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# *Interactive comment on* "A Model of Black Sea Circulation with Strait Exchange (2008–2018)" *by* Murat Gunduz et al.

**Anonymous Referee #1**

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The manuscript describes an implementation of the Black Sea circulation model (BSEA), which is based on the NEMO version 4.0 (Madec, 2008). The model domain includes the deep Black Sea basin (max depth: 2178 m) together with the shallow Azov Sea (depth 10m) in the north and the Marmara Sea in the south. In order to represent the Bosphorus exchange flows coupled to Black Sea, an artificial box representing the Marmara Sea and the Bosphorus Strait have been added to the domain. The manuscript focused mainly on the analysis of the 10-year (2009-2018) variations of temperature and salinity using their monthly and sub-basin averaged values.

The model could provide a good opportunity to improve the modelling ability of the Black Sea by incorporating the Bosphorus Strait into the simulations. However, I would like to express concerns that in its current form the manuscript focusing largely on



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the validation of a model rather than using the model to fully explore the processes responsible for the trends or capturing real dynamical features. This study does not focus adequately on new knowledge and instead focuses largely on developing and validating a model. I think the paper needs to be substantially revised in order to focus more on the new understanding of the processes that can be obtained, i.e. using the model as a tool to explore them. I recommend a major revision or possibly a resubmission. Detailed comments are provided below.

Main points:

In my opinion, a major weakness of the manuscript is the lack of hypothesis testing. In the Abstract the authors suggested that "The present formulation with temperature and salinity relaxed to the observed seasonal climatology of the Marmara box and open boundary conditions are found to enable Bosphorus exchange with upper, lower layer and net fluxes comparable to the observed range. This in turn enables to capture the trend of rapid climatic change observed in the Black Sea in the last decade."

I did not find in the entire manuscript how the authors test and prove the above hypothesis that their model can capture the trend of rapid climatic change. The manuscript does not present any new aspect or particular characteristic of the Black Sea hydrodynamics. For example, I would like to see how the new approach, taking into account the Bosphorus Strait in simulations, helps to gain new knowledge about the disappearance of the cold intermediate layer (CIL). Comparison with other existing Black Sea models is not presented, so I cannot decide whether the proposed model better simulated the Black Sea hydrodynamics or not.

I doubt that the proposed model cannot capture the CIL adequately. Figure 7 gives the average vertical distribution of temperature for the 3 regions, namely, East, West and Rim. Even the quality of the figure is very low I can see that the CIL is almost absent. Many recent studies based on ARGO float data (Stanev et al, 2013, 2014, 2017 and 2018) and numerical modelling (Miladinova et al., 2017 and 2018) clearly show the

presence of CIL in the period 2009-2015. CIL is eroded but exists in winter-spring. I recommend the authors to look at the figures of Stanev et al. and try to visualize their results in a similar readable way. Figure 7 shows many crossed lines of different meaning and the same colour. Please mark the isotherms 7, 8.5 and 15 ° C. What is the meaning of the white gaps in Figure 7? The CIL is not visible in figs. 12 and 14, too. On the base of the proposed model, the authors stated "The abundance of CIL in the initial conditions maintained for the first two years contrasts with the single event of cold intermediate water formation in 2012, and the weaker event in 2017". The CIL is present during the period of 'spin-up' and disappears after the second year. Thus, I can conclude that the model is not able to represent the CIL adequately.

Most of the figures are of poor quality. The range of variable variation is too wide to distinguish small but important variation.

Validation. It is not enough to present the comparisons graphically. It is necessary to give the coefficient of correlation, the absolute values of errors, and standard deviation. It is better to substitute the figures 8 and 9 with a table containing the statistics of the comparisons.

I cannot understand the following conclusion: "The reduced convection events in recent years both in the deeper central basin and near the coast stand as evidence that great changes are occurring in the Black Sea, much likely to be an amplified response to climate change in the isolated Black Sea basin severely limited in its communication with the Mediterranean Sea and eventually with the world ocean." Which their results indicate the "reduced convection" and also "great changes"? What is the meaning of the phrase "much likely to be an amplified response to climate change in the isolated Black Sea"?

I strongly recommend that the authors should:

a) identify the novelty of the knowledge gained; b) add the comparison with other numerical simulations; c) improve the quality and readability of the figures; d) present

statistics for the model-model and model-data comparisons; e) improve the presentations of results and conclusions.

Minor points:

I suggest authors to use the MEDAR climatological data (http://modb.oce.ulg.ac.be/backup/medar) for initialisation of temperature and salinity. MEDAR data is freely available until 2002. The thermohaline fields in 1992 are not appropriate for initialisation of simulations that start in 2008.

I couldn't understand this sentence "In this way the CIL, which is a product of convective mixing in the Black Sea, influences water mixed on the shelf and returned back to deeper layers of the Black Sea, also influencing Marmara Sea." How the return back of CIL waters is presented by the model?

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