Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2017-29-AC2, 2017

Ocean Science
Discussions

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

Oded Padon and Yosef Ashkenazy

ashkena@bgu.ac.il

Received and published: 15 August 2017

We thank Referee 2 for the helpful comments on the submitted manuscript. Please see below our detailed response.

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}.

We thank the referee for the accurate summary of our study.

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.

Our simulations indicate that nonhydrostatic effects affect significantly the overall circulation of the Dead Sea during winter. Following previous studies (e.g., Marshall et al., 1997) nonhydrostatic effects should be taken into account under weak stratification conditions and when the ratio between depth and length scales is relatively large, as in the Dead Sea. Yet since our setup is idealized (mainly due to the idealized surface forcing), it is possible that the nonhydrostatic effect will be less significant under more realistic forcing. We also note even small changes in the Dead Sea due to nonhydrostatic effects may be important for the potash industry of 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.

Unfortunately there are no continuous measurements of salinity as for temperature–this is since standard conductivity (salinity) device is not suitable for the Dead Sea water and densimeter is needed to measure the density and quasi-salinity. Every several month the IOLR is taking hydrographic measurements at the deepest point of the Dead Sea (some of which are shown in new Fig. 5 of the revised manuscript) but these, unfortunately, cannot support the main claim of the paper, as they reflect the state of the water column in a given location at a certain time. Although we admit that floods of freshwater may stand behind the observed colder water overlaying warmer water (shown in Fig. 12 of the revised manuscript), one has to remember that such floods occur several times during winter and contribute significantly to the lower surface salinity values during winter. The sea surface in the model is restored to these lower salinity values, and thus reflect the mean effect of freshwater floods and precipitation. Thus, we believe that our simulations account for, indirectly, the effect of flooding, and thus should be trusted.

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.

To our understanding, convective water plumes cannot be generated under the hydrostatic assumption. The convection parameterization schemes (like convective adjustment, implicit vertical diffusion and KPP), basically only mix rapidly the vertically unstable cells. The unstable situation we describe for the hydrostatic case is very short compare to the plumes' time scale–it is associated with the increased diffusion coefficient of either the implicit vertical diffusion coefficient or the KPP. Thus, although possible, as demonstrated in Figs. 10 and 11 of the new manuscript (old Figs. 9 and 10), unstable water column is very limited both in space (Fig. 7) and time compare to the nonhydrostatic 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 north-eastern part of the lake where apparently the hydrostatic simulation also produces this instability.

Exactly for this reason we show a cross section in Fig. 9 of the revised manuscript (old Fig. 8) that clearly show that the hydrostatic simulation is very different than the nonhydrostatic across the entire lake. Below (Figs. 1, 2) we present Figs. 10 and 11 of the revised manuscript (old Figs. 9, 10) for a point located at the northeastern part of the lake ($X = 15$ km, $Y = 40$ km), close to the eastern coast. These two figures resemble closely the results we obtain for the middle of the lake, shown in Figs. 10, 11 of the revised manuscript.

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.

Our main simulations are the 100 m resolution and the coarser resolution runs are mainly aimed to generate initial conditions for the fine, 100 m, simulations. Following Marshall et al. (1997) and experts that we consulted with (one of the developers of MITgcm and another world expert oceanographer), one should not use convection parameterization when the convection process is simulated (through the vertical momentum equation). In the absence of the full vertical momentum equation, the convection process cannot fully simulated and hence the convective parametrization of the hydrostatic simulations. Known examples of MITgcm of convection processes, like deep convection and plume on the slope do not include convection schemes. We also note that we the implicit vertical diffusion scheme is not the simplest convection scheme (the convective adjustment scheme is simpler) and that we have obtained similar results when using the more complicated KPP scheme (Large et al., 1994).

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.

We agree that there are better lakes to study nonhydrostatic effects. Yet, our main goal is to study the Dead Sea circulation, and nonhydrostatic effects within this unique lake. Dead Sea circulation is of local and regional importance, and we are the first to study nonhydrostatic effects in this lake.

Anyway, a proper validation of the model results that unstable density structure can remain for the entire nighttime is missing in this paper.

As we elaborated above, this is indeed a limitation of our study and thus the results presented in the paper should be regarded as model predictions. We hope that continuous salinity measurements will be available in the future, and base on these we hopefully will be able to validate these model predictions.

Minor comment:

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?

Following the referee comment, we deleted this paragraph.

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.

As mentioned in the revised manuscript (Sec. 2.2), we have repeated some of the numerical simulations (winter time) using 100 vertical levels (instead of 15), with upper ocean vertical resolution of 1 m and obtain similar results. As for the summer time, we have looked at temperature measurements from the center of the Dead Sea and it is apparent that the nocturnal cooling span more than the upper 10 m, such that our model could resolve this cooling.

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.

Yes, we meant "quasi repeating annual cycle" and we change the text accordingly as follows (end of the first paragraph of Section 2.2):

> it was run for twenty years to a quasi repeating annual cycle, representing the Dead Sea's circulation under annual cycle forcing.

This quasi repeating annual cycle results are demonstrated in Fig. 4 of the revised manuscript.

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.

We pick the restoring times based on Tziperman et al. (1994) and on the global ocean example of MITgcm for which the restoring time for temperature is three time smaller than the restoring time for salinity—for a top layer depth of 50 m the temperature and salinity restoring times are 2 and 6 months. The depth of the top layer in our setup is 5 m and thus restoring times of 4 and 12 days for temperature and salinity are reasonable. In addition, these restoring times yielded reasonable agreement with observations (see Fig. 5 of the revised manuscript). As now mentioned in the revised manuscript (in the paragraph after Eqs. 2,3), we agree that different restoring times may yield different results:

> ... were 12 days for surface salinity and 4 days for surface temperature; we note that a significantly different choice of restoring times may yield different results.

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.

Please see our response above to this comment.

Line 12 – why the authors compare to Gulf of Eilat? What is the relevance of such a comparison where the systems are so different?

We mention the Gulf of Eilat to demonstrate that indeed dense water can overlay lighter water for several hours as we observe in our model. The gulf of Eilat has similar surface temperature forcing as the Dead Sea and similar precipitation rate.

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.

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See our response above. We agree and clearly mention this limitation in the text. Please note that freshwater water floods occur in the Dead Sea and contribute to the fresher sea surface during winter. Thus, our restoring to the fresher winter sea surface indirectly (and in an average way) take into account such flooding events. We hope that in the future continuous salinity measurements will be available to validate (or reject) our model predictions.

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.

See above our response on this point.

In table 1 it seems that the day-night temp difference for forcing was chosen to be 10 deg, this seems very unrealistic.

Please note that the restoring times in these simulations are much larger than 1 day (4 and 12 days for temperature and salinity) such that the actual forcing does not reach a day-night difference of 10C.

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Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2017-29, 2017.

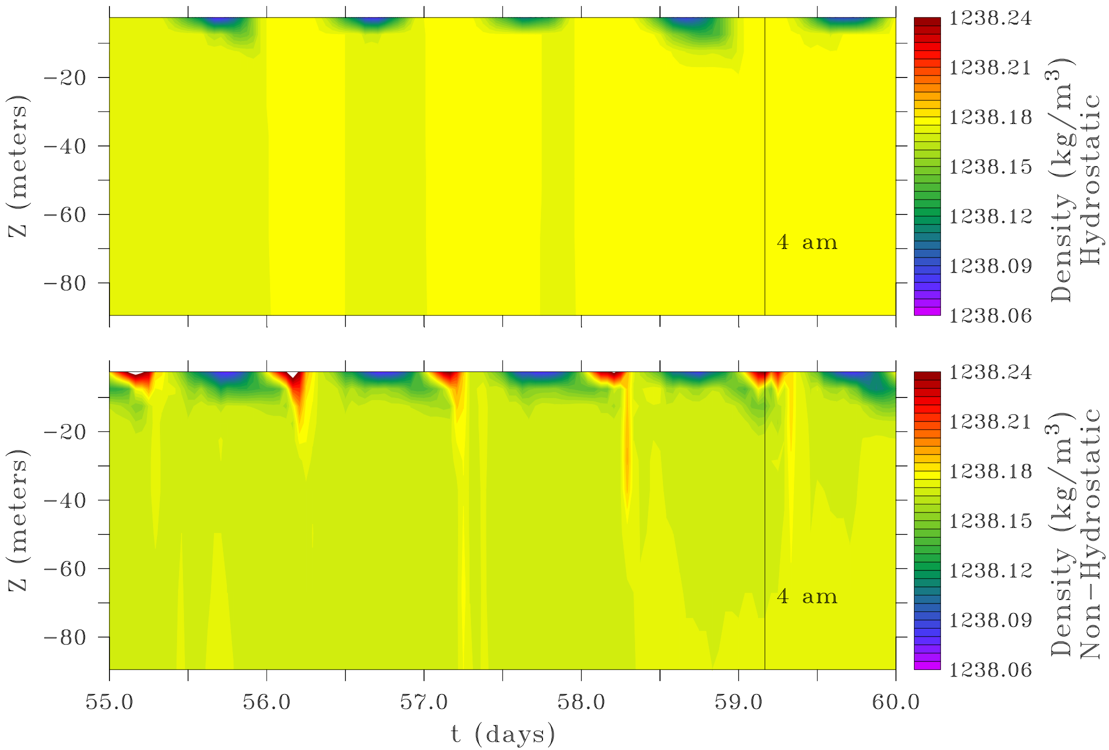

Fig. 1. Density profile versus time in the northeastern part of the lake (X=15 km, Y=40km), for the hydrostatic and the non-hydrostatic simulations of the diurnal cycle with a 100m resolution, in winter (Jan)

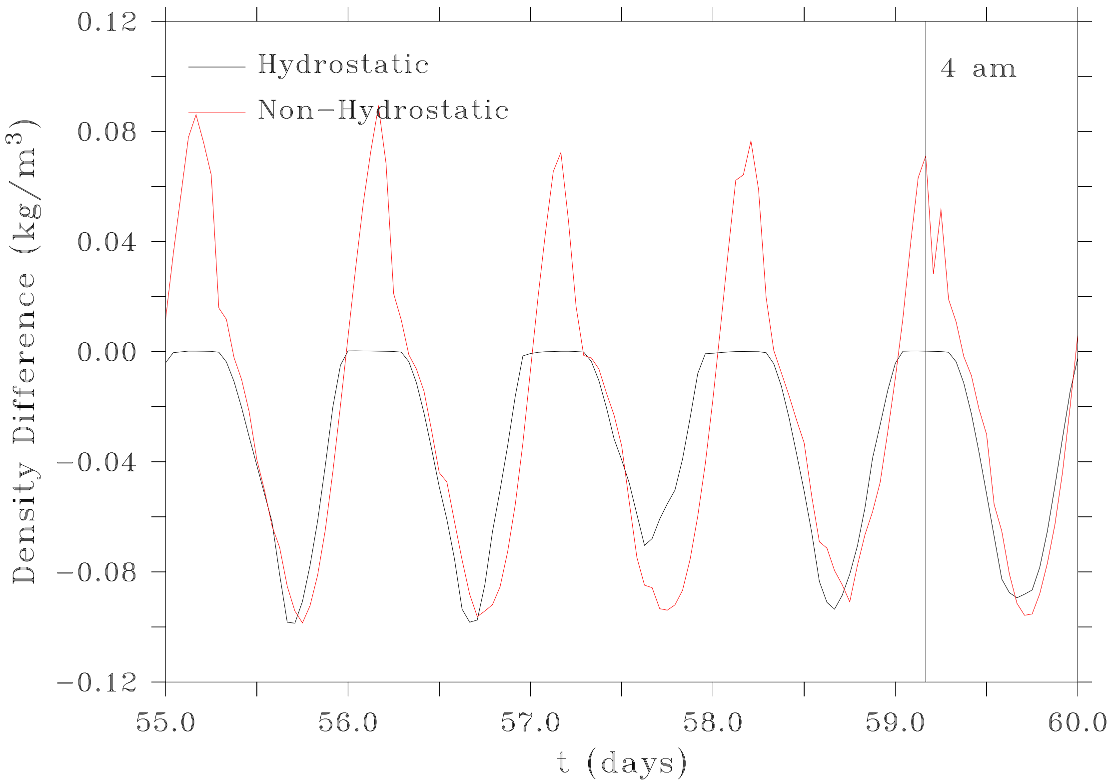

Fig. 2. Winter density difference between surface and deep water at a northeastern point (X=15 km, Y=40 km) versus time, for the hydrostatic and the non-hydrostatatic runs of the diurnal cycle (100 m).

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