

General comments

After the first round of comments on the manuscript, which the authors addressed in a satisfactory manner, the manuscript has been improved accordingly, and I only see very minor remaining issues. The only one of my new comments that require a response from the authors is that for Fig. 6. As soon as the authors have justified their choice of reference for the bias analysis in that figure, or changed to using the same reference as in the bottom temperature analysis, I recommend the manuscript for publication. I congratulate the authors on a nice piece of work.

Specific comments

Table 1. I appreciate that the authors have provided the details of the parameter choices for lateral viscosity and isopycnal tracer diffusion, as this allows for some further comparison with other modelling studies and between the simulations themselves. The coefficient value for the bilaplacian viscosity, ν , can be indicated by a targeted characteristic current velocity, U_c , at the grid size of the selected resolution, Δx , where

$$\nu = \frac{1}{12} U_c \Delta x^3 \quad (1)$$

Hence, we can also infer the characteristic velocity from the selected coefficient value ν . A typical value for U_c would be about 10 cm/s. (Gurvan Madec, pers. comm.) At the equator, 1/4° and 1/12° resolution correspond to roughly 28 and 9.3 km, respectively. The selected parameter values for ν thereby yield characteristic velocities of 8 and 19 cm/s. While 8 cm/s falls in the expected realm, the 19 cm/s for 1/12° seems high. It is possible that reducing the coefficient further for the 1/12° version of the model could help to further improve its performance, as this would likely act to slow down the ASC. I base this assumption on similar experiments with 1/10° NEMO-AGRIF where a reduction of the bilaplacian viscosity coefficient to reduce U_c resulted in a less vigorous ASC (Ödalen et al., in prep.). I do not expect the authors to change the viscosity coefficient setting in the experiments for the present manuscript, but it could be worth noting that the setting might still be a bit too high and that this could be one source of bias for the 1/12° simulation that does not exist in the 1/4° simulation.

Fig. 6. Please justify why ESA-CCI-SST is used for the bias analysis, instead of the EN4.1 reanalysis that is used as a reference for initialisation, and for bottom temperature comparison.

Minor comments

L. 140. Typo: Kerguelan → Kerguelen

L. 141. To avoid starting two sentences in a row with “The warm bias”, which reads a bit repetitive, the first sentence could be rearranged, so that it starts with “In this model, the warm bias...”, thus removing “in this model” from the end of the sentence.

L. 239: “are reduced in the Indian Ocean sector” (in is missing in the manuscript text)