Response to comments by referees to "DALROMS-NWA12 v1.0, a coupled circulation-ice-biogeochemistry modelling system for the northwest Atlantic Ocean: Development and validation" (2 October 2024)

We thank the referees for taking the time to review our manuscript and for providing supportive comments. Please find below our response to the referees, which is based on the author comments we posted to the interactive public discussion. All line numbers and page numbers in our response are from the marked-up version of the manuscript that we are submitting with this response.

<u>**Response to Referee #1**</u>

> *(…) 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 that four-year simulations are too short for climate research. The main focus of this paper, however, is 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 **(lines 954–958**, **page 47**). We also examined the model performance in simulating winter convection in the new Figure 16 on page 34 (discussed in the new subsection 3.5 on **lines 589–648**, **pages 32–33**), 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(1 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.

- *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 throughout the manuscript.

- *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 405–415** (**page 21**), 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. Description of the horizontal eddy diffusivity and viscosity was modified to include these points (**lines 156–159**, **page 7**).

- *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 187–189** (**page 8**).

- *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 198** (**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 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 is relatively small, exclusion of tides can cause GLORYS values in the subsurface zone of relatively shallow waters to deviate from true solutions. This discussion was added to the text (**lines 318–322**, **page 13**).

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

Here (**lines 351–356**, **page 18**) 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 405–415** (**page 21**) 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 478–480**, **page 25**).

- *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 (**page 31**).

<u>Response to Referee #2</u>

- *Lines 55-56: Is deep convection an additional or dominant component of CO_2 removal in the Labrador Sea, as Tian et al. (2004) discussed? Clarifying this point would strengthen the manuscript.*

In response to this comment and a comment from Referee #1, we added a new subsection (3.5, **lines 589–674**, **pages 32–34**) in which we assess the model performance in simulating deep convection through its turbulent vertical mixing scheme. In addition, Figure 1 (**page 7**) was revised to show the locations of the depth profiles shown in Figure 16 and the new subsection is mentioned at the end of section 1 when the structure of the manuscript is described (**lines 119–121**, **page 5**). We have not yet looked at profiles of simulated biogeochemical fields in the convective region, but in the last sentence of section 3.5 we state that this is part of our future research plans (**lines 645–648**, **page 33**).

- *Line 104: The resolution of "O(km)" is mentioned. Currently, eddy-resolving circulation models of the North Atlantic typically have resolutions between 1 and 10 km. A more specific definition of the model resolution would be helpful for readers.*

Since the horizontal grid size of our model ranges from ~8 km in the south to ~2 km in the north, we changed "a horizontal grid size of O(km) that decreases with latitude" to "a horizontal grid size of O(1 km) that decreases with latitude" accordingly (**line 105**, **page 5**).

- *Line 119: Is the freshwater flux separated into solid and liquid components? If not, could the authors provide a rationale for this choice?*

The freshwater fluxes described in this paragraph (**lines 197–223 and 225–229**, **pages 8–10**), from rivers and from the melting of ice and snow on land, are in liquid form. In response to your comment, we added the following sentences after the description of the freshwater inputs: "Another source of salt/freshwater flux at the sea surface is sea ice, which is a source of salt through brine rejection at the time of freezing and a source of fresh water at the time of melting. Lateral movement of sea ice results in these two surface fluxes occurring at different locations." (**lines 226–229**, **page 10**)

- *Lines 153-154: The model diffusion/viscosity is zero. What boundary condition is used for the tangential velocity component, and does this formulation accurately represent the friction in the lateral boundary Ekman layer?*

The adaptive radiation-nudging open boundary condition is used for both the normal and tangential components of depth-varying currents. For depth-averaged currents, the Shchepetkin scheme which we use is applied to the normal component, and specifying this scheme automatically results in the Chapman scheme being used for the tangential component. We added these points to the text (**lines 181–184**, **page 8**). We nudge the simulated currents, temperature, and salinity towards GLORYS reanalysis near lateral open boundaries to ensure the ocean state in this area is as realistic as possible, which might be difficult with just the lateral open boundary conditions (**lines 189–194**, **page 8**).

- *Lines 348-350: The salinity model error may be related to boundary conditions in this region of the model domain. Do the GLORYS simulations provide adequate boundary conditions? The higher horizontal resolution of your model suggests that the GLORYS model might underestimate horizontal advective transport through the open boundary of the St. Lawrence Estuary.*

The head of the St. Lawrence Estuary is not an open boundary in our model. Instead, there is an artificial channel representing the St. Lawrence River, at the head of which we specify the river discharge (**lines 203–205**, **page 8**). This discharge value is estimated by the St. Lawrence Global Observatory using the regression model of Bourgault and Koutitonsky (1999) whose input is the observed water level of the river, so we expect this dataset to be reliable.

- *Lines 550-556: The impact of tides on temperature and salinity in the Bay of Fundy appears minimal (Fig. 19). Could the authors discuss why the tidal effect is relatively small in this area? I would expect them to be more significant.*

Our model results do demonstrate large effects of tides on the sub-tidal circulation and hydrography in the Bay of Fundy, which were not mentioned in the previous version of our manuscript. Description of these effects now appear in **lines 731–742** (**page 36**).