Response to Comments by Reviewer 1
(Reviews are included in black font; Responses are in blue font)

The authors present a very nice study on water exchange between different subregions of the North Atlantic continental shelf. The chosen method is appropriate, the article is well written, the introduction into the topic is broad, and the conclusions are supported by the results.

Response: Thank you for the kind words and for the thorough and helpful review. Below we describe in detail how we intend to address your comments in the revised manuscript.

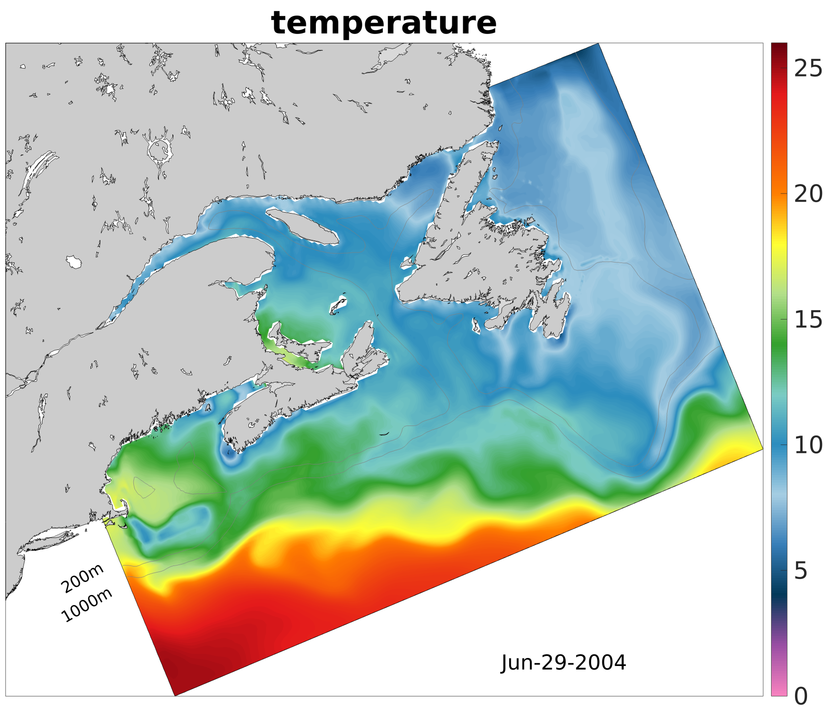
There are, however, three major points I would like to see improved:

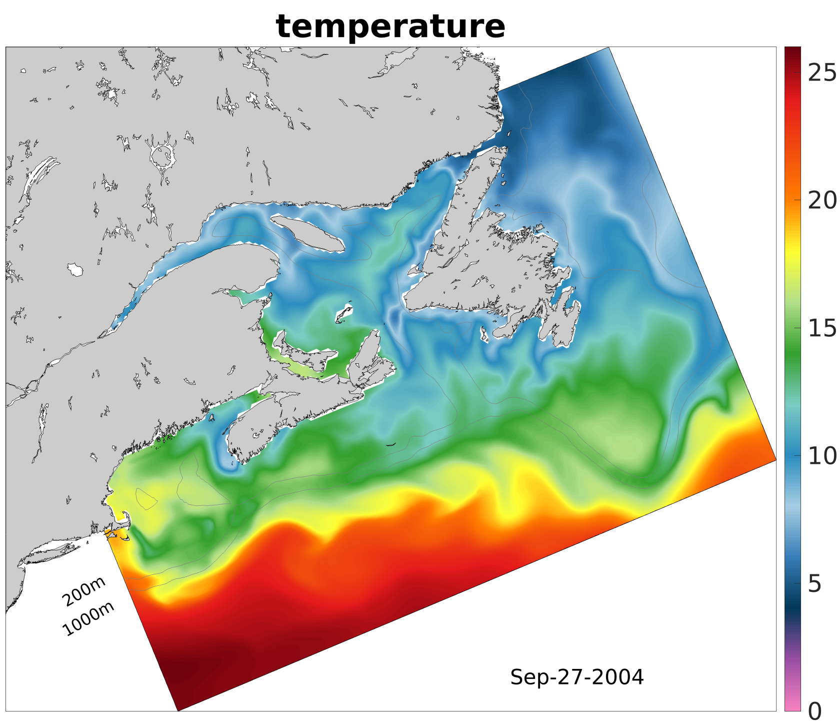
(1) As reviewers always do, I request some model validation. You refer to a different article describing the model setup, but that's not sufficient. You need to show, preferably in the online supplement, that your model is able to capture the hydrographic features of the system and its subbasins. What I would expect are vertical profiles of salinity or density which illustrate whether or not the stratification (as consequence of mixing and estuarine circulation) in the different subbasins is simulated appropriately. Also, a cross-shore transect of salinity from coast to offshore would be very helpful.

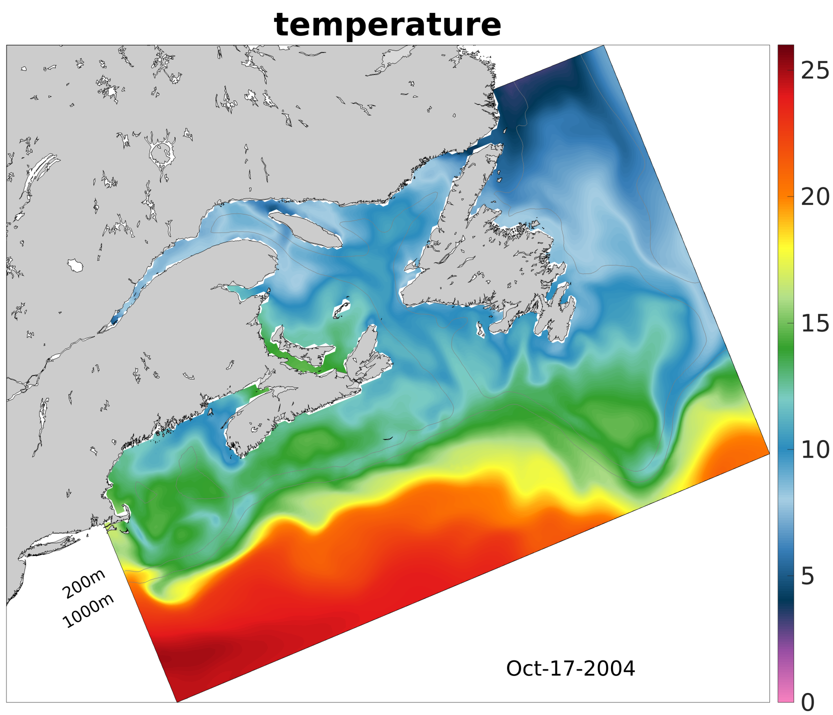
Response: Agreed. We would like to add transects showing model-data comparisons of temperature and salinity across the Scotian Shelf. More specifically, we will add comparisons of the model to the two Atlantic Zone Monitoring Program (AZMP) transects available for the Scotian Shelf (the Halifax Line and the Louisbourg Line) and glider transects, both of which encompass the cross-shelf T&S gradient and the T&S signals in the deeper shelf basins. We will additionally include a time series of Scotian Shelf area-averaged temperature and salinity at the sea surface in comparison to a climatology. These plots will be added to the supplementary information with a summary of a few other key points from the Brennan et al. (2016) paper that provides a detailed validation of our physical model, especially how our model set-up reproduces the volume transport in the region as compared to Loder et al.'s (1998) estimated mean annual transport.

(2) One of the main points of your study is emphasizing the weak exchange between coastal and offshore waters. Now a lateral mixing of water masses typically takes place by mesoscale and submesoscale eddies. As your model is not eddy-resolving, I would like to either (a) see a justification whether your model is still able to get the horizontal exchange right, e.g. by comparing mesoscale features like eddies separating from the surface current to satellite observations, or (b) read a paragraph discussing the model limitations stating this as a possible source of error.

Response: With a horizontal resolution of 10 km, we would argue that our model is eddy-resolving, although it certainly is not resolving sub-mesoscale variability. Shown below are selected snapshots of the model-simulated surface temperature, which illustrate clearly that a range of mesoscale circulation features is produced, such as tendrils of cool water off the Newfoundland and Nova Scotia coasts, and meanders in the Gulf Stream.







(3) There are two ways in which the dye concentrations can be interpreted, and you are mixing them up.

(a) You can use them to indicate the origin of the water, that is, the first region the water parcel resided in. In this case, every water parcel has just one color. That means that you just have to add dye to the "uncolored" water, coming e.g. from rivers or precipitation. In practice it means in the "yellow" area, you increase the concentration of yellow dye until the sum of all dye concentrations is equal to 1 kg/m3.

Response: In our experiments at model initialization, every water parcel has just one color equal to 1 kg/m3. We use the term "water parcel" here to mean the volume of a single grid box, which is the smallest volume we can practically consider. As the simulation progresses, water from different source regions mixes, and thus each grid cell can have a mixture of different dyes. We are not sure why the Reviewer states that "every water parcel has just one color" because clearly many grid cells have water from multiple source regions and thus different dyes. Perhaps the Reviewer is thinking of Lagrangian particles? Those would be different.

(b) You can use them to indicate all areas the water parcel has travelled through. In this case you increase the yellow dye concentration in the yellow area always to 1 kg/m3, irrespective of the other dyes. The water can be both red and yellow then, indicating it has previously been to two areas.

Response: Agree, this is exactly what is happening in our AGE simulations.

Interpreting "mass fractions" means you normalize the tracer concentration in such way that the sum of all dyes (maybe except the local one) is equal to one. But this makes sense only in case (a), not in case (b). A simple example can illustrate it:

Think of a single straight river with three areas: upstream=blue, mid-stream=yellow, downstream=red. All of the water arriving downstream originates from upstream and has passed mid-stream, so it has 1 kg/m3 blue and 1 kg/m3 yellow dye in it. Calculating mass fractions, like you do, means we find 50% blue and 50% yellow dye. But what do these 50% tell us? In fact, they are meaningless. In reality, 100% of the water came from upstream, and also 100% of the water came from mid-stream, this is no contradiction.

Response: We can't entirely follow the Reviewer's logic here. Below we apply to Reviewer's thought experiment to our two different types of simulations.

In the TRANS simulation (used to calculated residence times): In a **purely advective** river, where the three different water types do not mix, and the dyes are initialized as indicated by the Reviewer, the water arriving at the mouth would be either only red (initially), only yellow (once the mid-stream water has propagated to the river mouth), or only blue (once all the mid-stream water has propagated through and the upstream water is arriving). All the concentrations would be 1 kg/m^3. **But** if the system has **advection and mixing**, then water parcels will have mixtures of the different dyes and the dye concentrations will be equal to or smaller then 1 kg/m^3.

In the AGE simulation (used to get mass fractions and ages): In the **purely advective** case, initially only red dye would arrive at the river mouth, later both red dye (at 1 kg/m^3) and yellow dye (at 1 kg/m^3) would arrive simultaneously, and finally red dye (at 1 kg/m^3), yellow dye (at 1 kg/m^3) and blue dye (1 kg/m^3) would all arrive simultaneously. **But** in the case with **advection and mixing**, initially only red dye would arrive, later red dye (at 1 kg/m^3) and yellow dye (at < 1 kg/m^3) would arrive simultaneously and finally red dye (at 1 kg/m^3), yellow dye (at < 1 kg/m^3) and blue dye (< 1 kg/m^3) would all arrive simultaneously.

The only case where the water can have "1 kg/m^3 blue and 1 kg/m^3 yellow dye" at the river mouth is the purely advective AGE case. If there is mixing, this cannot occur in either the AGE or the TRANS cases. And our model does include mixing. In our opinion it is meaningful to report the dye mass fractions.

We agree with the Reviewer on his/her statement: "In reality, 100% of the water came from upstream, and also 100% of the water came from mid-stream, this is no contradiction."

If you, of course, only compare the mass fractions against each other, in the sense that one region contributed more than the other (or, in our example, both regions contributed equally), that's correct, but it does not require normalization. I suggest that you leave out normalization in Fig, 7 and 8 and Table 1.

Response: This is indeed why we report mass fractions – we want to illustrate which region contributed more and which contributed less. We prefer to use the normalization, because this yields more manageable ranges in the numbers we report.

Apart from these three points, I really like the article and have just a few minor comments:

Page 2, line 34-35: Could you write a few more words about the TTD approach, so a reader not familiar with it can have an idea on how it works?

Response: We would like to updates this sentence to read: "TTD is best used for steady flow applications and computes the full spectrum or distribution of transit times in a water parcel using Green's functions (Haine and Hall 2002), while CART is better suited to time-varying flow and is especially useful for highly resolved coastal applications."

Page 3, line 10: "it is necessary to describe residence time as a distribution" -> "it is necessary to describe residence time in a finite volume as a distribution"

Response: Agree, will be changed as suggested.

Figure 1: "the main panel" -> "the upper panel"?

Response: Agree, will be changed as suggested.

Figure 2: "2 depth levels (200 m and above, and below 200 m)" -> "2 depth levels (above and below 200 m)"

Response: We defined the first depth level to include 200 m (i.e. >=200 m) whereas the deeper depth level is below 200m (<200 m), so would prefer to keep it as is.

Page 5, line 3: "an age tracer" -> in Deleersnijder et al., this is called "age concentration tracer" to indicate that it does not store the age, but rather the product between age and concentration. I would suggest using this wording throughout the manuscript to avoid confusion.

Response: Will be changed as suggested.

Page 5, line 4-5: "the time since the associated dye tracer has left its initialization region" -> add "for the last time" (it may have left it before and then returned to it)

Response: Agree and would like to change to: "the time since the associated dye tracer has **last** left its initialization region"

Page 6, line 16: C(tau,x) should be C(t,x)

Response: Thank you for noticing this typo. Will be changed as suggested.

Page 8, line 12: "was found to perform better than the MPDATA advection scheme" -> I do not think this comparison is required, but if you want to compare, please state how you found out which one performs better.

Response: Comparison will be removed.

Page 8, line 13-19: Please explain this a bit more clearly - you use both the Urrego- Blanco and Sheng 2012 model and the Geshelin et al. climatology as boundary conditions?

Response: Urrego-Blanco and Sheng's 2012 model is used as boundary conditions, the Geshelin climatology is used in the climatology file for weak nudging inside the domain, but not at the boundary. This will be clarified in the manuscript text.

Page 9, line 31: "consistent with previous estimates" -> please add references already here

Response: Agreed, we will add reference to Shan et al. (2016), Sutcliffe et al. (1976), and Smith et al. (1989) here.

Figure 4: The values seem rather low. For example, the initial value at SLP-S should be 200 kg/m2, after six months it has reduced to below 6 kg/m2? Also they do not seem to match the values shown in Figure 5, if you integrate the concentrations given there vertically. Also, dashed lines indicating the source region boundaries would be nice.

Response: Thank you for catching this mistake. There was an error in the code for plotting that will be updated. We will also implement your suggestion to include the source region boundaries in both Figure 4 and Figure 9.

Page 10, line 1: "Sotian Shelf"

Response: Thank you for catching this typo. Will be corrected.

Page 10, line 1: Please note that the river water discharged during the simulation is uncolored, so your interpretation requires entrainment of dye into the river plume.

Response: We will add a sentence indicating that we assume dye is entrained in the river plume.

Page 10, line 1-2: "This timing indicates that the seasonal increase in dye mass leaving the Gulf is driven by increased river discharge into the Gulf." -> I am aware that you did not give the following simplifying explanation, but a non-oceanographer could easily misinterpret your sentence: It is certainly not the volume of river discharge pushing the dye out - it is too small for that. I would rather suppose that the river discharge enhances the estuarine circulation leading to a better exchange with the open sea. However, other factors like wind might have a seasonality as well, so the attribution to the river discharge is not straightforward.

Response: We suggest changing the wording from "dye mass leaving the Gulf is driven by increased river discharge…" to "…dye mass leaving the Gulf is enhanced by increased river discharge…"

Section 4.4: Please give the day when ages are evaluated in the main text, not just in the figure caption. How do the mean ages you found relate to the length of your simulation - did they already reach a dynamic steady state or will they increase if you simulate longer?

Response:

In response to part 1 of this comment: The in-text ages are from dynamic steady state and are therefore averaged over the steady-state period (the period of dynamic steady state varies between each individual dye). An explanation will be added to the text.

In response to part 2 of this comment: Ages have already reached a dynamic steady state, illustrated by our supplementary information. We previously ran our model for 9 years to ensure the mean ages reported would not increase with a longer simulation. A couple of the dye mean ages might increase slightly with increased simulation length but the supplemental figures show that most of the dyes have already reached a steady cycle with minimal increase expected.

Table 1: Stations should show up in a map, e.g. in a slightly larger version of Fig. 2.

Response: We will update Figure 2 to include the station locations.

Page 17, line 15: What different assumptions did they make compared to your study? Or is it just the resolution?

Response: The calculation itself as well as the objectives in Sharples et al. (2017) study are very different from ours. They focus on calculating export of river plume water to the open ocean without the help of numerical models based only on a few simple assumptions. One step in their calculation is the estimation of residence times. To accomplish this goal, they have divided the global shelves into two different cases using the S_p ratio (i.e. the ratio of the plume width to the shelf width). If $S_p > 1$, the low salinity plume is assumed to reach beyond the shelf edge into the open ocean and therefore the residence time is assumed to be governed by cross-shelf transport mechanisms within the buoyant plume. If $S_p < 1$, the low salinity plume is assumed to be confined to the shelf within the buoyancy current. River plume water is assumed to gradually mix with shelf water and therefore exchange with the open ocean should be controlled by transport across the shelf break. The Scotian Shelf falls into this latter category with $S_p < 1$. The residence time is therefore calculated as a function of the mean shelf depth, the shelf width and the transports that are assumed to dominate in this scenario ($S_p < 1$), which are cross-shelf break Ekman transport and a lumped transport that estimates the nonwind-driven exchange.

We will add in a short description explaining this.