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

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We thank the first referee for his deep and thoughtful report. The referee identified himself as Tal Ezer, an expert on ocean modeling who developed and run the first oceanographic model to study the water circulation in the Dead Sea. We are grateful for his report.

Numerical modeling of the circulation and mixing processes in the extreme environment of the Dead Sea (DS) is a very challenging task, so progress has been very limited since the development of the first coarse-resolution circulation models of this lake more than three decades ago (Ezer, 1984; Sirkes, 1986). Therefore, the current study, using advanced high-resolution, non-hydrostatic MITgcm, provides new insights

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into the dynamics of the DS, with focus on non-hydrostatic processes. Because of the unique characteristics of the DS, adapting to the DS model schemes and parameterizations derived for other oceans is usually not appropriate. Therefore, even this advanced model lacks some realism in aspects such as the equation of state, wind drag coefficient, lack of freshwater input and possibly unrealistic mixing schemes. Nevertheless, in my opinion, in the context of model sensitivity experiments to compare hydrostatic versus non-hydrostatic dynamics, the study is important and novel enough to be published, if these limitations are acknowledged. Since the focus of this study is on the surface mixing process, clarifications are needed about the (somewhat unrealistic) model mixing parameters and imposed surface boundary conditions. It seems that the main difference between the hydrostatic and non-hydrostatic simulations during winter nights is a direct result of the different vertical mixing used in those cases and the question is whether this is an artifact of the particular mixing used by the model or a more general result. The paper is generally quite well written (though some text and figures can be improved; see comments below) and the interesting results are clearly presented.

We thank the referee for his careful and accurate summary of our study. See our detailed response below.

Major comments:

1. The main concern is the vertical mixing used, which needs further clarifications (p. 4). In the hydrostatic model runs, slight instability due to surface cooling caused immediate strong mixing- this is typical for mixing that depends say on the Richardson number or based on a stability function in a turbulence scheme like the Mellor-Yamada (M-Y) model, but here it seems that (inappropriate) constant vertical diffusion coefficient is used everywhere, which do not explain the results. Line 15 is confusing, since only Large et al. (1994) describe the KPP, while Mellor and Yamada (1982) and Ezer (2005) describe and use the M-Y turbulence scheme, not KPP. The imposed restoring surface BCs (p. 5) can really limits surface variability and strongly impact the mixed

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layer variability (especially for hydrostatic models), so one wonders how different the results would have been without this restriction. Also, fresh water input from the Jordan river and winter flash floods which are neglected here may play a major role in the seasonal changes in stratification – this is only acknowledged at the end of the paper, but this limitation on the salinity and density fields should be clearly indicated when the model setup and surface BCs are described.

As mention on page 4 (line 15), we have used both the implicit vertical diffusion and the KPP vertical mixing schemes of MITgcm and both yielded similar results. In the implicit vertical diffusion scheme, a much larger vertical diffusion coefficient is set when unstable conditions are identified–this check is performed for each grid point at each time step and is aimed to parameterize the convection process. We clarified this point on page 4 as follows:

> In all hydrostatic simulations and the low resolution non-hydrostatic simulations (400 m and 200 m), implicit vertical diffusion was used. The implicit vertical diffusion scheme is a standard vertical mixing scheme (MITgcm-group, 2010) in which the vertical diffusion is drastically increased (i.e., from a value of 10-5 m^2 s-1 15 to 10 m^2 s-1) when the water column, at each time step and each grid point, becomes unstable.

As for the inaccurate citation of Mellor and Yamada (1982); Ezer (2005), we now exclude these references.

The restoring times we used for the temperature and salinity are close to the values used by the global ocean simulation of MITgcm (i.e., restoring times for temperature and salinity of 2 and 6 months where the top ocean layer depth is 50 m) and to the values discussed in Tziperman et al. (1994); the depth of top layer in our simulations is 5 m and hence the approximately one order of magnitude smaller restoring times of 4 and 12 days. These restoring times yielded sea surface temperature and salinity that are close to the observed annual cycle of surface temperature and salinity. We indeed aware that our results may be different when using drastically different restoring times and we acknowledge this in the revised manuscript (in the paragraph after Eq. 3) as follows:

> . . . 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.

In the revised manuscript we now mention in the model setup section that we ignore the Jordan river inflow and winter flash floods (page 5):

> The influx of fresh water from the Jordan river and winter flash floods are ignored here and indirectly considered through the relaxation to fresher sea surface during the winter.

2. Though this is a numerical modeling study, it would be useful to add (if possible, given the limited availability) some direct comparison with observation, to show at least that the seasonal variations in temperature profiles are reasonable (measurements by Anati, Hecht, Gertman and others are cited, and a 1997's book on the DS has further data). The only showing of some observations (Fig. 11) is difficult to relate to any model shown results (is it possible to show example of similar model results?).

Following the referee recommendation we now include in the revised manuscript data of temperature and salinity profiles reconstructed from the IOLR web-page (Fig. 5 of the revised manuscript). These are similar to the simulations results shown in Fig. 4 of the revised manuscript. This figure is also shown here (Fig. 1 below). In addition, Figs. 10, 11 of the revised manuscript basically show similar situation as in the observation (which are now shown in Fig. 12).

Other comments:

3. While the paper is quite well written, the text can be improved- some examples: - The second sentence of the abstract is awkwardly written - the water level of the DS "has been dropping" for a long time, it is not a new phenomenon, as can be interpreted here. - P. 2, line 14, "denser" instead of "heavier". - P. 10, title- "Diurnal Cycle Under Fixed Diurnally Varying Winds" is a confusing statement (is it fixed or varying?), what is probably meant is spatially even but temporally varying. And same page line 5- what is meant by "climatological means"? hourly, daily, monthly means?

Following the referee comments, we improved the second sentence of the abstract as follows:

> For at least three decades, the Dead Sea's water level has been dropping by more than 1 m per year, . . .

In addition, we have replaced the word "heavier" by "denser". We changed the title of section 5 to ". . . Diurnal Cycle Under Spatially Fixed Diurnally Varying Winds". As for the "climatological means" mentioned at the same page, as mentioned in Sec. 2.2, we refer here to the diurnal mean cycle (based on hourly mean data) averaged over January and July over years 2006-2010. To avoid confusion we refer the reader to Section 2.2 and Fig. 2 and now write "climatological hourly means".

4. Figures can be improved with clarifications in captions- some suggestions: - Fig. 1- since it is based on data from 1978 when water level was much higher, it should be clarified for what year the shown shoreline is and what is the water level at that year. - Fig. 2- may be show wind vectors instead of components?, also caption should state if these data are based on hourly observations and what source. - Fig. 4- may be show only the top 100 m (like other figures) to see the details.

We agree and corrected mentioned captions and figures as follows. The bathymetry shown in Fig. 1 corresponds to surface water level of -427 meter below mean sea level (year 2013)–the first sentence of Fig. 1 caption is now: "Dead Sea bathymetry (based on Hall, 1978), corresponds to surface water level of -427 meter below mean sea level (year 2013).". As for Fig. 2, we prefer to leave this figure as is, to allow better visualization of the fine details of the different wind stress component during the different months. We now write at the end of caption of Fig. 2 that

> The curves are based on hourly mean wind data of the Israel Meteorological Service's Ein Gedi station.

We have changed Fig. 4 as suggested by the referee–now we show only the top 100 m.

5. P.2, near bottom, to first paragraph of p.3- "non-hydrostatic effects are expected to be significant"- is it possible to quantify this assumption based on say scaling arguments such as ratio of horizontal and vertical velocity scales in the DS vs open oceans and the very steep bottom slopes of the DS?. Also, when citing past studies of hydrostatic vs. non-hydrostatic comparisons, one should add that these studies may not be applicable to the unique environment of the DS. For example, in the DS the very high salinity and variations in salinity (which are ignored here) play a major role in the static stability compared to the major role of temperature in most other oceans.

Indeed it is possible to quantify the non-hydrostatic effects based on the non-dimensional number (nonhydrostatic parameter) n developed by Marshall et al. (1997):

$$n = \frac{\gamma^2}{R_i} = \frac{U^2}{L^2 N^2}, \tag{1}$$

where $\gamma = H/L$ (H and L are the vertical and horizontal scales), and R_i is the Richardson number, $R_i = N^2 H^2 / U^2$, where N is the buoyancy (Brunt-Väisälä) frequency, $N = -(g/\rho_0)(\partial\rho/\partial z)$, and U is the horizontal velocity scale. The hydrostatic approximation holds when $n \ll 1$. During the summer, $N \approx 8 \times 10^{-3}$ s^{-1} and with $U \approx 0.1$ m s^{-1}, $H \approx 300$ m, $L \approx 20$ km, yielding $n \approx 4 \times 10^{-7} \ll 1$. Thus, the hydrostatic relation holds during the summer. The buoyancy frequency becomes much smaller during the winter and approaches zero when the entire water column mixes; n is expected to be much larger than 1 then. When the bathymetry is steep (relatively large γ), the water column becomes nonhydrostatic even faster.

We find the above discussion too complicated to actually improve the understanding of the nonhydrostatic effect in the context of the Dead Sea. In essence, the most important ingredient is the stratification–as the water column becomes weakly stratified, nonhydrostatic effects are expected to be significant. In the revised manuscript we refer the interested reader to Eq. (2) of Marshall et al. (1997).

Following the referee comments, we also included the following sentence (top of page 3 of the revised manuscript):

> One should note however that, due to the unique environment of the Dead Sea, these previous studies may not be applicable to the Dead Sea; e.g., salinity variations affect significantly the stability of the water column in the Dead Sea in contrast to most of the world oceans in which salinity variations play less significant role (in comparison to the temperature variations).

6. P.3- in the description of the model, it is not clear if some of the features are actually used here, e.g., partial cells (probably important for the steep DS topography) and adjoint (probably not relevant here).

Correct, the partial cells option is used but the adjoint. We clarify this in the revised manuscript (section 2.1 of the revised manuscript) as follows:

> In the current study we use the hydrostatic and full non-hydrostatic options of MITgcm together with the partial cells option, to account for the steep bathymetry of the Dead Sea.

7. P.7, line 20 (and elsewhere) – “water immediately mixed once it became denser

than the water below"- why?, is vertical mixing enhanced based on stability?, does this contradict the "implicit vertical diffusion" (constant?) mentioned before?.

We used the wrong terminology. We now replace the words "immediately mixed" by "very fast mixed" (according to the time associated with the implicit vertical diffusion coefficient).

8. P.9, line 25 and Fig. 12- it seems that mass is not conserved whereas in the non-hydrostatic model the entire lake is less dense than the hydrostatic case?, please explain how this is possible if the same surface BCs are used.

One should note that in the winter (January) simulation we did not run the model to a steady state as the winter is a transient phenomenon (in which, e.g., there is a positive net freshwater flux and surface cooling). During this relatively short simulation one may achieve different mean density between the hydrostatic and nonhydrostatic simulations, due to the different dynamics of the two.

9. P.12- line 31- delete "xxx" (missing texts?). Lines 9-10- "the hydrostatic simulations are not suitable for simulating fine resolution. . ."- one should add, "unless more sophisticated vertical mixing scheme than used here is applied".

We deleted the extra "xxx" and added the end of the sentence suggested by the referee.

References

Anati, D. A. (1999). The salinity of hyper saline brines: Concepts and misconceptions. *Int. J. Salt Lake Res.*, 8:55–70.

Ezer, T. (2005). Entrainment, diapycnal mixing and transport in three-dimensional bottom gravity current simulations using the mellor–yamada turbulence scheme. *Ocean Modelling*, 9(2):151–168.

Gertman, I. and Hecht, A. (2002). The Dead Sea hydrography from 1992 to 2000. *J. Mar. Sys.*, 35(3-4):169–181.

Hall, J. K. (1978). Dead Sea Geophysical Survey, Bathymetric Chart. Marine Geology Division, Geological Survey of Israel.

Large, W. G., Mcwilliams, J. C., and Doney, S. C. (1994). Oceanic vertical mixing: A review and a model with a nonlocal boundary-layer parameterization. *Rev. Geophys.*, 32(4):363–403.

Marshall, J., Hill, C., Perelman, L., and Adcroft, A. (1997). Hydrostatic, quasi-hydrostatic, and nonhydrostatic ocean modeling. *J. Geophys. Res.*, 102(C3):5733–5752.

Mellor, G. L. and Yamada, T. (1982). Development of turbulence closure model for geophysical fluid problems. *Rev. Geophys. Space Phys.*, 20(4):851–875.

MITgcm-group (2010). MITgcm User Manual. Online documentation, MIT/EAPS, Cambridge, MA 02139, USA. http://mitgcm.org/public/r2_manual/latest/online_documents/manual.html.

Tziperman, E., Toggweiler, J. R., Feliks, Y., and Bryan, K. (1994). Instability of the thermohaline circulation with respect to mixed boundary-conditions: Is it really a problem for realistic models. *J. Phys. Oceanogr.*, 24(2):217–232.

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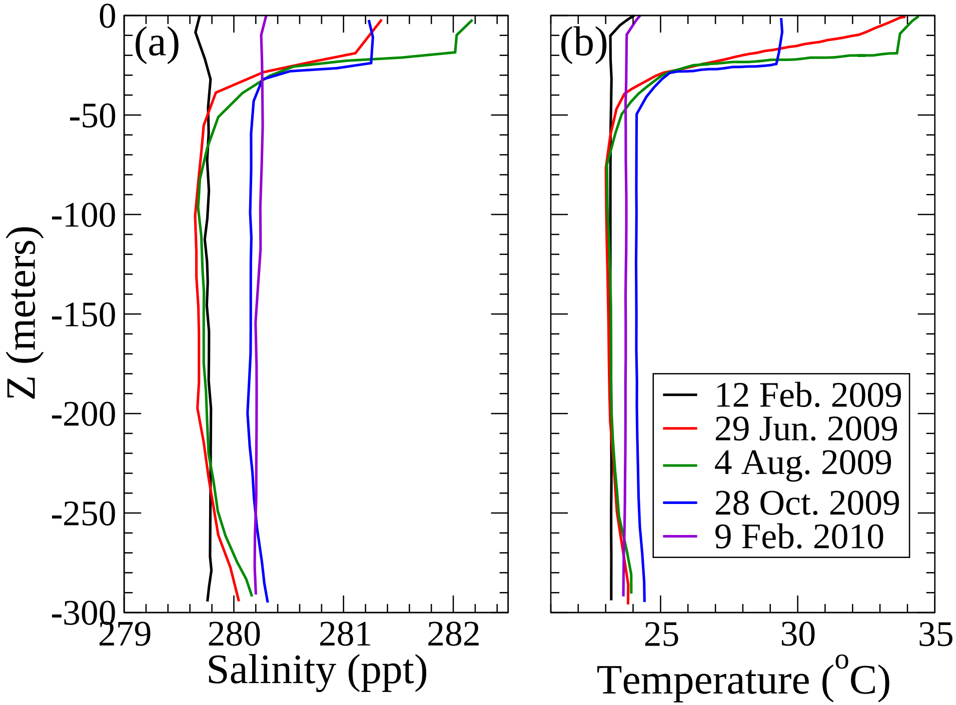

Fig. 1. Salinity (a) and temperature (b) profiles measured by the IOLR at the deepest point of the Dead Sea. The simulations shown in Fig. 4 are in the agreement with these observations.

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