Response to comments posted 11 August 2024 by Referee #1 to "DALROMS-NWA12 v1.0, a coupled circulation-ice-biogeochemistry modelling system for the northwest Atlantic Ocean: Development and validation" (28 August 2024)

We thank the referee for taking the time to review our manuscript and for providing supportive comments. Please find below our response to your comments.

(…) Although the short simulation period of 4 years may not be useful for climate research, I suggest the authors select specific locations to demonstrate whether the models can properly simulate the evolution of key variables (monthly mean or annual mean). Numerous observed sea surface temperature data, bottom temperature data (summertime), current meter data, and other types of data are available for this region, which could be used to further investigate model performance. This would enhance the confidence of readers and future users in the model. Additionally, winter convection events in the Labrador Sea are believed to be a significant driver of circulation in the North Atlantic Ocean, impacting water mass properties in this region. It is worth reporting whether the ROMS model can well simulate these winter convection events during the study period.

We agree with you that four-year simulations are too short for climate research. The main focus of this paper, however, is on the development and validation of an advanced coupled physical-biogeological modelling system for the northwest Atlantic Ocean. In our future studies, we will run this modelling system for much longer simulation periods to examine the effects of climate change on the marine conditions over the region. This sentence was added to the last paragraph of section 5. We also examined the model performance in simulating winter convection in the new Figure 16 (discussed in pages 30 and 32–33 of the revised manuscript), which shows daily-mean temperatures simulated in the prognostic simulation, vertically interpolated to 5-m depth intervals between the 0- and 2000-m depths, at two locations in the Labrador Sea: one near the centre of the "convective region" identified by Luo et al. (2014), and the other on the AR7W transect and within the area for which Yashayaev (2024) composited available observations to construct his time series of depth profiles. Although the horizontal grid size of our model (O(km)) is too coarse to resolve convective plumes (O(100 m)) and the model does not include any parameterization of convection, the response of the model's turbulent vertical mixing scheme to the conditions that trigger convection can be expected to generate conditions similar to those resulting from convection. At the location within the “convective region”, the time series of simulated temperature profiles includes several features that appear in Yashayaev’s observation-based time series, such as the contrast between weak and strong convections in 2014 and 2015 respectively. In the future, we hope to develop a finer-resolution model focusing on the Labrador Sea which will be nested within our current model and will include the capability to simulate or parameterize convection.

Below is a list of specific comments.

- *Line 34 “up to 7”, could “up to 7 psu” be better”?*

We agree that “up to 7” sounds somewhat awkward. Modifications were made accordingly.

- *Lines 153-154. Why are the eddy viscosity and diffusivity set to zero? Given that you employ high-order accuracy schemes (3rd and 4th order), it’s unclear why these values would be zero. Could you provide a rationale for this choice?*

The reason for setting the horizontal eddy diffusivity to zero is that the third-order upstream advection scheme generates some numerical diffusion which is large enough to eliminate numerical noise in tracers. As we discuss in lines 525–535 (page 19) of the revised manuscript, this numerical diffusion associated with the advection scheme already appears to cause excessive mixing in areas such as the St. Lawrence Estuary-Gulf system. We set the horizontal eddy viscosity to zero in order to preserve as much as possible of the simulated eddy activity and because we have not noticed any numerical noise in the simulated circulation field that needs to be smoothed out with a non-zero horizontal eddy viscosity. Text was added on Page 7 accordingly.

- *Lines 172-174. Are the inter-annually variable GLORYS12 data used at open boundaries or the daily climatological ones? Please specify.*

It is the former. We use daily GLORYS values with interannual variability for the simulation period and not climatologies. Text was changed to “derived from the daily fields of Copernicus global 1/12° oceanic and sea ice reanalysis (GLORYS12V1, hereafter GLORYS; Lellouche et al., 2021) for the simulation period” in lines 245–246 (page 8) of the revised manuscript.

- *Lines 182-183. I am curious why it is through the bottom. River discharge is through top layers.*

In previous versions of ROMS, riverine freshwater input was specified as horizontal flow into the river mouth grid cell from a neighbouring dry grid cell. When the riverine input is specified through the bottom of a wet grid cell, the river scheme has several advantages including: 1) the model is more stable because there is no longer an addition of horizontal momentum and 2) the user no longer needs to worry about specifying the grid cell from which the fresh water enters the river head, which can be tedious when the domain includes a large number of rivers flowing in different directions. Details of this method are described by its developers in this ROMS user forum post: https://www.myroms.org/forum/viewtopic.php?t=5156. This link was added to line 254 on page 8.

- *Lines 298-299. Glorys12 uses data assimilation (4d var), and in theory, this can eliminate those biases from the non-inclusion of tides.*

We agree with you that, in theory, the use of data-assimilation in generating GLORY12 can eliminate biases associated with non-inclusion of tides. But in reality observations used in data assimilation are very sparse in time and space. Therefore it is highly unrealistic to expect GLORYS to be an exact reproduction of real ocean conditions at all locations. In areas and during periods for which the number of observations are relatively small, exclusion of tides can cause GLORYS values in the subsurface zone of relatively shallow waters to deviate from true solutions. Text was added accordingly in lines 378–382 (page 15) of the revised manuscript.

- Lines 310-311. Same as above. You may want to find some literature to support this statement.

Here we are merely speculating, based on the fact that error metrics tend to be large: 1) near the model's lateral open boundaries, 2) during the summer when temperatures are not constrained by sea ice, and 3) at the surface, where the metrics are calculated with respect to an independent dataset, that the errors in GLORYS (which exist even after the assimilation of available observations) might be propagating into the model domain via lateral open boundary inputs.

- Lines 369-378. This seems an indication of numerical scheme issue or the horizontal mixing issue (zero is used).

The third-order upstream and fourth-order Akima horizontal tracer advection schemes that we tested have their advantages and disadvantages. The third-order scheme is much less prone than fourth-order schemes to numerical under- and over-shootings, but it generates relatively large numerical diffusion (even with zero eddy diffusivity) that can result in excessively smooth simulated tracer fields. The fourth-order scheme performed better at reproducing the general three-dimensional structures of temperature and salinity in areas such as the St. Lawrence Estuary, which in turn led to more realistic simulations of sea ice. This scheme, however, is prone to numerical under- and over-shootings that produced patches of unrealistic tracer values, which is why we decided to use the third-order scheme. The topic of numerical schemes, including the possible development of a flux limiter for a fourth-order advection scheme, is something we hope to revisit in the future. Text in lines 448–458 (page 19) of the revised manuscript was modified accordingly.

- *Lines 409-425. Data from Drinkwater (1988) were for the year of 1982, and the data your model and GLORYS12 are for the recent years. You need to mention the probable existence of decadal or even longer variability for the current in this area.*

We agree. We added “Although we need to keep in mind the existence of interannual variability and long-term trends which limit the conclusions we can derive” to the beginning of the sentence in which we point to the possibility that the inclusion of tides in our model results in a more realistic vertical structure of currents (lines 522–524, page 23 of revised manuscript).

- *Figure 13. I cannot see any dots there. Could the quality of the figure need improving?*

The “dots” referred to the straight black line at the sea surface. We have revised the caption to clarify what the lines represent.