DETAILED RESPONSES TO REVIEWER #2

For completeness, we suggest reviewer #2 to read our answers to reviewer #1, since some of the remarks are overlapping.

In the manuscript "North Atlantic Subtropical Mode Water properties: Intrinsic and atmospherically-forced interannual variability" the authors investigate the contribution of the two different types of variability on the total interannual variability of the eighteen degree water (EDW) using a 50-member ocean/sea-ice ensemble simulation with a horizontal resolution of ¼°. They validate the model results against a gridded product based on observations. The authors use a combination of potential vorticity, density, and latitude/longitude criteria to define EDW which differ for the model output and observation-based product to account for differences in the datasets. The authors define the ensemble mean as the atmospheric-forced variability and the ocean's chaotic intrinsic variability as differences of each member from the ensemble mean. Six properties of the EDW are investigated and the authors find that between 10-44% of the interannual variability can be explained by the ocean's intrinsic variability depending on the property, with 44% found in temperature.

This paper is interesting and well written. However, I have questions regarding the analysis which I detail below. I hence recommend major revision for the manuscript.

General comments:

The authors compare their model results to one gridded data product compared to observations. I cannot see in the presented evidence that the modelled and observation-based results agree as well as the authors claim (e.g. L5-7, L210, L243, L276-278). Fig 1 and 2 simulated and observed sections at 65°W water masses with low potential vorticity occur shallower and warmer to observations and simulated EDW seems to deepen and densify toward the east which is not visible in observations. Fig 3-5 show that the simulations seem to clearly overestimate the interannual variability compared to the observation-based product.

Thanks for this remark, which underlines the need to clarify our statements, and revise the text on a few points: please refer to our answers to reviewer #1's question 2, 2-1, and 2-2 ("We have moderated several statements about the "good" (replaced by e.g. "relatively good" or "correct") model-ARMOR3D agreement at several places in the paper"). We also recall that our study is focused on the (forced and intrinsic) interannual variabilities of volume-averaged properties of the EDW, rather than on the details of its structure.

Figures (now labeled) 2 and 3 indeed illustrate some differences between EDW properties in ARMOR3D and NEMO, as occurs in all free-running numerical simulations. Regarding time-averaged volume-integrated EDW properties:

- We do agree with the reviewer that the model EDW pool is shallower than in ARMOR3D: this is mentioned in section 2.3, and we relate it to a fresh bias.
- At the scale of the averaged EDW pool that we consider in this analysis, however, there is no persistent temperature bias: the numbers shown in the top right panel of Figure 4 indicate that on spatio-temporal and ensemble average, the simulated temperature of EDW (17.9°C) is close to that in nature and in ARMOR3D. Figs 2 and 3 only give a partial view of the 50-member time-varying 3D structure of the simulated EDW, the details of which are not crucial for the study.
- It is unlikely that the model overestimates the real ocean's interannual variability: NEMO at 1/4° mostly tends to underestimate it, as been reported e.g. for sea-level (Penduff et al, 2010) or AMOC at 26°N (Leroux et al, 2018). In contrast, it has been reported by Guinehut et al (2012) that ARMOR3D underestimates the observed interannual variability; please also see our detailed answer to reviewer #1's question #2. An underestimated ARMOR3D variability is thus much more likely than an overestimated simulated interannual variability.

Second, I am concerned about the short-comings of the observational-based product which is known – as the authors state – "to substantially underestimate the actual interannual ocean variability" (L218). I appreciated the authors reasoning to use an observation-based product not depending on an underlying model, however given the short-comings of the used product with respect to its interannual variability, which is the time-scale of interest in this manuscript, I would highly recommend to include a few ocean reanalysis products like ECMWF ORAS5, CMCC C-GLORS or GLORYS2V4 to enable a more robust model validation.

We thank the reviewer for this suggestion. However, as detailed in our answer to reviewer #1's question #2, GLORYS12 and other analyses are unlikely to provide a more realistic 4D multivariate evolution of the EDW than ARMOR3D, and the differences between the reanalyses themselves would complicate a lot the model assessment (which is also not the main goal of the study).

The authors mention the arbitrary definition of EDW and I think their approach to based it on criteria of three different properties (potential vorticity, density and region) is good. However, it would be great if the authors could provide more information about why they choose the criteria as they are. The criteria differ notably for their simulation and the observation-based product and based of Fig. 1 and 2 it is not clear to me, why for the simulations the density range (1.2 kg/m^3) is so large compared to the observation-based product (B: 0.72 kg/m^3).

As in other mode water studies (e.g. Forget et al 2011), the PV upper bound is our most important criterion: it must (and does) fit the features of EDW PV in both datasets. The other two criteria (horizontal and density limits) were chosen in each dataset to exclude weakly stratified waters that do not belong to EDW: [1] other mode waters (e.g. SPMW and Madeira mode water) that are found away from EDW, [2] mixed layer waters sitting above the seasonal thermocline, and [3] deep waters sitting below the main thermocline. This discussion has been summarized in section 2.4.

In consequence, the upper and lower density surfaces are located in the seasonal and permanent thermoclines in both datasets, whose densities are different. The subsequent density range turns out to be larger in the model, consistently with the more stratified character of the model EDW compared with its ARMOR3D counterpart.

In the abstract and throughout the manuscript the authors mentioned the good agreement between simulation and observation-based product for the mean EDW volume. However, they choose their criteria for observation-based EDW to match the ensemble EDW volume mean (L178-180, L210-211), so it is designed to match.

This latter statement is mostly true: we tested various criteria with plausible ranges of uncertainties based on the physical considerations presented above, and on EDW characteristics in both datasets. From these tests (whose results are illustrated for options A, B and C), we adopted the sets of criteria that gave similar time-mean volumes in both datasets: it seems to us that this is a good choice to capture the same oceanographic feature in two different datasets. However, this particular choice turned out to also give almost identical interannual volume variances in both datasets, which is a distinct result: we think this adds some robustness to this choice.

L178-180 (previous numbering) just mentions that option B gives the same volumes so has not been modified. But we agree that L210-211 deserved to be shortened, as follows:

"In other words, the model remarkably simulates the interannual STD of the EDW volume in ARMOR3D in an ensemble averaged sense."

As the mean EDW properties seems to depend on the choice density range and max PV, it would be good to show and discuss this dependency for model and observation-based product for a fairer comparison.

EDW properties depend on the criteria used to define the water body. This is why it has been so difficult over the years to reconcile miscellaneous estimates of EDW formation or destruction rates with storage volumes (e.g. Marshall et al, 2009). In targeting this issue, Forget et al (2011) assessed the impact of the criteria used on the EDW seasonal cycle estimates. They concluded that the stricter the criteria (e.g. smaller max PV threshold), the smaller the volume, and more importantly, the larger the seasonal volume change (down to a reasonable value to define the EDW, after which no water parcels match the over-strict criteria). We build on these results and do not consider that it would bring something crucial to the paper to discuss further the impact of the EDW definition on its properties. The goal of the paper is to provide the first estimate of the EDW CIV in a simulation ensemble where the EDW has been identified as a low PV reservoir on the Equatorward flank of the Gulf Stream with fairly realistic properties.

The authors stating in the abstract and throughout the manuscript that the simulations are in good agreement with the observation-based product in terms of location, seasonality, mean temperature and volume. However, **section 2.3 is to brief and from my understanding does not provide the evidence for their statements**. In lon-depth space the simulated EDW is clearly shallower compared to the observation-based product. **No maps of the spatial (lat/lon) distribution of EDW in simulations and in the observation-based product** are shown. It would be also great to show the **spatial distribution (lat/lon) of temperature**, because from Fig. 1 and 2 it looks like it **varies with longitude in the simulations and is not constant**. **How does the spatial variance of temperature compare between model and observation-based product? How does this impact the temporal variability of the spatially averaged EDW temperature? A section/figures about the seasonal cycle for the different properties is missing.**

As discussed above in our answer to Reviewer #2's first comment, we have clarified in the paper that the model solution exhibits some local differences with ARMOR3D (which itself differs from the real ocean, see our answers to Reviewer #1's item 2), and a few differences on certain STMW-integrated quantities : we have thus moderated several statements about the "good" (replaced by e.g. "relatively good" or "correct") model-ARMOR3D agreement at several places in the paper.

In fact, our intent in Figs 2 and 3 is to illustrate the typical model skills (persistent STMW, with a correct seasonal cycle) and biases (larger PV of STMW, shallow/fresh bias), in a simple and honest way. Vertical sections make the main process controlling the STMW variability (its seasonal cycle) visible in both datasets ; latitude/longitude maps would not provide a significantly more detailed view. More generally, the local structure of STMW fluctuates in 4D (time and space) in ARMOR3D, and in 5D in the model thanks to the ensemble dimension : a detailed comparison of the multi-dimensional fluctuations of the water mass structure lies beyond the scope of the present study. Our focus is in fact on the low-frequency, forced and intrinsic fluctuations of the SPMW's integrated properties, rather than on its detailed local structure. We have slightly modified the first sentence of section 2.6 to clarify this focus.

Figures

I would suggest to add <u>a,b,c labels to any figure</u>, as they all consist of several subpanels and it would make referencing easier.

This has been done.

Figure 1 and 2: As sensitivity B was chosen for the comparison it would be better to show the B limits for the observation-based product instead of the A-limits.

This has been done

Figure 4 and 5: The RMSE contours and numbers are too fade to be readable. Also the correlation labels of the right hand side panels overlap so that they are not readable. Please adjust this. Think about to change either the red or green to a different color as these are not colorblind friendly in one plot.

The comments of the reviewer have been taken into account and the colours, contours and labels have been modified to be more readable. Taking into account Reviwer 1's comments, Figure 5 has been replaced with histograms of correlations instead of Taylor diagrams.

Minor:

L125: Add reference for Ertel PV definition

Added.

Throughout section 2.5: Units displayed as cursive inconsistent with other text.

We have realized that we had displayed several units as cursive throughout the submitted manuscript, sorry for this. In order to fit Ocean Science requirements, all units are now displayed correctly.