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Interactive comment on "Non-hydrostatic effects in the Dead Sea" *by* Oded Padon and Yosef Ashkenazy

Anonymous Referee #2

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Dear Editor, Please find my review on the paper "Non-hydrostatic effects in the Dead Sea", Padon and Ashkenazy, submitted to the OS.

The aim of this paper is to investigate the role of non-hydrostatic processes in the hypersaline Dead Sea through a series of numerical simulations using the MITgcm. The model is run on three computational grids with horizontal resolutions of 400, 200, and 100 m and surface forcing consisting of idealized annual and diurnal forcing through relaxation of surface temperature and salinity, and either constant or diurnally varying winds. The model is run in both hydrostatic and non-hydrostatic modes and the results compared. For the 400 and 200 m grid simulations the results are nearly identical. In the 100 m grid simulations the only significant difference occurred at night in the winter diurnal forcing simulations where the non-hydrostatic model maintained an unstable

stratification across the northern two-thirds of the lake for several hours before dawn with the upper water denser than the deep water by as much as 0.05 kg m-3. In the corresponding hydrostatic simulation this instability was restricted to a much smaller region with values of less than 0.001 kg m-3. These results may be an interesting curiosity but they appear to be very minor and restricted to very specific and limited situations and therefore it is doubtful that this non-hydrostatic phenomenon plays a significant role in determining the overall circulation in the Dead Sea. The manuscript contains several serious flaws. If the authors wish to convince the reader that these non-hydrostatic effects are responsible for maintaining unstable stratification for many hours they need to prove that such events really occur in this lake and then provide a more convincing explanation. In figure 11 the authors present data that is supposed to support the claim of existence of colder and denser surface water overlaid warmer water for hours, however the authors admit (in the figure caption) that the cooler surface water can be due to river runoff that keep the surface water diluted, cooler and less dens than the underlying brine. Thus it seems that fig 11 cannot be considered as supporting evidence, unless positive evidence of both salinity and temperature point that an unstable situation really exists. Figures 9 and 10 present the time series of the significant differences between the hydrostatic and non-hydrostatic simulations. The main difference is the instability and convective mixing of the dense plume in the early morning hours. On one hand the authors suggest that this is a feature that appears to be unique to the non-hydrostatic simulations. Yet in Figure 7 they indicate that it also occurs, albeit to a lesser extent, in the hydrostatic simulations. The location chosen to assess this effect is the center of the lake, however it would make more sense to compare the density difference time series at a point in the northeastern part of the lake where apparently the hydrostatic simulation also produces this instability. Regarding the experimental setup, in the 100 m runs the do not use the convective parameterization that was used in the 400 and 200 m runs, claiming that the 100 m resolution should be fine enough to explicitly simulate convective mixing. Furthermore in the 400 and 200 m grid simulations they use the simplest scheme available in the model (implicit vertical diffusion). One would expect including 100 m grid simulations that include the convective parametrization in order to be able to properly compare these results to the coarser grid results. It is unclear why switching off the convection scheme in the 100 m non-hydro run while leaving it turned on in the hydro run? This raises serious doubts about demonstrating conclusively that the effects they are seeing are physical or just an artifact of the convection scheme. The choice of the case of the Dead Sea for exploring the non-hydrostatic effect is not clear, the there are many narrow and deep lakes on earth, which experience winter convection driven by surface cooling. These freshwater lakes are much simpler to explore, without the added complexity of the Dead Sea. Anyway, a proper validation of the model results that unstable density structure can remain for the entire nighttime is missing in this paper. Minor commnet: Page 3, lines 20-25 talk about the complications and computational expense of running non-hydrostatic simulations. It is unclear what the message in this paragraph is? Line 30 - only 15 levels in the vertical are used, this is very coarse, with the upper five levels having thicknesses of 5 m each. While this may be ok for the winter when the stratification is weak to nonexistent, it seems to be very problematic in the summer.

Page 4, line 5: with an annual cycle in the forcing the model will never reach a steady state. It may reach a repeating annual cycle, which was not demonstrated.

Page 5, lines 16-17: on what basis do the authors pick the restoring time scales for the surface forcing of 12 and 4 days for S and T respectively? This is especially problematic for the diurnal cycle experiments. Also, the diurnal cycle experiments only use T forcings since S in constant.

Pgae 9: In their 100 m resolution runs, the hydrostatic simulations include the convective parameterization while the non-hydrostatic model has the parameterization turned off with the explanation that the 100 m resolution should be able to explicitly simulate the convective mixing. From a modeling perspective this is probably the major weakness in the manuscript and eliminates the possibility of attributing the differences in the result to non-hydrostatic effects. Line 12 – why the authors compare to Gulf of Eilat?

What is the relevance of such a comparison where the systems are so different? Lines 18-19 (and Fig 11): in the figure caption (fig 11) the authors say that the cooler water can be due to dilution, which means that freshwater input may be sufficient to neutralize the effect of cooling in terms of density. If that is the case, then the simulations based on T forcing alone may be a curiosity but they have no real significance or value for the Dead Sea. Regarding the summer simulations, it is not clear that an upper model layer thickness of 5 m is adequate to simulate the shallow summertime convection, which is primarily wind forced with possibly some help from night time cooling. In table 1 it seems that the day-night temp difference for forcing was chosen to be 10 deg, this seems very unrealistic.

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