Dr. Qiang Wang
Topic editor
Geoscientific Model Development

Re: gmd-2021-95

Dear Dr. Qiang Wang and reviewer

Thank you for handling and reviewing our manuscript entitled “Comparison of ocean heat content from two eddy-resolving hindcast simulations with OFES1 and OFES2” to be considered for publication in the GMD. We appreciate your very constructive comments and have addressed accordingly.

Major comments:

1. *It is interesting that the ocean heat content changes primarily by the change in isopycnal depth. Does the total heat content calculation depend upon the calculation of heat content change by heaving motions? It would be good to provide corroborating evidence regarding the heat content decomposition, such as an independent calculation of total heat content variability.*

Response: In this paper, we decomposed the potential temperature change into heave and spiciness components as a method to identify the way how the water warms or cools. In Figs. 2-6 and 8, we have OHC, HV (heave) and SP (spiciness), and please be noted that the OHC here is calculated directly from the potential temperature following Eq. 1 (Line#131 in the clean version). To make it clearer, we specify in the revised manuscript that "The OHC hereafter is directly calculated from the potential temperature". (Line 209-210 in the clean version)

2. *Abstract: Heat transport is stated to not always be responsible for ocean heat content changes. Doesn't it have to be either the heat transport or the air-sea flux, given thermodynamic energy conservation? Heat content storage will be the residual of these terms. It is puzzling to consider where the thermodynamic energy is transported. The manuscript would benefit with a closed energy budget analysis, which may require the deep ocean and/or the Arctic to accurately assess where the energy goes.*

Response: In the first version of this manuscript, we presented the pattern of net surface heat flux, horizontal and vertical heat transport. Their pattern is geographically similar, although differences are also clear in some places. However, as it is not easy to link this pattern to the examined OHC differences between the two OFES products, we therefore speculate that the OHC differences may result from the discrepancies in the mixing, especially the vertical mixing, given the OFES1 and OFES2 used KPP and mixed layer vertical mixing, respectively. Although the spatial pattern helps to qualitatively analyse the differences, a detailed heat budget is desirable indeed. In the revised manuscript, we used the currently available data to calculate the inter-basin heat exchange and vertical advection of heat, as can be found in Tabs. 4-5. Caused by a temporal suspension of data from JAMSTC, we are not able to access the vertical diffusivity data of the OFES2 (OFES1 itself does not provide the vertical diffusivity data). Alternatively, we approximately took the residual of the OHC variations and the heat input (net surface heat, inter-basin heat exchange and basin-integrated vertical heat advection at a given depth (500m and 1500 m in this paper) as the vertical diffusion of heat. This indirect method may suffer from some errors, but could help to identify the major vertical mixing distinctions between the OFES1 and OFES2. As the new results show, we found there is less heating for the major basins in the OFES2. The horizontal heat advection is largely similar but the OFES2 has a much stronger meridional heat transport associated with the Indonesian Throughflow (ITF). It was also found that the vertical heat advection differs significantly. Therefore, it was claimed that the marked OHC differences may arise from the less heat

input from the atmosphere, significantly different vertical heat advection and the inferred vertical heat diffusion.

Minor comments:

1. L29: does this sentence equate objective analysis and ocean reanalysis? They are normally considered to be distinct

Response: Yes, these two are different. In this sentence, we want to say that different types of oceanic data are available for the 4D studies of the ocean thermal state, e.g. objective analysis (e.g. EN4) and ocean reanalysis (e.g. ECMWF ORAS5). To make it clearer, this sentence was revised to "Typical examples of these approaches include the objective analysis of observational data **and** ocean reanalysis by physical ocean models constrained by observations". (Lines# 37-38 in the clean version)

2. How are ocean heat content changes related to algorithmic changes between OFES 1 and 2?

Response: These two use the same model MOM3, same horizontal resolution and horizontal mixing scheme. That's is, the core of the algorithmic should be the same between these two. However, they used different surface forcing, vertical mixing schemes, different initial conditions. As no vertical diffusion coefficient is available, it could be difficult to evaluate these impacts.

3. How did the authors justify restricting their analysis to the upper 1400 meters? Their rationale following Emery (2001) and Wunsch (2011) is not compelling. Did these previous works suggest that ocean disequilibrium occurs suddenly at 1400 meters depth? (That would be surprising.) During the time period of interest, i.e., 1950-present, why should the deep ocean be in equilibrium?

Response: The primary reasons we consider to focus only on the upper 1500 m (There was a misinterpretation of water depth in the previous version, it should be 1500 m rather than 1400 m, which has been rectified in the new revised manuscript) are mainly two-fold. 1) we analyse the OHC variations largely from the perspective of water masses. As defined in Emery (2001), the world ocean was divided into three layers (0-500 m, 500-1500 m and below 1500 m). 2) the observational data ingested by the EN4 is largely confined to the upper ocean, with much lower density of data in the deep and abyssal ocean.

Furthermore, the ingested data in the EN4 version we used here is bias-corrected following Levitus et al. (2009), in which only the upper ocean is considered. Another less important reason is that the maximum depth among these three data is significantly different.

4. For comparison purposes, does the NCEP reanalysis give air-sea conditions every 6 hours, as opposed to 3 hours in OFES2?

Response: The OFES1 was forced by the daily NCEP forcing, as can be found in (Line# 88 in the clean version).

5. L86: "Validation" is not possible with EN4 as it is also an incomplete and uncertain product.

Response: This was changed to “objectively evaluate”. (Line#102 in the clean version)

6. Figure 1: What happens in the Arctic? What error is incurred by eliminating the Arctic?

Response: It is very important to look into the state of the Arctic. However, the OFES1 (OFES2) is confined to 75°S-75°N (76°S – 76°N). Also, the potential temperature decomposition into heave and spice is valid between 80°S and 64°N. In addition, a sea-ice model applies in the OFES2 but not in the OFES1. We therefore, did not include the Arctic in this work.

7. Figure 2 is fascinating, if correct. What is going on with OFES2?

Response: As the Figs. 12-14 shows, the large-scale pattern of the surface heat flux and the horizontal and vertical heat advection is much the same. But there is generally less heat input or more heat loss in the OFES2. In addition, from the calculated heat flux through the major water passages around the world ocean and basin-integrated vertical heat flux at 500 m and 1500 m, we can see that the horizontal heat flux is close to each other, except the one associated with the Indonesian Throughflow, for which the OFES2 shows much stronger heat flux. There exists large differences in the vertical heat flux between the OFES1 and OFES2, and we also infer that the vertical heat diffusion is largely different between the two data.

8. L161: dividing by 56 "years".

Response: This was addressed (Line# 222 in the clean version).

9. What does "SP" stand for?

Response: The SP means the spiceness component of the potential temperature change and similarly the HV stands for the heaving. (Line#58 in the clean version)

10. L242: 10 to the 6th power

Response: This was addressed (Line#318 in the clean version).

11. Does OFES 2 fit surface data (i.e., SST)?

Response: The OFES2 developer examined the SST comparison between the OFES2 and WOA13 over 2005-2012 and shows good results.

12. Table 4 doesn't seem very useful with the inexact metrics for the water-mass source properties.

Response: Tabs. 4-5 are built by following the definition of Emery (2001), to help the readers to have a first impression on the water masses, should they are not familiar with these. To be more concise, we move these two tables into supplementary section. Alternatively, we roughly label the geographic coverage of the major water masses in Figs 6 and 8.