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# *Interactive comment on* "Diagnosing transit times on the northwestern North Atlantic continental shelf" *by* Krysten Rutherford and Katja Fennel

**Anonymous Referee #1**

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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.

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

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(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



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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.

(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.

(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. (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.

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.

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.

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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?

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"

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

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

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.

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)

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

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.

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?

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

Page 10, line 1: "Sotian Shelf"

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.

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.

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?

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

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

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