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

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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 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. Never-

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theless, 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.

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 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.

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?).

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?

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.

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.

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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).

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 befdore?.

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.

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”.

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