relative distance from the shore to the lake center. For vertical walls $S = 0$, for linear slope $S = 0.5$, for the typical "bowl"-shaped lake $S \approx 0.3$, and for the authors' $tanh$-approximation $S \approx 0.6$. Hence, application of uncorrected G to different basin shapes can lead to ≥ 2 times differences in the result. Removal of G and related discussion is *strongly* recommended unless the basin shape is incorporated in the scaling.

L131 $1/\lambda = 2.5$ m^{-1} (< 1 m Secchi depth) is rather turbid than moderately clear and is not typical for the majority of ice-covered lakes. Would the differential heating increase in more transparent waters? How the transparency affects the rotation effects? Make it clear in the text.

L202-206 The geostrophic balance does not hold true in the bottom boundary layer (BBL), and the Coriolis effect on bottom-slope currents is strongly reduced by bottom friction. How good is BBL is reproduced by the model?

L314 Avoid the term "fjord-type" lakes, because the effect of the non-unity horizontal aspect ratio was not investigated in the study,

L320 "...Peruvian Andes ...": Any example of a seasonally ice-covered lake at latitudes below 15^o? A lake at 20^o lat or lower can develop ice cover only at altitudes where liquid water is extremely rare. The whole discussion on the latitudinal variability is vague and should be rethought in terms of lake size (see above).

L321-323 The sentence is unsupported and — to be straight — wrong and misleading. Has to be removed.

L330-332 It is quite an interesting point deserving more discussion in view of the presented results. If the littoral water temperatures reach the maximum density point (TMD),

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but the lake interior stays colder than TMD, the ice cover will quickly melt over the shallows, forming the well-known "moats". As long as the rest of the lake stays ice covered, water temperatures in these open areas will be close to TMD without long-lasting stable stratification. "Moat" formation has been traditionally referred to terrestrial heat fluxes; the role of heat capture by rotation was never considered in this context but deserves a closer discussion.

Presentation in form of a HESS publication of the otherwise well-performed and insightful study to a wider community can be recommended *only* after resolving the above-mentioned issues.

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