Dear Editor and Reviewers:

Thank you very much for your thoughtful and constructive comments concerning our manuscript "NEMO-Bohai 1.0: a high-resolution ocean and sea ice modelling system for the Bohai Sea, China" (No. gmd-2021-100). The comments are valuable and helpful for improving our manuscript, which we have addressed in our point-by-point responses enclosed. In what follows we will bring the original comments in blue and the responses in black, with new text added in the manuscript highlighted in *Italic*. The changes made in the revised paper are highlighted in the track changes version of the manuscript attached.

Best regards,
Yu Yan and Petteri Uotila (on behalf of all the co-authors)

Response to comments from Referee #1

This is my second review of this manuscript, and I think there has been a great improvement from the first version. The language is a lot better and many additions have been done which give more backbone to the article.
I still have however a few points, mostly about ocean dynamics. I do not see these points as compulsory for publication, but I think they would improve the manuscript.

We appreciate the reviewer's recognition of our first revision, and thank for the second round of review on our manuscript. We have considered your comments to improve the manuscript and answered point by point.

- The authors mention that the circulation in the Bohai Sea is mostly barotropic. From Figure 5, I do not think it is that obvious, especially in the Northern part of the basin where the surface velocities go South whereas the deeper ones go North. Also the representation with vectors makes it very difficult to distinguish velocities close to the entrance of the Bohai Sea, I suggest you use stream functions instead. You could for example plot a barotropic stream function, but since there is a obviously a baroclinic circulation also plot one stream function for the upper layer and one for the lower layer, and compute the divergence of each layer which should correspond to the baroclinic circulation. I guess since the baroclinic circulation occurs in Winter and Summer then it is a circulation of haline nature, and because of the restricted size of the area it is obviously not completely geostrophic.

Yes, we agree that it is hard to distinguish velocities represented with original vectors plots in the Bohai Strait. Following your suggestions, we have used stream functions instead. In Fig.5, the black lines and arrows represent the streamlines and directions of the current vector field, respectively. Besides, as you indicated, the barotropic flow is not that obvious. Our statement on the barotropic circulation is a speculative one and thus we decided to delete this statement and study further in future study. We have also modified the corresponding text in section 3.2.3:

The simulated monthly mean current velocities at the surface and 16 m depth in February and August are shown in Fig. 5. The monthly mean current velocities are calculated based on hourly model output during August 2012 and February 2013. The figure shows that both the sea surface *and 16 m depth currents are usually less than 0.4 m s^{-1}. Due to the blocking effects of the bays, the currents are weak at the head of the three bays, which is consistent with the observations by Chen et al. (1992). The maximum current velocity zone is located in the northern Bohai Strait, in a good agreement with the model simulation result of Ji et al. (2019). The inflow and outflow occur in the northern and southern parts of the Bohai Strait in both seasons, respectively, which is consistent with the observations (Zhang et al., 2018). Specifically, the strongest modeled inflow from the Yellow Sea through the Bohai Strait occurs in a narrow channel in its northern part, namely the Laotieshan Channel, which agrees with the observations of Wan et al. (2015). Also, Lin et al. (2011) suggested that persistent winds drive a cyclonic coastal current in the northern Yellow Sea, and one branch of the current enters the Bohai Sea at the northern Bohai Strait, which transports warm and saline water from the Yellow Sea.*

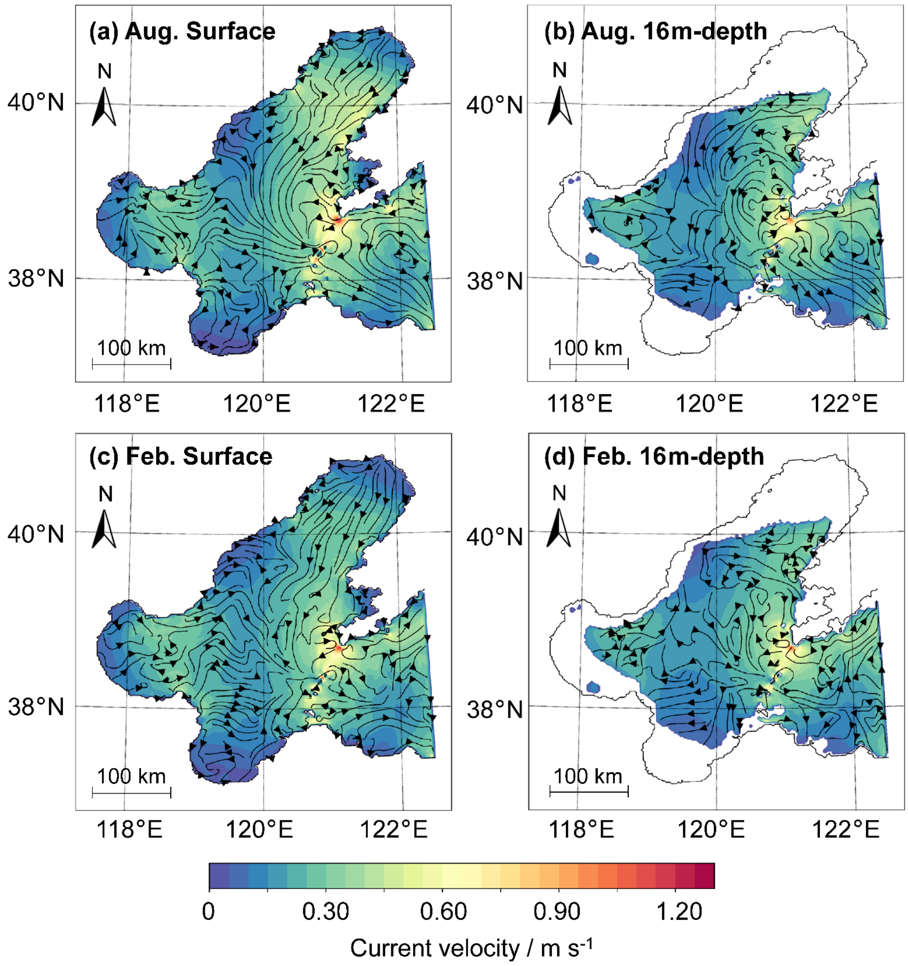

Figure 5: Simulated monthly mean current velocities at surface and 16 m depth in August 2012 and February 2013. The monthly mean current velocities are calculated based on the outputs with hourly intervals. The black lines and arrows represent the streamlines and directions of the current vector field, respectively. The filled contours denote the current speed in m s^{-1}.

- Not only there seem to be a baroclinic circulation, but also it is possible that it could be stronger in reality than in the model: the comparison between model and T/S profiles shows that there are thermal and haline frontal structures. Which brings me to a second point, the model is indeed too mixed, a fact the authors explain with bathymetry errors, and with which I do not agree. I think the use of TKE with no specific tuning for such a region is responsible. Given the low inertia of the system, it is easy and it would be interesting to see how testing a turbulence closure that is more realistic would change the currents and perhaps even the sea ice cover. This can be easily done by switching to a GLS approach, or tuning the TKE scheme that you already use. For the latest, I suggest to refer to this article https://gmd.copernicus.org/articles/8/69/2015/, and more specifically to the Hp parameter, and the background diffusivity/viscosity.

Indeed, we agree that the model is too mixed. As for the difference between modeled and observed T/S profiles, especially for the lack of stratification in summer, it is possibly caused by the vertical mixing setting with the used TKE closure scheme, and the high setting of vertical diffusivity in the model. So far, our vertical mixing is actually strong, but we plan to carry out the sensitivity experiments with different turbulent closure schemes and vertical diffusivity coefficients. Following your suggestions, we have mentioned this in 3.2.4:

The modeled salinity stratification in summer is weaker compared to the atlas, which is possibly caused by the vertical mixing setting with the used TKE closure scheme, and the high setting of vertical diffusivity in the model.

Thanks for pointing out the study (Reffray et al., 2015), which provides a comprehensive overview of the turbulent vertical mixing options. We have added this reference in the perspective part (see section 5) and further emphasized the role of tuning vertical mixing:

In addition, in order to carry out more accurate estimation of vertical mixing, it is worth implementing the experiments of turbulent vertical mixing options (Reffray et al., 2015) for the Bohai Sea for further development of NEMO-Bohai.

Other remarks:

- Please check references, there are obviously bugs in some places. An example is Bernard (2006) which refers to Barnier (2006).

Thank you for noting this. We have followed you and checked the references. We have revised Bernard et al. (2006) to Barnier et al. (2006) accordingly.

Barnier, B., Madec, G., Penduff, T., Molines, J. M., Treguier, A. M., Le Sommer, J., Beckmann, A., Biastoch, A., Böning, C., Dengg, J., Derval, C., Durand, E., Gulev, S., Remy, E., Talandier, C., Theetten, S., Maltrud, M., McClean, J., and De Cuevas, B.: Impact of partial steps and momentum advection schemes in a global ocean circulation model at eddy-permitting resolution, Ocean Dynam, 56(5), 543-567, 2006.

- In table 1, please put a black separating column at the centre so that one does not read an entire line and falls into confusion.

Done.