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The present manuscript focuses on improving the choices that go into a mixing scheme used in the NEMO ocean model. The authors have evaluated a vertical mixing parameterization based on turbulent kinetic energy (TKE) in the Arctic region. They have checked the mixed layer penetration effects modeled in the TKE based scheme. The MLD penetration effects account for additional mixing caused by near inertial effects and ocean swells. They find that it impacts the evolution of mixed layers below sea-ice and impacts the formation of sea-ice itself. They also checked the influence of adding information about the sea-ice concentration in the mixing scheme and it leads to trends in the mixed layer depths that agree with observations.

The authors convincingly argue about the choices of tunable parameters used in mixing parameterizations. Unconstrained turbulence mixing parameterizations lead to major sources of uncertainty in climate projections. It is vital to improve mixing parameterizations, especially those of upper ocean processes which regulate the upper ocean response to surface forcing. Because of the important nature of the work done in this manuscript, I recommend this article to be published in the journal 'Geoscientific Model Development'.

We thank the referee for their detailed assessment of our work and positive appreciation. The points they raised were very useful in improving our work. Please find below detailed answers to each of their specific remarks.

I have a few minor comments / suggestions to improve the clarity of the manuscript:

1. Line 75: "The parameterization is activated in NEMO when the parameter nn_etau is set to 1 (and deactivated when nn_etau=0)."

This belongs in the NEMO documentation rather than in this article. If the authors want to document this, they can consider adding these kind of sentences to a supplementary material for additional documentation.
We have removed this phrase as it was too technical for the new version of the text.

2. Line 75: It would be helpful to write the evolution equation of TKE. It would help in understanding how the various terms such as N^2, velocity, and energy (e at time t) go into the evolution of e at time t+dt.
We have included the prognostic equation that describes the default TKE formulation in NEMO (lines 77-80) as follows:

" The prognostic equation is given by:

$$\frac{\partial \bar{e}}{\partial t} = K_m \left(\frac{\partial \bar{U}_h}{\partial z} \right)^2 - K_\rho N^2 + \frac{\partial}{\partial z} \left(K_e \frac{\partial \bar{e}}{\partial z} \right) - \varepsilon$$

It results from the balance between the vertical shear, the dissipation of TKE due to buoyancy, the vertical

diffusion of TKE, and the energy dissipation."

3. The equation written in line 75 is a bit confusing. On the left hand side, the authors have written e(t+dt,z), and on the right hand side is there a missing term: e(t,z) ?
Should the equation be:
$e(t + dt, z) = ((de(t, z)/dt) \times \delta t) + e(t, z) + e_{inertial}(t, z)$,

Where all the d are partial derivatives?

Please check the equation in line 75.
We thank the referee for pointing this out. The expression taken from Calvert et al. (2013) is:

$\bar{e}(t + \Delta t, z) = \int_t^{t+\Delta t} \left(\frac{\partial}{\partial t}\bar{e}(t, z)\right) + \bar{e}_{inertial}(t, z)$

which we rewrote as:

$\bar{e}(t + \Delta t, z) = \frac{\partial}{\partial t}\bar{e}(t, z)\Delta t + \bar{e}_{inertial}(t, z)$

Indeed, the expression is a bit confusing, so we have decided to remove it. We believe that the explanation is better without it, as it was intended to be an ad hoc parameterization of kinetic energy production through a source term.

The phrase has been modified as follows: "The TKE $\bar{e}(t, z)$ includes an additional energy source term $\bar{e}_{inertial}(t, z)$, which represents the contribution of the TKE MLP as:"

4. Table 1: This table is not necessary. You have already given a short summary around line 100.
We have revised Table 1 to include the values of each parameter. We believe summarizing this information in a table format provides a useful and easy comparison across models for the reader.

5. Line 125: You have already defined MLD (0.03) at line 115.
We thank the referee for pointing this out. However, we believe it is necessary to retain the expression to introduce the values of the reference depth, which differ between NEMO (0.5m) and the LOPS climatology (5m).

6. Line 195: Do you mean standard deviation of MLD?
In line 195, we do not reference the standard deviation. We discuss the differences in the reference depth between NEMO and the LOPS climatology.

The discussion about the standard deviation is now around line 209 in the new version of the manuscript. The phrase has been modified as follows:

" To quantify the spatial variability of the mixed layer within each basin, we measure the MLD standard deviation for each month. The seasonal cycle of the MLD standard deviation during summer is almost negligible, and in winter, for the Makarov and Canada Basins, it remains below 15 m, showing a similar spatial variability between experiments in these regions (see Fig. A2 in Appendix). However, for the Eurasian Basin, differences between experiments appear to be more substantial. For instance, the MLD std reaches up to 30 m for the $\chi = 1$ experiment and is only 8 m for the $f_r = 0$ experiment."

7. Line 80: Using the symbol f_r should be enough for the manuscript. Describing the namelist parameter is not needed.
We thank the Referee for the suggestion. We have modified the full text to refer to the simulations using the names of the parameters in the equation (f_r, χ and h_τ), rather than the individual names of the NEMO parameters (rn_efr, nn_eice and nn_htau).

8. Line 95: Please provide a reference for ORCA1. Is this a specific configuration or some different model is not clear.
We have included the following explanation: "To carry out this investigation, we utilize the NEMO4.2 version with the SI3 sea ice model, using the eORCA1 configuration. The eORCA1 quasi-isotropic global tripolar grid has a nominal resolution of 1°, extended to the south to better represent the contribution of Antarctic under-ice shelf seas to the Southern Ocean freshwater cycle. The grid has a latitudinal grid

refinement of $1/3^{\circ}$ in the equatorial region. The vertical discretization consists of 75 levels, where the initial layer thicknesses increase non-uniformly from 1 m at the surface to 10 m at 100 m depth, reaching 200 m at the bottom.“ (lines 103-107).

9. Line 130: For table 2, you could provide an experiment number as an additional column before the ‘parameter’ column. In general, I find that writing down both: variables and namelist variables used in NEMO code to be confusing. The authors could consider using only symbols rather than namelist variables. All the symbols and their corresponding namelist variables could be added to a table in an appendix.
Following the answer to remark 7, we have removed this table because we now refer to the simulations using the names of the parameters in the equation, rather than the individual names of the NEMO parameters.

10. Line 140: “The WOA23 dataset is available at the NOAA website.” No need for this sentence. You can reference WOA in the earlier sentence.
We have removed this phrase. The revised text now reads: “ Our study also incorporates temperature and salinity vertical profiles provided by the latest version of the World Ocean Atlas 2023 (WOA23), which integrates data from 1955 to 2022 at a resolution of 1°(Regan et al. 2023). “

11. Around lines 195-205: These sentences seem more suitable in the discussion.
We have extensively revised the discussion section. However, we chose to keep this particular sentence in this part of the text to highlight the differences between the control run and the LOPS climatology.

12. Line 240: When you mention the Brunt-Vaisala frequency, please state to refer to results in Figure 8.
We have updated this part of the manuscript. The new text now begins the discussion of upper ocean properties with the Brunt-Vaisala frequency, as follows (lines 226-228): “Fig. 7 shows the vertical distribution of ocean physical properties in September, including stratification, salinity, and temperature. To assess the stratification strength across simulations, we calculate the Brunt-Väisälä frequency as ...”

13. Line 370: ’Surprising’ seems like a strong word. Stratification is sensitive to OSBL mixing parameterization, so it is not that surprising. Just remove the word “surprising”.
We have thoroughly revised the conclusion and discussion section, and this phrase has been removed.