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This is a useful modelling study with a finding that differences in ocean stratification due to TKE MLP parameters have significant implications for the Arctic freshwater content. For global NEMO modellers would be nice to know how these various simulations perform in other sea-ice covered seas, not just in the Arctic. Also, is there a specific configuration that is recommended and differs from default settings?

The text could be improved significantly. Its expressions are often confusing and the writing style could be clearer, in particular scientific terms and simulation names could be used in a more uniform manner, which would help reading a lot. I give some detailed comments to assist on this:

We thank the Referee for his/her detailed assessment of our manuscript and suggestions aimed at improving the clarity and relevance of our work. We agree that it would be valuable to understand how our various simulations perform in sea-ice covered regions beyond the Arctic, and it is one of the perspectives of this work as for instance evaluate them on the Southern Ocean, the Sea of Okhotsk, and the Bering Sea. However, we decided to focus on the Arctic region because there are more observational data available for validation. We have added this point to the conclusion and discussion section.

Additionally, we have revised the conclusion and discussion section to include recommendations for the TKE MLP in the regions studied here. We have integrated a summarized table detailing the performance of each simulation across the seasonal cycle and inter-annual variability of the MLD and sea ice thickness in each region. We have also provided a discussion on the limitations and potential improvements of the scaling parameter.

Hereafter, we give a short list of the implemented changes in view of the Referee's comments. Please find below a part-by-part reply.

- line 58: Instead of writing 'several regions', be specific and write '3 regions'
Included (line 69). We changed the phrase to: "... the upper ocean and sea ice properties in three regions of the Arctic Ocean ..."

-line 58: 'by varying the mixing scheme' is more clear, if that is meant here. 'varying the mixing just below the mixed layer TKE parameters' is hard to understand.
Included (line 69). We replaced it with "by varying the mixing scheme".

-line 61: Change to 'The vertical turbulent kinetic energy (TKE) closure scheme'
Done (line 72). We changed it to: "The vertical turbulent kinetic energy (TKE) closure scheme implemented in NEMO is based on the model developed by Bougeault et al. 1989 for the atmospheric boundary layer."

-line 63: Change to 'integrated by Blanke and Delecluse (1993) into the OPA model'.
Included.

-line 75: 'ocean's surface boundary layer'.

Done.

-line 84: Can f_r be > 0.1? If it can, would it make sense to try higher values?
We thank the Referee for this question. The upper limit for the f_r parameter is 0.1, as it is set in the NEMO model. Therefore, it is not possible to evaluate higher values.

-line 84: Do you mean that 5% of TKE is redistributed below MLD or in the MLD and below it?
We thank the Referee for pointing out this misleading phrasing. It means that 5% of TKE is redistributed within the MLD and below it. We have significantly revised this section of the text and removed this sentence for the sake of clarity.

-line 94: Explain what ORCA1 is.
We have included the following: "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 $1/3°$ 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).

-line 97: 'Rathore et al. (2024)' [year is missing]
We added the year to the reference.

-Table 1 and others: Table captions are usually above tables.
We have modified the position of the captions in the tables.

-line 114: Is pss Practical Salinity Scale? Does it equal psu which is more commonly used?
We thank the Referee for this question. In NEMO the salinity unit used for the equation of state is the practical salinity scale.

-line 120: 'de Boyer Montegut (2024)' [typos in the reference]
We corrected the reference to: "de Boyer Montégut C.: Mixed layer depth over the global ocean: a climatology computed with a density threshold criterion of 0.03 kg/m3 from the value at the reference depth of 5 m, https://doi.org/10.17882/98226, 2024."

-line 125: 0.03 kg/m^3 [there should be space between number and unit, change also line 127]
Done.

-Table 2: It is difficult to follow in text what simulation is what. Perhaps number simulations in this table from 1 to 9 and use those numbers consistently in text when refererring to particular simulations.
We thank the Referee for the suggestion. Following the comments of Referee 2, we have modified the 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).

-line 137: 'We computed'
We added this modification, the new phrase is (lines 149-151): "We computed vertical potential density profiles using the TEOS-10 Gibbs Sea Water toolbox (Mcdougall et al (2011), and determined the MLD by applying the threshold density criteria. This was done to compare ITP observational data with NEMO sensitivity experiments, where the surface reference depth for ITP varies from 10 to 0 m depending on the profile."

-line 140: 'The ITP data are from'
Included (line 152). The new text is the following: "The ITP data are from 2004 until 2019, with the majority of the observations between the years 2007 to 2015. "

-Figure 2 caption: 'Basins following Peralta-Ferriz and Woodgate (2015).
We have corrected this caption to: "Figure 2. Bathymetry in meters of the ORCA1 configuration, derived from the ETOPO2 dataset. Dashed color lines show the boundaries of the Makarov (in red), Eurasian (in green) and Canada (in yellow) Basins following Peralta-Ferriz and Woodgate (2015)."

-lines 155-157: Seems that latitude-longitude boxes can not be used to define the regions drawn in Figure 2.
We thank the Referee for pointing out this mistake. Effectively, the boundaries for the Eurasian Basin were incorrect. The text has been revised as follows: "Our analysis focuses on the Arctic region, specifically the Makarov, Eurasian, and Canada Basins, which are characterized by year-round sea ice coverage. These regions are defined as follows: The Makarov Basin (83.5–90°N between 50–180°W and 78–90°N between 141–180°E), the Eurasian Basin (82–90°N between 30°W–140°E and 78–82°N between 110–140°E), and the Canada Basin (72–84°N and 130–155°W)–see Fig. 2." (line 168).

-lines 157-162: This text does not belong here but fits better in Introduction.
We agree to the Referee that this part of the text fits better in the introduction. We have moved this text to the introduction in lines 60-65.

-line 172: 'deeper than modelled ML'
Here, we are discussing the LOPS climatology and its tendency to exhibit excessively deep values along the coast of Greenland. This discrepancy may be attributed to the data obtained offshore, particularly in the ocean and deep ocean regions. We are not comparing it to the modeled ML.

-lines 176-178: It is hard to see these salinity differences from Figure 4 which shows absolute salinities. A solution is to add Control-LOPS salinity difference panels to Figure 4.
We thank the Referee for this suggestion. We have added the differences between the Control-LOPS for both salinity and temperature in Figure 4, as has been done for the MLD.

-line 181: 'data are averaged spatially and temporally'
We modified the text to (line 190): "Data is averaged spatially and temporally for each basin."

-line 189: Here is a place where indicating the parameter would be a reader: 'Increasing the fraction of surface TKE (f_r) that'
We thank the Referee for this suggestion. The revised text is (lines 199-200): "Increasing the fraction of surface TKE that penetrates into the ocean f_r from 0 to 0.1 increases the MLD as well as the amplitude of the seasonal cycle."

-line 190: Do you mean 0.75 rather than 0.08?
We made a mistake in the higher value mentioned in the previous text. It should indeed be 0.1, not 0.08. The correction has been made, as shown in the previous response.

-lines 191-192: The effect of nn_tau is not shown in Figure 5, so this sentence in this context is a bit misleading.
The effect of the simulation $h_\tau = 10$ m (nn_tau) is shown in Figure 5 (in green). When the value of h_τ is changed from 30 m (in the control simulation) to 10 m in the $h_\tau = 10$ m simulation, the amplitude of the seasonal cycle decreases.

-line 194: 'could arguably be caused'
Included (line 203). The text have been modified to: "These summer biases could arguably be caused by the different reference depths used for computing the MLD with the density threshold criteria: 5 m for the LOPS climatology and 0.5 m for NEMO4.2."

-line 195: 'However, we recomputed'
It has been included as: "However, we recomputed the MLD using the 5m reference for the control run ..." (line 207).

-line 198: Is the MLD std modelled and/or regional, what are the summer months used? This information should be added.
We modified the text to: "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)." (lines 211-213).

-line 201: 'and is only 8 m'
Done (lines 214-215). The new phrase is: "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."

-line 204: 'with MDL at least 20 m'

Done (line 218). the new sentence is: "The largest ML deepening is observed for $\chi = 1$ (no attenuation of mixing due to sea ice coverage), with MDL at least 20 m thicker than the control run in both months across the studied regions. "

-line 207: '20 m' [add space between number and unit]

Done.

-line 207: 'in the Canada Basin and the Eurasian Basin in March'

We included to the text as: "Conversely, the most significant ML shallowing is observed for $f_r = 0$ (TKE MLP turned off), resulting in ML shallower than the control by 20 m, particularly in the Canada and Eurasian Basins in March." (line 221).

-line 209: The spatial distribution looks similar in September too, not only in March.

We agree to the Referee and it has been corrected. The new sentence is: "Decreasing the characteristic depth of TKE penetration h_τ from 30 m to 10 m has an impact similar to a decrease of the penetrating fraction of energy f_r, with a similar spatial distribution in March and September." (lines 221-223).

-lines 215-216: The effect of deeper ML on surface salinity is mentioned. What about temperature?

We have changed this paragraph to the following: "A decrease in the MLD with a strong stratification corresponds to a reduction in sea surface salinity and an increase in surface temperature compared to the control simulation. In contrast, an increase in MLD with a weak stratification is associated with an increase in sea surface salinity and a decrease in surface temperature. This can be attributed to the fact that a shallower mixed layer and a strong stratification result in less mixing during ice melt, leading to a fresh anomaly at the surface and trapping heat at the surface. On the other hand, a deeper mixed layer and a weak stratification allow freshwater to mix more deeply, resulting in higher surface salinity and facilitating vertical heat exchange." (lines 263-269).

-line 226: Could continue that the TKE MLP parameters do not improve the temperature maximum below 200 m because the temperature maximum is due to the heat advection at that depth.

We thank the Referee for this suggestion. We have added the following clarification: "While our control simulation demonstrates improvements compared to these models, adjusting the TKE MLP parameters does not improve the representation of the temperature maximum below 200 m, as this maximum is primarily affected by heat advection at that depth." (lines 247-249).

-line 266: Note that there is also negative sea-ice thickenss bias in March.

The negative biases of the sea ice thickness are discussed in lines 281-282 as follows: "In March, negative biases between the simulated sea ice thickness and PIOMAS are more pronounced in the region next to Greenland. "

-line 266: Is the East coast the Siberian coast?

Indeed, we have clarified this part of the text by adding: "Specifically, regions near the East coast (Chukchi, East Siberian, Laptev, Kara, and Barents Seas) display sea ice thickness close to zero during this month." (line 280)

-line 267: 'negative biases between'

Included (line 281). The new sentence is: "In March, negative biases between the simulated sea ice thickness and PIOMAS are more pronounced in the region next to Greenland. "

-line 268: Discrepancies between OSI-SAF and model are not relatively minor in September but look quite substantial. What is the OSI-SAF uncertainty? Would make sense to use other satellite sea-ice concentration data for comparison. It is known that different data sets provide somewhat different sea-ice concentration and the ice edge.

The OSI-SAF uncertainty, as indicated by Lavergne et al. (2019), is approximately 3%. In the same study, a comparison with the ESA CCI data revealed uncertainties of similar magnitude. We opted not to include this dataset in our analysis because it only covers the period from 2002 to 2017.

-lines 295-296: The link between stratification and sea-ice melt is interesting. What would be the role of ice-albedo feedback strengthening this process?

We thank the Referee for this question. In winter, the central Arctic Ocean is almost completely covered by sea ice, with concentrations reaching nearly 100%, and sea ice thickness ranging between 2 and 3 meters. During this period, the role of ice-albedo feedback in upper ocean-sea ice interactions is likely to be less significant due to the extensive ice cover. However, in summer, as sea ice melts and some regions are exposed to open water, the effect of ice-albedo feedback becomes more pronounced. As surface waters warm due to the increased absorption of solar radiation, ocean stratification increases. The warmer, less dense surface layer becomes more stable and less likely to mix with the cooler, denser water below. This enhanced stratification acts as a barrier, preventing the upward mixing of colder water which could otherwise help mitigate surface warming. Consequently, the ice-albedo feedback strengthens the link between stratification and sea-ice melt, creating a positive feedback loop that further accelerates ice loss.

-line 299: Would be reader friendly to remind what these three sensitivity experiments are, not just list their parameter values. Here Tables 1 and 2 could be used/ cited.
We have simplified the name of the experiment, and the table 2 have remove.

-Figure 11. Differences seem often not very clear and there can be biases both ways. Perhaps calculating RMS or mean absolute differences per basin would give clearer and more quantitative measure.
Figure 11 illustrates the spatial distribution of differences in sea ice concentration and thickness between the sensitivity experiments and the control run for March and September. The differences were calculated as the difference between the experiment and control for sea ice concentration. A positive difference indicates that the sea ice concentration in the control run is larger than in the experiment, while a negative value indicates the opposite. Additionally, the mean absolute differences are discussed in lines 300-310.

-Figure 12: Caption: 'MLD, sea-ice concentration and sea-ice thickness'. How much these timeseries correlate? Seems that year-to-ear variability is captured rather well due to the same atmospheric forcing and free drift in summer.
For a given experiment, there exists correlations between sea-ice concentration, MLD and sea-ice thickness. As explained in the last paragraph of section 3, correlations between MLD and sea-ice properties are explicitly mediated through the parametrization employed for the attenuation coefficient. In presence of a non-trivial attenuation coefficient, all time series tend to display similar trends. When the attenuation is turned off, though, ($\chi = 1$, purple timeseries) the MLD seems to decorrelate to the sea-ice variability.

-line 317: 'is close to'
Done (lines 331-332). We revised the phrase to "In the Canada Basin, the sea ice thickness is close to the PIOMAS reanalysis during the full period."

-line 325: What is short-term?
Thank you for the remark. Indeed, we referred to the short-term trend as the trend from 2000 to the present. We have changed the text to: "This short-term trend (from 2000 to the present) is also evident in the simulated sea ice thickness and the sea ice thickness from the PIOMAS reanalysis in all three basins." (lines 338-340).

-line 332: 'exhibit closer resemblances'
Included (lines 345-346). The new text is: "Comparing simulations with ITP observational data, the $\chi = 1 - f_i$ and control simulations exhibit closer resemblances."

-line 346: Can you link the MLD increase to the erosion of the Arctic halocline? We thank the Referee for this question. Indeed, as discussed in the previous section of the paper, there is a close link between the deepening of the ML and the Arctic halocline: a deeper ML is associated with weaker stratification, while a shallower ML exhibits a strong stratification. However, it is important to note that one is not the direct cause of the other. Including a detailed discussion on stratification in this section would have been repetitive. Therefore, we decided to omit it from this part of the paper.

-line 370: 'in these regions than the $1 - f\_i$ option'
Done (line 376).

-line 371: 'the LOPS climatology'
Done (line 377).

-line 372: 'upper ocean vertical properties'
This part of the text has been removed.

-line 398: 'their coverage'
Done (line 385).

-line 401: 'to entirely remove TKE MLP'
Done (line 388).

-line 405: 'this parameterization lacks a'
Done (line 410).

-lines 406-407: Do you have a reference for the claim of time dependence of the NEMO TKE MLP scheme? This reference to the time dependence was too technical for the paper, so we decided to remove it. However, the reference for this issue is as follows: `http://forge.ipsl.jussieu.fr/nemo/ticket/2748#no2`.